Below we provide point-by-point responses to the reviewer comments (in blue), with reviewer comments shown in black. Line numbers (L) in our responses refer to the revised manuscript with tracked changes.

On behalf of the co-authors,

Gian Maria Bocchini

General note:

Following additional tests to better constrain the choice of frequency bands for magnitude estimation, we refined the frequency bands used for both seismic-station local magnitudes and DAS-based relative magnitudes, resulting in minor adjustments to filtering and small changes in the final magnitude values. In addition, we lowered the SNR threshold for absolute location from 15 dB to 12 dB during catalog reprocessing in order to increase the number of events with absolute locations. This resulted in additional events with reliable phase information, increasing the number of earthquakes with absolute locations from 284 to 356. Consequently, the total number of events in the enhanced catalog increased from 2,780 to 2,871. The overall workflow and methodological framework remain unchanged.

REVIEWER #1

Comments on "Earthquake Catalog and Continuous Waveforms from a Two-Week Distributed Acoustic Sensing experiment on Kefalonia Island, Greece" by Bocchini et al.

This paper analyses a period of high seismic activity in the Kefalonia region (Greece) during two weeks in 2024. The authors combine data from seismic stations and a 15 km long telecommunications cable using DAS. They construct an enhanced earthquake catalogue with a factor of ~37 increase compared to the NOA catalogue. The DAS dataset is also made available, which is valuable for testing and benchmarking new methods. The authors have done a large amount of work, and the paper addresses an interesting and relevant topic. In my opinion, the manuscript is suitable for publication in Solid Earth after the following comments are addressed.

We thank the reviewer for the assessment of our work. We would like to clarify that the manuscript is currently under review in *Earth System Science Data (ESSD) (*not in *Solid Earth)*, which is a data journal focusing on the publication of datasets and related descriptions. The emphasis on dataset production and release is therefore intentional and aligned with the journal scope. We have revised the manuscript to further clarify its focus on the earthquake catalog and the associated DAS waveform dataset (e.g., L14-15, L77-81).

General comment

I think the manuscript mixes different main goals, and this makes its overall focus unclear. In some parts, the paper is presented mainly as the release of a new DAS dataset for future benchmarking and method development. In other parts, the focus is on the methodological workflow, the integration of DAS and land seismic stations, and the construction and analysis of an enhanced earthquake catalogue. In my view, the methodological workflow and the integration of DAS and seismic station data are the central aspects of the paper, while the public release of the dataset is an added value. I also think the manuscript goes too far in stating what future users may do with the dataset. It is useful to mention possible applications, but in several places the text moves from presenting the dataset to making broad claims about its future use.

We thank the reviewer for the comment. We would like to clarify that the primary focus of the manuscript is indeed the public release of the datasets, namely the DAS waveforms and the earthquake catalog, which is consistent with the scope of the *ESSD* journal. We now better clarify the objectives of the study (L14-15, L77-81, L487-489, L632-634)

We note that the earthquake catalog has not been published previously, and therefore a detailed description of the methodological workflow is essential to reproducibility. This is particularly important because the integration of DAS and seismic station data is not yet standardized. Including these methodological details improves the transparency and reproducibility of the catalog construction.

Following the comments of both reviewers, we have also moderated statements concerning potential future applications of the datasets and now frame these as possible use cases rather than broad claims (L37-39, L82-84, L538-556, L633-635).

Specific comments (in the order they appear in the manuscript)

• L15 and throughout the manuscript: please use distributed acoustic sensing in lowercase.

We thank the reviewer for the suggestion. We have changed "Distributed Acoustic Sensing (DAS)" to "distributed acoustic sensing (DAS)".

• L15: remove "on a telecommunications cable", as this is repeated below.

Thanks for the suggestion. We have rephrased the entire sentence (L14-19).

• L22: the enhancement results in 2,780 events. I suggest also adding here the increase with respect to the official catalogue.

Please note that we have slightly edited the initial version of the catalog by considering for locations all events with SNR >12 instead of SNR > 15 as in the initial version.

We have followed the suggestion of the reviewer and included the increase in number of earthquakes with respect to the official catalog from the National Observatory of Athens. (L28, L31)

• L28-30: *"The aim is to provide a resource for researchers to test, develop, and benchmark DAS processing algorithms on tectonic earthquakes and to investigate the physical processes that drive complex seismic sequences"*. Please see my general comment above.

We thank the reviewer for this comment. We agree that the manuscript should primarily focus on presenting the dataset rather than making overly broad claims about future applications. We have therefore revised the text to moderate speculative statements and to frame potential uses as examples of possible applications rather than definitive outcomes. At the same time, we believe it is important to briefly highlight prospective uses of the dataset to help readers understand its relevance and potential value to the community (e.g., L38-40). Please see also our response to the general comment.

• L38: "..., and monitoring..."

Rephrased. L49

• L40: "...large volumes of data that introduce..."

Rephrased. L51-52

• L59: From *"The seismic sequence occurred…"* to *"…along with other natural events in the region"* does not read well.

We rephrased the entire paragraph (L70-76).

• L58: *"Here we present a new dataset…"* and L62: *"In the following, we present a new earthquake catalog…"*, please see my general comment above.

We rephrased the entire paragraph (L70-81). See also our response to the general comment.

• L68-69: *"The dataset released with this paper…"*, please see my general comment above. This idea is repeated again in L97, where it could be kept. Also, nothing is said here about the metadata being shared as well (for example, cable and channel coordinates).

