Response-to-reviewers

We thank the editor and reviewers for their feedback on this manuscript. The comments have helped improve our manuscript, and we hope that the manuscript explains our research more clearly now.

Reviewer A:

This manuscript discusses the advantages and challenges of using new, emerging dense arrays such as DAS as monitoring networks, particularly from the theoretical and practical perspectives. The topic is highly relevant, as these dense arrays are becoming increasingly common. The manuscript also presents a unique submarine cable dataset, which makes it a valuable contribution.

Overall, the manuscript is well-written. However, I think a more thorough discussion is needed on two points: (i) the lack of coherency in the data, and (ii) the potential mixing of P and S arrival times. I recommend acceptance after the authors address the clarifications and suggestions below.

Concern:

1. It is important to demonstrate that P and S phases are confidently picked and distinguished from possible converted phases, particularly given the relatively poor SNR in the data.

There are conflicting statements and figures. For example, Line 250 notes:
"Picking S waves is often impossible for the many small events that DAS detects that are underneath the detection threshold of existing networks, such as the event in Figure 1d."
However, in Figure 2, the Kolumbo synthetics and the observed moveout speed both suggest that the large-amplitude phases are S-waves, with P-waves being weak (in the first few seconds). Other waterfall plots (e.g., Figure 5) also show S-waves consistently stronger. Thus, Figure 1d may be showing S-waves, with the P-wave buried in noise.

Thank you for pointing out these conflicting statements.

Confidently distinguishing between P, S and converted phases is unfortunately not possible for this dataset. In some cases, such as figures 1c and 5a, it is easy as both the P and S phase are visible. However, this is not the case for the vast majority of events, such as the events in figures 1d,e and 2b. Due to the relatively poor SNR in the data and single-component nature of DAS data, we cannot identify seismic phases with certainty. This is a major challenge in the analysis of DAS data as it affects any further analysis.

We have rephrased some sentences and added details throughout the manuscript to make this challenge and the consequent limitations clearer.

To clarify this for readers, I strongly recommend labelling P and S arrivals in all record sections. This would help show that phases are confidently identified.

We have added P and S labels to all figures where we can confidently identify the P and S phases, namely Figure 1c and 5a.

Similarly, in Figure 3 it would be useful to distinguish detections based on P, S, or both, since this is central to event location. From the text and Figure 5 it appears that locations are based on P arrivals only, but this should be stated explicitly.

We do not have a phase allocation in our catalogue for all events. For all events we picked only one phase. If multiple arrivals were visible, we preferentially picked the P wave if it was strong enough. However, in most cases we only see one arrival. In those cases, we cannot, with absolute certainty, identify the phase. Most likely, those arrivals are S-waves or surface waves. We always pick the first *visible* arrivals.

We have added some more text in the methods and discussion sections to discuss this.

2. The manuscript often references a “lack of coherency” as a rationale, but it is unclear how this appears in the waveforms. More description is needed. Does incoherency occur even in direct P and S phases, despite consistent polarity (Line 192)? By “incoherent,” do you mean differing waveform shapes after onset that reduce correlation coefficients or the emergence of secondary phases due to shallow heterogeneities? A clearer explanation (possibly with an illustrative figure) would strengthen this point.

With “coherency” we mean amplitudes with a relatively consistent polarity in space and time of seismic arrivals.

We have added a more explicit definition and description of our use of the word “coherency” in the first-arrival time picking subsection, and we use figure 2 to show the (lack of) coherency for synthetic and observed data of this DAS experiment and another experiment.

Other comments (in order of appearance):

1. In methods, What are the exact dimensions of the image? For space, does it include all channels (14–23 km)? For time, what is the length of the time window, and is it applied as a moving window? The choice of time window could affect detections, e.g., whether P and S waves are separately detected which is noted in the manuscript, especially for more distant events.

The algorithm is run on an image using all channels between 14-23 km and 7.5 seconds. We read in 5 minutes of data at a time, and then we use a moving window of 7.5 s to trigger detections. This moving window is kept small to allow for the separate detection of small events that occur with a small soon after each other. However, it can happen that the P and S arrivals occur more than 7.5 seconds apart, which results in separate detections. We have manually verified the results and merged those events with separate P and S detections into one detection.

We have added these clarifications in the manuscript.

