Your manuscript provides a first-order description of an OBS deployment, with accompanying land stations, that is worthy of publication in a *Report* format in Seismica. However, based on the reviewers' reports, along with my own view, I feel that your manuscript would benefit from some more detailed analysis and clarifications, so that future users of the Zafran dataset could familiarise themselves with the characteristics of the experiment and its resulting data, and so that a future OBS deployment in a different area could yield some important insights from your experiment to help design their own experiment. In the following text, I summarise the main points that should be focussed on for a revised version of the manuscript. The reviewers' comments, along with my own minor, specific comments, are attached to the bottom of this email.

1. Neither the reviewers or I are experts in the geographic focus of the study, but to first order, the Red Sea appears to be quite a confined ocean basin compared to many other OBS deployments in large oceans such as the Atlantic and Pacific. Therefore, I am wondering how the oceanographic (e.g., seafloor currents; wave heights) and meteorological (e.g., surface wind speeds) sources of noise might be somehow different to many previous OBS experiments. Given the relatively localised nature of the network, I would like the revised version of the manuscript to delve (even if just at a speculative level) into these regional factors and possible processes of noise generation in the Red Sea (see also a similar point by Reviewer A), and their potential variation over the course of the deployment period. Also, given that an ROV was used for some OBSs, are there any direct observations that can help to give insights about the sensor-substrate coupling conditions at the seafloor? Following your and Reviewer A advice we deepened our noise analysis to give some insights into the source of ambient noise in the Red Sea. We found that the ZFZ, south of Mabahiss Mons, has a distinctive short-period noise, probably due to the high ship traffic in the region (see new section 3.2.1). We also find a strong correlation between wave and wind and noise period between 0.2 and 10 s (see new section 3.2.2). We also investigated the temporal variation of the noise and we found typical seasonal variations of the microseismic noise. These variations are discussed anyway for each period range (lines 297-304, 351-355,392-395). Unfortunately, there are no sea bottom current or sediment thickness datasets, but ROV images recently published by our collaborator seem to point at almost no currents at the sea bottom of the Red Sea (see line 519-520). From the ROV images, you can see only the layer of loose sediments. However, the thickness cannot be estimated.
2. The Tectonic Setting and Motivation section provides some excellent key scientific question about the study area, but it ends quite abruptly, and seems to rather stand-alone in the manuscript because many of these points are not referred to later in the manuscript. For example, I think what is missing is a few sentences stating how the new OBS network practically helps to answer these. For instance, I would guess that high-precision seismicity locations from the OBSs would help to answer the question of transform vs. non-transform offsets? This would then lead very nicely onto the following section about the new network. More generally, I am wondering whether it is worth moving/combining the "Tectonic Setting and motivations" section to the Introduction given that the Introduction is currently quite short, and that the tectonics/geology help to form some of the key aims of the study? Perhaps the tectonic setting part could be shortened a little bit if many of these points are not referred to later? Following this comment, we now shortened the introduction section, and it has no subsections (section 1). We briefly described the main geological features of the study area; we highlighted the gap in the dataset, we stated our research questions and we proposed how our dataset will help to answer them (this last point is between lines 59 to 68). The introduction also contains a few references to the seismic ambient noise that will be

discussed in the rest of the article. I was surprised not to see OBS horizontal component azimuths mentioned anywhere in the manuscript. Knowing these azimuths and their uncertainties would greatly help future users of the dataset who perform more advanced, downstream analyses. Given that there are several out-of-the-box packages to compute OBS rotation directions, I am wondering if sensor rotation analysis could be included as part of this initial deployment report. This was a good suggestion and we added a paragraph with the OBS orientations (see lines 155 -163 and table in the supplementary material).