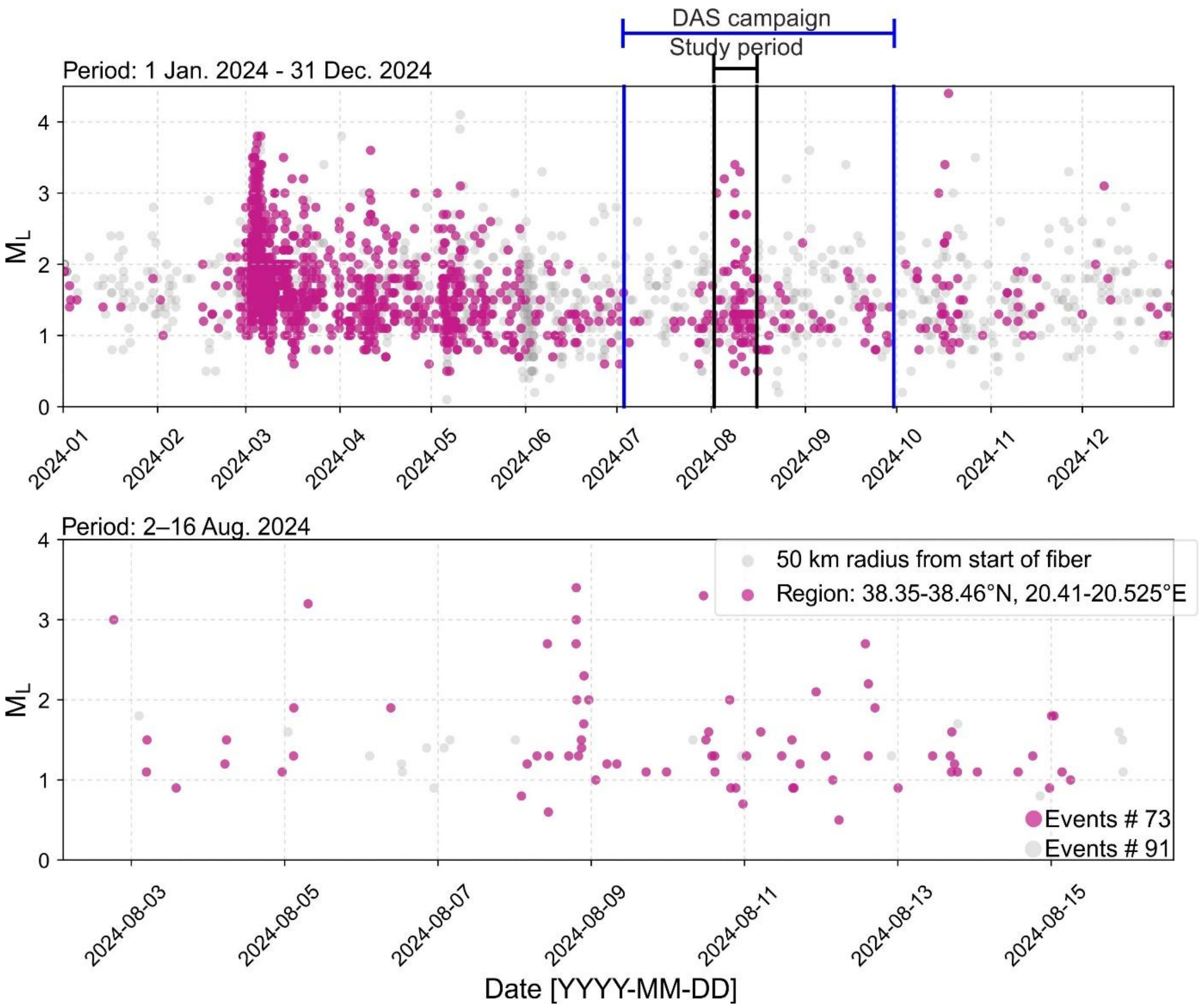
We have rephrased the text which now appears on L82-84. Following the comment of reviewer #2, we have added a new section about the metadata (Section 9).

• L84: The meaning of DAS is already given in the Introduction.

Changed. L114

• L87-88: *"The DAS measurement period ranges from early July to late September 2024"*, but the study focuses only on two weeks (2-15 August 2024). Please explain why this time period was selected.

We select the time after checking the number of events vs. time from the revised NOA catalog. We noticed an increase in seismicity rates in the region encompassing the seismic cluster northwest of Kefalonia and decided to investigate the provided period (see figure below). We now detail it in the manuscript (L118-123).



• L89: "National Observatory of Athens (NOA)".

We rephrased this section of text and now use only "NOA" after defining the acronym earlier in the text. L118-123

• Figure 1 caption: *"Grey triangles indicate public seismic stations from the HUSN not used in this study"*. Please explain why these stations were not used.

We exclude these stations because they are either too far from the target region (i.e., seismic cluster northwest of Kefalonia) or provide azimuthal coverage comparable to closer, higher-quality stations. We now include this information in the Figure caption (L139-140).

• L120-122: Please explain how the cable location refinement is performed, or provide a reference.

There is no reference to add in this case. We rely on a combination of two empirical constraints to refine the offshore cable geometry:

1. Onshore Tap Testing: We performed tap tests on land to precisely identify the channel numbers at the two distinct points where the cable enters the sea. This

allowed us to constrain the exact number and length of the remaining offshore channels.

