Discussion Paper 'Distributed acoustic sensing as a tool for exploration and monitoring: a proof-of-concept', by Nicola Piana Agostinetti and co-workers.

Response to Reviewer #2

Reviewer #2

This paper is a relatively brief focused discussion on methods for how a DAS array can be used to 1) map subsurface heterogeneity ("exploration") and 2) detect and locate seismic events ("monitoring"). The methods are validated and compared with geophone ("node") data co-located with the array in the publicly available PoroTomo data set. The paper achieves its goals and is a useful contribution to understanding how to best to use DAS data. The paper should be particularly valuable to anyone planning a surface DAS array for one or both of the targeted applications. While I recommend publication, I do have a list of editorial comments, suggestions, and questions.

The most significant one is for readability and it is to consider combining the methods and results for the two categories of results ("exploration" and "monitoring"). I do also prefer that "exploration" be called "subsurface site effects" or "subsurface heterogeneity mapping" and the "monitoring" be called "seismic event and location detection". These titles are clumsy but they don't overpromise. After all, the basic result in the "exploration" category is relative site effect time delays of +/- 0.12 seconds.
As discussed in the response letter to Reviewer #1's comments, we think that we have to clarify the manuscript goals and how we reach them. We think that "exploration" and "monitoring" should be more appropriate terms, but we also know that such terms have been used for deeper targets ("exploration") and wider workflows ("monitoring") than what we did present here. We will try to clarify goals and terminology as much as possible in the revised manuscript.

Therefore, I might also suggest a more specific title, e.g., Methods for subsurface mapping and event detection using Distributed Acoustic Sensing: Examples from Brady Hot Springs PoroTomo Experiment
We agree with the suggested title and we will change it to make the manuscript more appealing

Specific Comments, Suggestions, Questions.
l. 1. "PoroTomo" is short for "Poroelastic Tomography" so there is no reason to have the "TOMO" all caps.

l. 36 - Marra et al. is not DAS and is not relevant to this paper.

l. 103-104 - No node instrument response corrections were made. Is that relevant?

l. 115-117 – Given over 100 events in the Li and Zhan catalogue, maybe give a liitle more reason for choosing the 14 March 10:41 UTC event. Is it the closest to the array?
Yes, that event was selected since, according to a preliminary inspection of arrival times, it appeared to be located in close proximity of the deployment, and it exhibited good visibility in

both waveforms and cross-correlations (see Supplementary Materials in Li & Zhan, 2018). Anyhow, in the revised MS we will present results for more events in the revised manuscript.

l. 118-150 – These paragraphs from “Methods” would most seamlessly be followed by the “Location” results (see l. 219).l. 124-125- Is the meaning of “20-channel step” that channels 1-11, 31-41, 61-71, etc. are stacked if Channels 1-100 are a segment?

l. 179-180 Be explicit that “strictly compared” means that nodal horizontal components need to be vector summed in DAS direction.

L.207-209. Can you suggest a reason why the criterion for removing channels needs to be increased to 2 gage lengths from a corner?
There’s no particular reason behind this choice. It represented a conservative approach toward channel selection.

l. 212-216 – Can you relate this result to local site effects presented in Parker et al. or Zeng et al.?

l. 219 – Note how out-of-context this statement is because its antecedent goes back to l.

118-135. It is best to discuss this result right after l. 135. Even though the paper is short, I don’t think the reader can be expected to keep this in mind. Consider keeping the “data and methods” and their corresponding results together.

l. 225 – Similarly to above, EDT was referred to back in “methods” section. Also, “Conversely” is a term of logic. The meaning here is “In comparison”.

l. 233-234 - “projection” implies vector, not tensor, projection. cos^2 is stronger than cos.

l. 250 - “PWF” Plane-wave fitting is never defined.

l. 270 – Don’t understand “(2) with a relatively poor areal coverage with respect to a standard seismic network” when DAS arrays up to 50-km in length can be deployed.
Covering an area with a continuous fiber cable is much more labour-intensive than covering the same area with a "large N nodes array". Here the area spanned by the DAS cable is smaller than the area covered by the nodes. Conversely, maintaining the cable could be much cheaper. We will clarify these concepts.

l. 271-272 - The statement about "old style" neglects recent deployment of large N node (e.g., Long Beach). So the proper comparison is between large N nodes and DAS.

l. 276 – How does scattering of late arriving surface waves affect P? In general, the wavelengths of P mean that segment-scale heterogeneity or topography should not be cause poor wavefield coherence. Variable coupling of the cable seems more likely.

l. 283-285 – Are “not-aligned segments” the same or different than “three separated segments.” i.e., is the difference 2 vs. 3, or parallel vs. not parallel? What is the pointì here?

l. 289-290 - Maybe add that teleseismic waves coming into low-velocity, near-surface sediments are nearly vertical and horizontal DAS sensitivity is attenuated by its cos^2 dependence. Also, emphasize that this is a consequence of the PoroTomo geology and is not necessarily a generalization about DAS.
Thanks for the suggestion; these points will be added in the revised MS.

l. 302 - Clarify meaning of “intra-profile channels”. Statement seems contradictory. How can very coherent channels be dominated by locally-scattered waves, unless the profile length is small compared with scatterers?
Pertinent observation. By intra-profile channels we mean a group of adjacent, closely-spaced channels whose spatial coverage is comparable to the size of a scatterer / velocity heterogeneity. Though exhibiting mutual coherency, those channels would imagine a deformed portion of the wavefront.

l. 311-315. Did not try to estimate magnitude of events, but maybe ML can be applied if there are a large number.
Yes, in principle it may be possible, but the event must be located within the deployment; otherwise it is challenging to obtain reliable locations and hence epicentral distance.