2. What is the rationale for using a 5–19 Hz bandpass, rather than a broader range? Would including lower frequencies risk ocean noise contamination in the submarine section, or might it improve detection?

The upper boundary is chosen because we work with the decimated dataset of 40 Hz. The lower boundary is chosen through trial-and-error. We are removing some signal from the data with a lower boundary of 5 Hz. However, purely for the purpose of detecting of events, we found it easier to use a lower boundary of 5 Hz.

We have added a clarification in the text.

3. Line 121–122 (travel-time model): A citation (Kennett and Engdahl) is given but not specified in the text. Could you confirm which one is used for theoretical P and S arrival times (IASP91, AK135, etc.)? A regional velocity model with an appropriate Moho depth would likely be more suitable if available. Also, why not use the 1D velocity model from the “event location” section for consistency?

We used IASP91.

In this case, the accuracy of the travel times is not very important. We only use the theoretical travel times to facilitate the automatic matching of the events in our catalogue and the NOA catalogue. These theoretical travel times are not used for anything else. Given the uncertainty in the local velocity model/seismic phase, we used very broad margins to match events, and then we manually checked the matches.

We did not use the 1D velocity model from “event location” for 2 very practical reasons. Firstly, we matched the events between the catalogues before we had set up and created the local 1D velocity model. Secondly, the IASP91 model is one of the default models in the Obspy functionality we used to calculate theoretical travel times, which made the analysis very easy to implement. Given that we do not depend heavily on the accuracy of the theoretical travel times to automatically match the events, and we manually verified the results, we choose not to redo this calculation.

We have added a clarification of these details in the manuscript.

4. Equations (2) & (3): What constant values were used for K, C and A0? How exactly is amplitude measured, using strain rate and if it is peak or RMS amplitude?

We realise that these Equations are causing some confusion. We are actually not using the DAS data to calculate the magnitudes at all, since we do not have accurate locations based on the DAS data. Instead, we reverse engineer what the kappa of this equation would be based on the events, locations and magnitudes as recorded in the NOA catalogue. The slope of the red line, manually drawn into Figure 4b corresponds to the kappa in Equations 2 & 3.

We have moved the location of the Equations and their description to the paragraph where Figure 4b is discussed. Hopefully, this will clarify our intention better.

5. Line 135: What frequency range is used in the cross-correlation algorithms? Would lower frequencies improve coherency by reducing scattering from shallow heterogeneities?

Unfortunately, my attempts to make this algorithm work are such a long time ago that I cannot retrieve exactly which frequency ranges I tried in the process. In general, I have experimented with different frequencies, including simply using the raw data directly. The “incoherency” that makes it difficult to correlate channels with each other is also visible in the raw data, where lower frequencies are included.

6. Line 154: Does “few hours of consistent picking” mean per earthquake, or across all earthquakes combined?

Good question, this is for all events combined. We have clarified this in the manuscript.

7. Line 190: Are the “first arrival times” P-wave only, or both P and S?

See the explanation earlier. We preferentially picked P waves, but if only one phase could be identified, then the seismic phase could also have been an S wave or surface wave. We always picked only 1 phase.

The manuscript should explain this more clearly now.

8. Line 191: The discussion could be clearer on the cable coupling conditions. For example, on Grimsvötn the fibre is directly buried, while submarine cables are laid on the seabed with additional complication from the cable construction type, which may reduce coupling. A short explanatory sentence would help.

Thank you, we have added some explanation.

9. Line 203: The statement about clustering of SNR values implying natural vs. anthropogenic origin is unclear. Usually frequency content is used for this distinction. Please clarify the reasoning.

Anthropogenic activity can cause repetitive events with similar energies, which would result in spikes at a certain SNR value, or a flat curve on a Gutenberg-Richter-like plot. This is not a certain way to distinguish between anthropogenic and natural events, but it gives us a hint that, at least, the majority of the events are natural in order.

We have extended the explanation in the manuscript.

10. Line 206 & Figure 3 and 4: The statement *“we would be able to locate [the event] if the submarine fibre section were less linear”* is confusing. The caption says the event is detected with SNR > 1, so it is detected, but not located. Consider rephrasing to emphasise the distinction: linear layouts do not bias detection by azimuth but do limit location accuracy. This point (already noted in the abstract) should be reiterated in the discussion.

Thank you for mentioning this is unclear.