2. Independent Seismological Alignment: We used the hypocentral locations from the revised NOA catalog (which do not incorporate DAS data and therefore provide a strictly independent constraint). We then plotted the automatically picked P-wave arrival times along the DAS channels against the corresponding channel-to-epicenter distances (Fig. S1). Because any geometric misalignment in the assumed cable path would introduce obvious spatial anomalies or breakdowns in the moveout velocity trend, we systematically adjusted the offshore trajectory until the picked arrivals showed a good match with the epicentral distances.

We rephrased the relevant text to improve clarity (L153-156).

• L127: The sentence *“Automatic picking of P-wave arrivals along the fiber resulted from applying PhaseNet-DAS (Zhu et al., 2023)”* should be moved earlier (around L121), when P-wave arrivals are first introduced.

We moved the sentence to appear earlier in the text as suggested. L158-159.

• L132-134: The sentence *“We observe that we can successfully record signals from teleseismic earthquakes, mostly on the offshore portion of the cable (Fig. S4).”* is distracting in this context and could be moved elsewhere.

We have now moved the sentence to L182-183.

• L134-137: *“Expected amplitude scaling with earthquake magnitude is also evident: for example, an amplitude ratio of ~230-240 between events of ML 0.6 and ML 3.0 located ~1.7 km apart (epicentral locations from NOA catalog) is consistent with the theoretical value of ~250 that is expected if the events were co-located (Fig. 2c).”*

(i) It is not clear how the amplitude ratio of 230-240 is obtained from Fig. 2c. Please specify how amplitudes are measured (e.g., which channels or averaging procedure are used); (ii) Please clarify that the theoretical value (~250) is computed as $10^{(\Delta M)}$.

Following the comments of both reviewers, we have expanded this part of the text to include the missing information (L170-178)

• L165: “…; without additional…”

We have rewritten the entire paragraph. L210-219

• L176: “…6,817 detections…” This number is different from the one given in the abstract, because it includes false detections. Please make this distinction clear.

We thank the reviewer for the comment. This difference in numbers stems from our intentional choice of terminology: at this stage of the manuscript (L229), we refer strictly to 'detections' (the total raw output of the algorithm, which inherently includes false triggers and noise). In contrast, the abstract and subsequent sections refer exclusively to 'earthquakes' or 'events' only after we have applied our manual revision processes to clean the initial detections.

Throughout the text, we maintain this strict distinction between raw detections and verified earthquakes to ensure clarity.

- L165-166**:** *"The detector is unable to distinguish between P- and S-waves, without additional processing this can lead to P- and S-waves from a given earthquake being classified as separate events."* Please clarify what this additional processing is, or indicate clearly if it is described later in the manuscript.

We rephrased the sentence to improve clarity (L210-219)

- L192: Please clarify whether Ts-Tp is determined manually and how events are classified when both phases are not observable.

The T_S-T_P values and arrival-time moveout are determined entirely by manual inspection of the record sections. We have updated the text to explicitly state it for clarity. L244-245

When both phases are not observable, we classify the events based on the move-out of the single visible phase and its similarity to other events. In such cases, the observed phase is almost always the S-wave, which is the most energetic among the body waves.

We distinguish these single-phase events from more distant regional earthquakes using signal duration and moveout clarity. Local events exhibit a sharp, clear, steep moveout across the cable array (Figs. 5a–b). Distant events show a longer-duration signal preceding the main S-wave arrival and a much less steep moveout profile (Fig. 5c).

- L218: "We compute the average…".

Rephrased. L271

- L230-232: *"We note that we convert strain to strain-rates as one of the required preprocessing steps for PhaseNet-DAS."* Please provide evidence or a reference for this statement, as this is not described as a required step in the original PhaseNet-DAS manuscript. The model is generally considered robust to input type. In addition, this statement seems inconsistent with L231-232 (*"We observe P and S arrivals of good quality despite PhaseNet-DAS being trained on raw strain rate data sampled at 100 Hz"*). If the model is robust, the need for this conversion should be clarified.

We thank the reviewer for this comment. We followed the preprocessing implemented in the publicly available PhaseNet-DAS workflow available at https://ai4eps.github.io/EQNet/phasenet_das/. In the section "support for OptaSense hdf5 format", the example code includes the preprocessing step

```
data = np.gradient(data, axis=-1) / dt
```

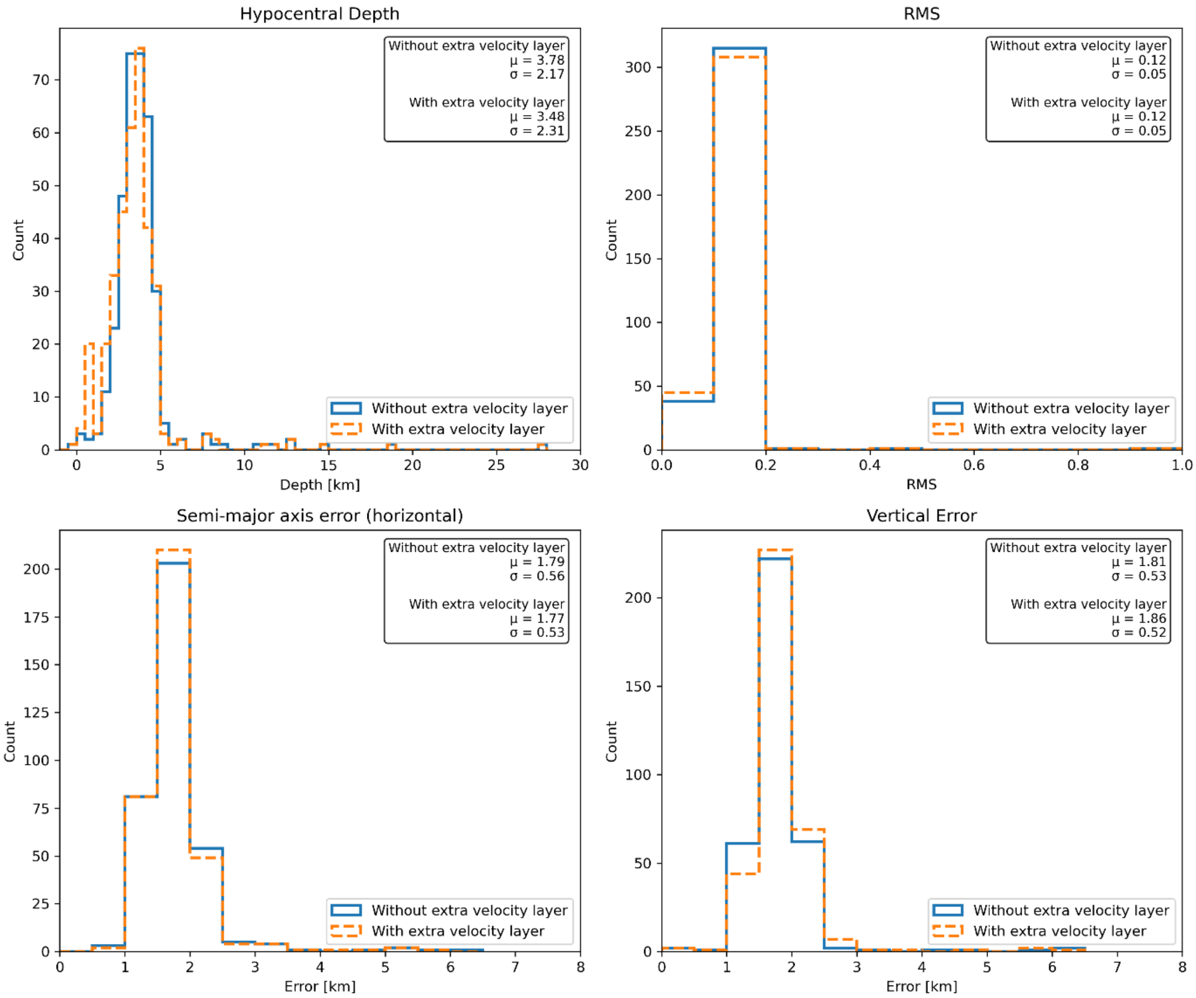
which converts strain measurements to strain rate through temporal differentiation. We therefore applied the same preprocessing procedure for consistency with the released implementation. We agree that the original manuscript wording implying this step is “required” was too strong, and we have revised the text accordingly.

We have revised the text for clarity (L287-292).

• L245-247: *“We revised the original velocity model by removing the second layer at 0.5 km depth (Vp = 5.47 km/s),* maintaining *lower velocities from 0 to 2 km depth (Table S1) to reflect the presence of unconsolidated sediments beneath the offshore cable segment.”* Please clarify how this modification is constrained. For example, is it based on borehole data, reflection/refraction studies, or other independent observations?

This modification was made based on the results. Removing the second layer at 0.5 km depth reduces the number of very shallow earthquakes (0–1 km) and slightly improves depth resolution. We note that there are no significant differences between the hypocentral locations obtained using the original velocity model and those obtained with the modified model presented in this study (see figure below). Please also note that the errors and depth distribution shown in the figure included in this response do not include station corrections, unlike the final catalog statistics discussed in the manuscript and reported in the Supplementary Material.

We now detail it in the manuscript (L313-315)



• L254-255: *"A total of 280 of the 284 earthquakes have semi-major error ellipse axis values and vertical errors < 5 km."* This statement is not very informative, as Fig. S8 shows that most location errors are already well below this threshold.

We have followed the reviewer's recommendation and removed the sentence (L322-323). Please note that the number of events has changed after updating the catalog.

• L265-267: *"We estimate local magnitude (ML) of located earthquakes using seismic station data following the empirical equation of Hutton and Boore (1987). We apply a bandpass filter to the waveforms in the band 2-30 Hz and calculate the maximum amplitude on the two horizontal components at each of the four seismic stations..."*

Please clarify that the NOA catalogue computes local magnitudes using the Hutton and Boore (1987) relation. In addition, if this relation is used here, the amplitudes should be instrument-response corrected and converted to Wood-Anderson equivalent amplitudes.

We now clarify that the NOA catalogue computes local magnitudes (M_L) using the empirical relationship of Hutton and Boore (1987) and add the reference to the paper of

(Melis and Konstantinou, 2006). For the seismic station data used in this study, we removed the instrument response and converted the recordings to Wood–Anderson equivalent amplitudes prior to calculating M_L. In addition, we applied a 1 Hz high-pass filter to the seismograms to suppress longer-period noise, such as microseisms, which particularly affect smaller-magnitude earthquakes (L335-339).