We have changed that specific line to be more specific (the figure is actually an example of an event that we can detect & pick arrival times for). Our inability to accurately locate the events with our fibre is mentioned later in the discussion when discussing the event locations.

11. It would be helpful to show the distribution of seismic stations that contributed to the NOA catalogue in one of the figures, to highlight the spatial coverage for detection and location.

Figure 5 b&d show the seismic stations that are used to detect that specific event in the NOA catalogue. It would be challenging to include a figure of all seismic stations that contribute to the NOA catalogue, since the stations used for each detection vary. In general, the stations are located all over Greece, on the mainland and the islands. We have referenced the HL, HT and HA networks specifically in the additional information and the link in the references directly refer the reader to a map of each network's station distribution.

We have added an explicit sentence when the NOA catalogue is introduced to reference some of the station networks it is based on.

Recommendation: Revisions Required

Reviewer B:

Dear Authors,

You present an investigation of seismicity in the Kolumbo–Santorini region using a telecommunication fiber cable instrumented with a DAS system. The study is interesting and relevant to *Seismica*. I have a few minor suggestions that could make the manuscript more complete with respect to seismicity monitoring using DAS in this area.

You clearly demonstrated an increased number of detected earthquakes using your approach (Fig. 3). I particularly appreciated your discussion of the strengths and limitations of each method tested.

My main suggestion is to include a magnitude estimation for the newly detected events using one of the proposed DAS-based magnitude scales. For instance, you might consider:

Yin, J., Zhu, W., Li, J., Biondi, E., Miao, Y., Spica, Z.J., Viens, L., Shinohara, M., Ide, S., Mochizuki, K. and Husker, A.L., 2023. *Earthquake magnitude with DAS: A transferable data-based scaling relation.*Geophysical Research Letters, 50(10), p.e2023GL103045.

Any suitable magnitude scale would be acceptable. Estimating magnitudes would allow you to compare the magnitude of completeness before and after including the DAS-detected events, and would provide deeper insights into the effectiveness of your approach and of DAS in general.

Unfortunately, we cannot calculate the magnitudes of our events directly from our data with any scale, since we do not have locations for the events. Without a location, or at least a distance measure from the source to the receivers, we cannot determine a magnitude estimate at all. Instead, we calculated the SNR of each event, as a magnitude-proxy in the absence of accurate event locations.

Figure 4c shows that all the DAS events that we detect and locate are located near the start of the submarine section of the fibre. We know that these locations are inaccurate because the submarine section of the fibre is too straight to accurately locate the event. We have tested this using the event in figure 5, where we know the accurate location from the NOA catalogue. If we use the entire fibre, we can get a relatively accurate location. However, if we only use the submarine section of the fibre, the event is located inaccurately near the start of the submarine section of the fibre (this figure is in the appendix). In general, we can only use the submarine section of the fibre to locate the vast majority of the events, since they are only detected along this submarine section.

Additionally, once magnitudes are estimated, you could present a magnitude–distance detection threshold for each channel and for the overall array. Such an analysis would add a valuable perspective that is not commonly seen in similar studies.

Minor comments:

- What is the frequency content of the simulation and the data shown in Fig. 2? The frequency content in Fig. 2b appears to be significantly higher than in Fig. 2a. Please clarify and provide additional details in the text.

All synthetic simulations are created with an upper frequency of 3 Hz. To compare the synthetic data with the observed data, we applied similar bandpass filters to both the synthetic and observed data. For the Kolumbo data, we applied a filter of 0.1 – 3 Hz, and for Grímsvötn, we applied a filter of 1.5 – 3 Hz.

We find it hard to judge the visual appearance of the frequencies figure 2b, since the data quality is rather poor with incoherent waveforms. We expect that the waveforms may seem to contain more high-frequency content due to the incoherent waveforms. In the zoom of fibure 2b, the frequency content does not seem much higher than in figure 2a.

We added the simulation / filter frequencies to the figure caption.

- In the introduction, you state that no training data are available for earthquake detection using DAS (particularly for submarine cables). However, one could follow a semi-supervised approach similar to Zhu et al. (2023). With an appropriate ML workflow, it is possible to construct a relevant training dataset for this array as well. I recommend rephrasing your statement about this limitation.

Thank you for the comment, we have rephrased the statement.

Recommendation: Revisions Required