We note that the comparison between our M_L estimates and those reported by NOA remains highly consistent regardless of whether the 1 Hz high-pass filtered data or the Wood–Anderson equivalent amplitudes are used (Figure 1 in the response to this comment). However, the benefit of applying a 1 Hz high-pass is evident when calculating relative magnitudes from amplitude ratios of events with absolute locations and M_L values determined from seismic stations, as the lower-magnitude events become more tightly clustered after applying the 1 Hz high-pass filter (Figure 2 in the response to this comment)

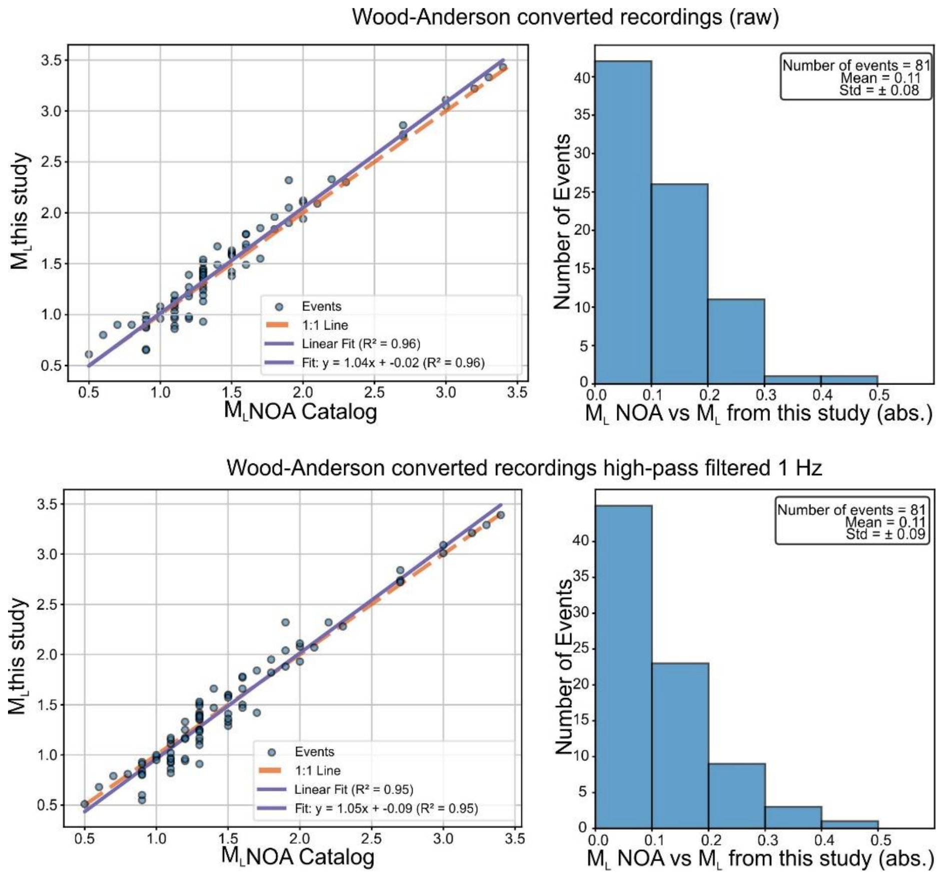


Figure 1. Comparison of M_L from this study with those from NOA. The upper panels use Wood-Anderson-converted recordings without filtering before estimating the amplitudes. The lower panels use Wood-Anderson-converted recordings and a 1 Hz high-pass filter before estimating the amplitudes.

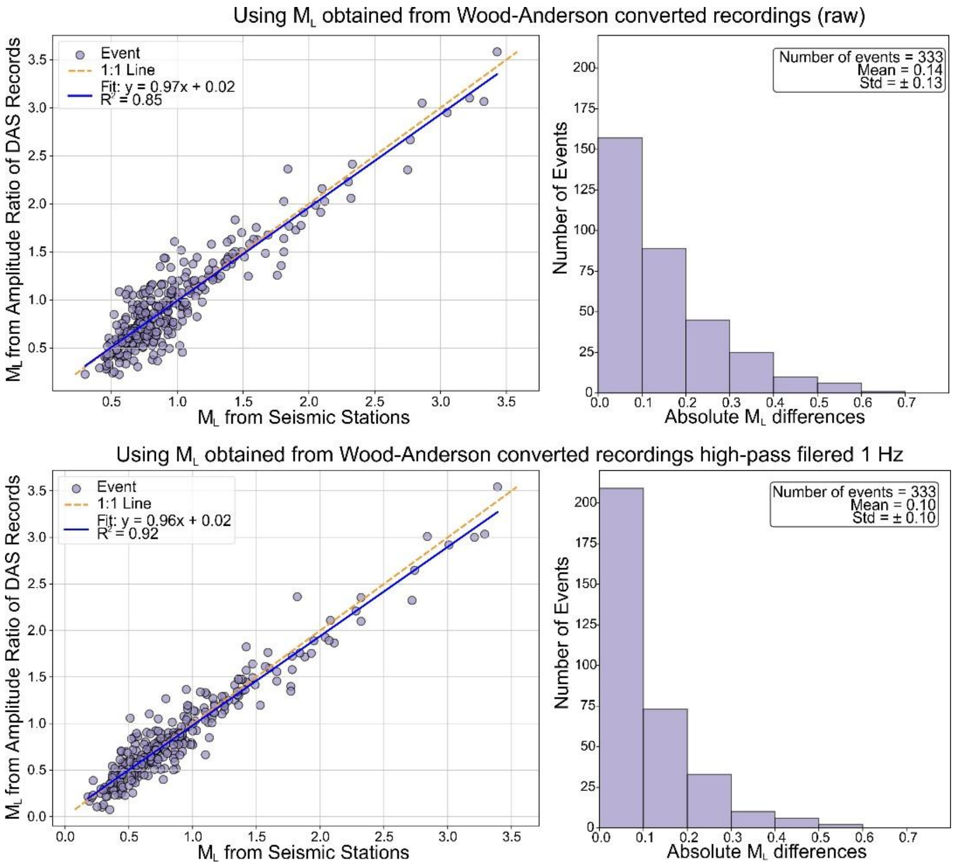


Figure 2. Comparison of M_L obtained from seismic stations and those derived from amplitude ratios with the most similar of the located earthquakes with a magnitude estimate. Only events with a similar event having a cross-correlation coefficient ≥ 0.52 are considered. The upper panels use M_L values obtained from Wood–Anderson-converted recordings without filtering. The lower panels use M_L values obtained from Wood–Anderson-converted recordings with a 1 Hz high-pass filter.

For DAS data, instrument-response removal and conversion to Wood–Anderson-equivalent amplitudes are not yet well established. Therefore, for DAS, we rely on relative amplitudes, which we demonstrate can reliably retrieve magnitudes from relative amplitude ratios (Fig. 2 in the response to this comment; Fig. 9 of the manuscript). We apply a 3–20 Hz band-pass filter to the DAS waveforms. The lower corner frequency is chosen to suppress long-period noise present in the offshore DAS segment, while the upper corner frequency is sufficiently high to capture the peak amplitudes of smaller earthquakes whose peak spectral amplitudes occur at frequencies below the source corner frequency (e.g., Abercrombie and Baltay, 2025). At the same time, the upper corner frequency of 20 Hz is kept as low as possible to avoid introducing significant effects related to source complexity, as lower frequencies have been shown to better capture source dimensions (Turgman et al., 2021). The details discussed above are now described on L435-439. The two papers cited in this response are now added to the manuscript.

• L280-282: *"A comparison of earthquake magnitudes estimated here and common events in the revised NOA catalog shows a correlation coefficient of R = 0.96 and a near one-to-one relationship, suggesting a strong correlation (Fig. S9)."* I do not think the correlation coefficient is the most relevant metric here. A high R2 value only shows that both magnitude estimates covary, but it does not demonstrate agreement. In this case, the important comparison is with the 1:1 line, and Fig. S9 shows some disagreement. Please quantify the residuals and discuss the deviations from the 1:1 relationship rather than emphasizing only the high correlation coefficient.

Thanks for the suggestion, we now report the average residuals and standard deviation. (see new Fig. S9).

We have added a sentence to mention the possible causes of the observed differences (L348-350).

• L295-300: The reference to Fig. S10 here is confusing, because these figures refer to the CCC among template events, which are introduced only later (L314-319).

We thank the reviewer for the comment. We understand the concern regarding the reference to Fig. S10 before the introduction of the CCC analysis among template events. Note that Fig. S10 is referenced together with Fig. 7, which shows the same event pair. The difference is that Fig. S10 displays all channels, whereas Fig. 7 includes only the channels used in the analysis. At this stage, these figures are intended to illustrate the variation in signal-to-noise ratio (SNR) across channels and the channel-selection procedure, which is the same approach applied to template-template and target–template event cross-correlation analysis. Therefore, we believe that referring to Fig. S10 and Fig. 7 at this point in the text (L381) is appropriate.

• L320-325: The choice of the CCC threshold of 0.52 needs a clearer justification. It is not obvious how this specific value is determined, and the precision of this threshold gives the impression of a well-defined criterion, while it seems to be based on an empirical relation. In addition, I do not think it is enough to say that other users can test different thresholds with the provided data. Since this threshold directly affects the results, its effect should be assessed within the manuscript itself.

We thank the reviewer for this important comment. We agree that the choice of the CCC threshold of 0.52 requires clearer justification, as it directly influences the number of associated events. We clarify in the manuscript that the threshold is empirically motivated based on the observed CCC–distance relationship. We now include a new figure in the supplement that shows the effects of different CCC thresholds on the number of events per magnitude bin (Fig. S12). L412-422

• L335-342: The use of amplitude ratios to compute relative magnitudes implicitly assumes that absolute calibration is not required. While this approach is valid in principle, it relies on strong assumptions (similar path, radiation pattern, and frequency content), which may not hold for event pairs with non-negligible separation (CCC threshold = 0.52). This should be discussed more explicitly. In addition, the use of different frequency bands (5-20 Hz for DAS and 2-30 Hz for seismic stations) may affect amplitude ratios and should be justified.

As noted in response to the previous comment, we quantify the expected inter-event distances between events with different cross-correlation coefficients. We also mention the potential errors in M_L determination based on expected inter-event separations from the analysis of CCC-distance relationship carried on with templates events with known absolute locations. L412-422

Please note that we have now changed the frequency band used to calculate M_L at seismic stations as well as relative amplitudes from DAS channels and justify the choices (L335-336 and L434-439).

• L345-347: the validation based on a high coefficient of determination (R^2 = 0.92) and a near 1:1 slope is not sufficient to demonstrate agreement. As discussed above, correlation reflects covariance but not accuracy. Please quantify the residuals between initial and relative ML estimates and discuss possible biases.

We now report the mean residuals and standard deviations (Fig. 9). We also discuss possible bias (L446-452). Please note that the total number of events shown in the revised figure has increased due to the inclusion of events with SNR > 12, compared to the previous threshold of SNR > 15 used in the original version.

• L357: The reference to Fig. S11 here may be incorrect.

Thanks for spotting it. We are now changing it to Fig. S13 (Fig. S12 in the submitted version). L462

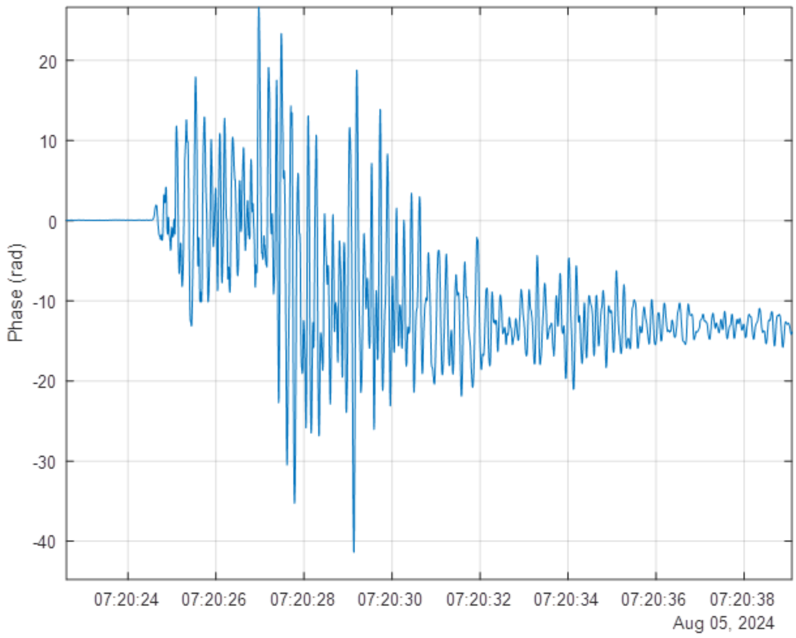
• L404-405: The interpretation of the strong amplification observed along the marine segment seems too limited. This behaviour may not only reflect shallow crustal structure, but also differences in DAS sensitivity to body and surface waves, mechanical coupling, cable construction, and installation conditions along the array, especially across the onshore-offshore transition.

We now expand the discussion on the possible origins of the observed signal amplification (L515-517). We also note that this is a data mining study, and that interpretation of the underlying physical mechanisms is intentionally limited.

.• L406: Section 6.1 (Distorted waveforms). The description of waveform distortion is interesting. The authors note that identifying the exact cause is challenging and suggest cycle-skipping as a possible explanation. Phase unwrapping and the associated ±π ambiguity occur at the interrogator level, before any decimation. In this study, only decimated data (250 Hz) are available, which makes it difficult to verify directly whether true phase-cycle ambiguity occurred during acquisition. However, the observation that the distortion is largely reduced after bandpass filtering suggests that the observed step-like changes, while preserving the overall waveform shape, resemble a baseline shift and may therefore also be related to high-frequency processing artifacts or other amplitude-dependent instrumental effects. I think this possibility is worth examining more carefully.

We thank the reviewer for this comment. We checked the phase shift in radians of the events showing the "distorted waveforms" (see figure below). The observed phase shift corresponds to an integer multiple of 2π, consistent with cycle skipping; a 4π shift, as in the figure below, could result from two successive events. A high-frequency oscillation appears to occur around 07:20:27.40–07:20:27.50, although the exact sequence cannot be resolved with the available decimated data (from 5000 Hz to 250 Hz). The reduction of the step-like feature after bandpass filtering may also suggest that part of the distortion may be related to baseline shifts or other high-frequency or amplitude-dependent instrumental effects, which we agree merit further investigation.

We now provide additional detail to the description of the observed waveform distortion (L522-523).



• L423: Section 7 (Applications). I suggest renaming this section as Section 6.2 of the Discussion and adjusting the title (e.g., *"Potential applications of the dataset"*). In addition, in line with my general comment, I think some statements in this section are too strong. I suggest moderating these statements.

We have renamed Section 7 as suggested (L537).

REVIEWER #2

Comments on paper 'Earthquake Catalog and Continuous Waveforms from a

Two-Week Distributed Acoustic Sensing Experiment on Kefalonia Island, Greece' by Gian Maria Bocchini and co authors, submitted for publication on Earth System Science Data.

This paper accompanies the distribution of a high-resolution earthquake catalog for the Kefalonia region in Greece, leveraging a brief yet data-rich, two-week-long observation window in August 2024. The authors used Distributed Acoustic Sensing (DAS)—converting a 15 km underwater telecommunications cable into a dense seismic array—and integrated this data with recordings from a traditional, regional seismic network.

Overall, the paper presents the outcome of a huge amount of work, which has been conducted in a rigorous and consistent manner. The resulting dataset is valuable both from a methodological perspective, as it features simultaneous observations via DAS and traditional seismometers, and from an observational standpoint, as both systems captured a vigorous seismic sequence whose spatio-temporal evolution certainly deserves further in-depth studies.

Given these considerations, I undoubtedly recommend the publication of this paper and associated dataset in Earth System Science Data. I attach a list of comments aimed at improving clarity and readability of the manuscript, which can be easily assessed in a round of minor revision.

We thank the reviewer for the positive feedback on our manuscript.

General Comments

- A first, general comment concerns the mixing of different objectives, and the repeated statements about what users could (or should) do with the data set. Considering the scope of the journal, the distribution of a high-quality and, in some respects, unique dataset is inherently the principal scope of the paper. Then, future end users should be kept free to choose the most appropriate utilization of that data. As a matter of fact, the objectives of the work are stated in the abstract (L28-30), then in the Introduction (L97-101), in the Discussion (L380-384) and finally in the Conclusion section (L463-467). I suggest synthetizing the objectives according to a few statements, and presenting them just once. The same holds for the possible utilization of the data set.

  Following the reviewer's comment, we have revised the manuscript to reduce redundancy in the presentation of the study objectives and the potential applications of the dataset. The objectives are now stated more concisely and consistently, and repeated statements regarding possible uses of the dataset were removed or rephrased throughout the manuscript (L14-15, L37-39, L77-81, L113, L129-133, L487-492, L537-556, L632-635).

- It would be helpful to include a short Section describing the metadata accompanying DAS and seismic recordings.

  Done. We added section 9. Metadata. L571

Specific Points

L39-41. Please control the paragraph: there's something missing

We have rephrased it. L51-52

L58-65. The paragraph does not read very well, and there are repetitions. Please rewrite in a more concise and clear manner

Thanks for pointing out the poor readability, we have rephrased it. L70-76

L59. Please define here the KTF acronym, so that it can be freely used in the following sections (e.g., at line 72).

Done. L70-71

L80-82. '...and interpret the sequence as being complex with swarm-like behavior, highlighting the important role of fluids in its triggering.' The sentence does not read very well; in addition, not necessarily a seismic sequence exhibiting a complex behaviour is triggered / driven by fluids.

We have rephrased the sentence (L108-109).

L89-90. The NOA acronym should be defined in full here

Done. L107

L97-101. The objective of the work is to provide documentation and supporting information about the data set to be . Please don't be too assertive in stating how users should use the data.

We removed this part (L129-133). Please refer to the response to the first general comments.

L106 HUSN acronym has already been defined earlier

Thanks, we have removed the second definition (L138).

L79, 89-90, 105. Please sort out the reference to the NOA catalog. First time you mention it, provide the URL and define in full the acronym. Then, you can use just the acronym.

Done. We now define the acronym on L107.

L135 '…an amplitude ratio of ~230–240 between events of ML 0.6 and ML 3.0'. Please invert the order of the magnitudes; the amplitude ratio of ~230-240 is between events with ML of 3 and 0.6, respectively

We have now rephrased this part to account for the comments of both reviewers (L170-178).

L106,138: The acronym HUSN has already been written in full earlier (L67), no need to rewrite it.

We now use HUSN throughout the paper as suggested.

L153-154. Please be less cryptic, by writing for instance '… removal of wavenumber equal to zero, corresponding to signals not exhibiting any phase delay throughout the different DAS channels.'

Done. L200-202.

L195. Insert an 'and' in between 'cable' and 'are'

We cannot find it in L195 of the original submission.

L207 Figure 5. Example types of event detections. -> Figure 5. Typical examples of event detections.

Changed as suggested. L260

L212-213. This does not read well, please rewrite

We rephrased the sentence L265-266.

L235 What is the clustering algorithm used for selecting the cable segments?

We have clarified the text to better describe the specific algorithm (L293-298). The clustering pipeline was actually developed from scratch specifically for this study to handle the geometric partitioning of the cable. The code performs spatial channel clustering using DBSCAN based on user-defined minimum and maximum channel counts, creating a new segment whenever the azimuthal angle variation exceeds a specified threshold. We uploaded the code in the git-lab repository cited in the paper.

This custom workflow later evolved into the spatial clustering module of the *ORION* framework (Bozzi et al., 2026).

Bozzi, E., Pascucci, G., Rapagnani, G., Bocchini, G. M., Harrington , R., Ugalde, A., Saccorotti , G., & Grigoli, F. (2026). Near real-time channel selection for Distributed

Acoustic Sensing technology. *Seismica*, *5*(1). https://doi.org/10.26443/seismica.v5i1.2251

L245 The reasons why the original 1D velocity model by Haslinger et al. (1999) has been modified should be better argumented.

See response to L245-247 of Reviewer #1. We have added a sentence to better support the change in the initial velocity model (L313-315).

L266-267. Were seismic traces convolved with Wood-Anderson's response prior to amplitude measurements?

Yes. We have now rephrased this text for clarity (L335-339).

L280. Linear correlation is not sufficient to define the mutual consistence between the estimates. Other metrics are more appropriate, e.g. the distribution of the differences between the two sets of measurements.

We have now changed Fig. 9 (and related text L352-354) to include the distribution of the magnitude differences.

L288-290. This paragraph is not clear, please rewrite.

We rephrased entire paragraph to improve clarity (L357-380).

L291-294. Methods and parameters used for correlation analysis should be moved above, at the beginning of the Section.

See our response to the previous comment.

L296 an -> a

Changed. L382

L315-316. We do so by cross-correlating all template events with one another -> We perform this task by cross-correlating all independent pairs of template events

We have partly rephrased the sentence, but we avoid the use of "we perform this task" since we do not think it improves clarity L402-404

L318-319 Because only absolute locations are available, our inter-event distance estimates for given CCC values should be considered conservative. The meaning of this statement is not clear.

Rephrased. L405-406.

L320. The logical order of this statement should be reversed. CCC is the dependent quantity, which diminishes for increasing inter-event distances.

Rephrased. L408-409.

L320-324. The choice of the particular CCC threshold (0.52) should be better described and motivated.

Done. L412-422. We also add a new figure in the supplement Fig. S12.

L431-432 Furthermore, DAS recordings acquired on the ocean floor may exhibit a higher signal-to-noise ratio compared to more distant land-based seismometers. This statement is a bit out of context; I suggest removing it.

We rephrased nearly the entire section to account for the comments of both reviewers (L537-556).

Conclusion section.

See above, General Comment #1.

Rephrased. L632-640.

30 April, 2026