3. Examples of local and teleseismic earthquakes are shown in Figures 7 & 8. I realise that detailed analysis of local seismicity throughout the deployment will likely be the focus of a future study, but it would be useful to give a few more details about this specific example given it is only one event. Presumably, this earthquake was not documented in any operational regional catalogues. Can you include a little inset map about its preliminary location, or plot the location on Figure 1? Does the inter-station spacing and azimuthal coverage allow for any constraint on its hypocentral depth? It would also be insightful to show a figure sub-panel, potentially with spectrograms, showing a zoom-in of the P and S-wave arrivals. Was the first-motion signal strong and impulsive enough to compute a focal mechanism? For the teleseismic earthquakes in Figure 8, there appear to be additional arrivals aside from the direct P and S; for example, ~200 seconds after the S – is this a depth phase? Can any of these additional phases be annotated? Following this comment, we deepened the analysis of the local earthquake and we found that it is a hybrid event (see new section 4.3). We added the spectra and spectrograms for the 3 closest stations. The picking of the first arrivals, and even more the polarity, is not straightforward, especially at high frequency. This is probably due to the complexity of the source as highlighted by the hybrid nature of the event. While the x-y location is well constrained (within the uncertainty of the not easy picking), the depth is strongly dependent on the velocity model used for the location. More analyses of this event will be included in the following articles. Regarding the teleseismic event, we remade the figure (now figure 13) as we realized that the components for the station SOUTH were inverted and we used colors to distinguish the type of instruments since reviewer A asked to facilitate the identification of the stations throughout the manuscript. Since it is already a busy figure, we prefer to keep it simple and not add further phases.
4. From a presentation point of view, there were numerous places in the manuscript where figure quality and clarity could be improved, along with making clearer links between the text and what is presented in the figures (via e.g., figure annotations/labels) - see the specific comments below. Ok, thanks.

REVIEWER A

This manuscript presents a summary of the recent ZAFRAN experiment, which deployed seismometers in the Red Sea, as well as on islands and onshore nearby. This is the first deployment of broadband OBS in the Red Sea. The deployment includes two types of OBS – Lobsters from the German DEPAS pool that have Guralp CMT-40Ts and two Fugro OBS that contain Nanometric Trillium Compacts. The manuscript compares the noise characteristics of these different OBS instruments, as well as nearby land and island stations by examining the spectra and time series of seismograms. The manuscript also introduces the motivation and goals of the experiment. This manuscript will likely be of interest to the marine seismic community due to the comparison of instruments, unique setting in the Red Sea, and tectonic setting. However, I think a few aspects of the manuscript should be improved.

Most importantly, there are some places where the clarity of the manuscript is limited. These are noted in the line-by-line comments in more detail, but generally there are places where I was unable to understand the connection between discussion text and features of the data ostensibly shown in the figures. In some places, there is just a lack of clarity, in others it seems like the text and the figures do not agree, which is a more serious problem. In its current state, I can't assess if these issues are due to my misinterpretations of the manuscript, or due to mistakes in the text or figures. Regardless, I think these issues need to be improved for clarity for the reader prior to publication.

I also think the manuscript could be improved by a more quantitative analysis and clearer figures. For example, a comparison of actual noise from the spectra with the quality of the seismograms would help with understanding how much the noise varies over time. Currently, the discussion just broadly focuses on some trends without referencing the noise spectral amplitudes. The figures and discussion often revolve around a few "type examples" of instruments that behave differently, but the features that distinguish them are not always clear. Moreover, it becomes difficult to track which OBS is in which category as they are simply discussed by name, and the reader must refer back to previous figures on their own to make comparisons to understand the relationships discussed in the text.

Although a quantitative noise analysis was not the scope of this deployment paper, we performed some initial analysis of the temporal and spatial variations of the noise level. The new noise section (section 3) has two main subsections, one where we try to analyze the noise as a proxy of station performances and one where we conduct a first noise analysis. The noise is analyzed considering three different period ranges (short [section 3.2.1], medium [section 3.2.2], and long [section 3.2.3]). Where available we use additional data to explain the noise sources (e.g., ship traffic for the short period, wind and waves for the medium period). For the long period, we added a noise tilt analysis (lines 372-383). For all periods we added a correlation analysis between noise level and water depths (Figure 8 and lines 290-296,347-350,389-392).

Moreover, we ensure that there are now more accurate and frequent reference between the new text and the new figures.

We modified all figures and made the new ones in a way that the reader understands the link with the category of instruments. However, within the text, we specify whether the station belongs to the island or land group as the other distinctions are straightforward and explained in the text (lines 109-111, 164-172). All the DEPAS OBS have names OBS**, Fugro OBS are NORTH and SOUTH, and all other different names are for the onshore stations.

Finally, I was just left wanting a bit more detailed discussion from this manuscript, particularly given the interesting setting of the Red Sea for the deployment. Perhaps some aspects of these are planned for a future manuscript, and beyond the scope here. I think an inclusion of these topics would make the manuscript more broadly useful to readers, but I think it is appropriate for the authors to decide if these are appropriate for this manuscript.

Yes, many aspects will be discussed in the next manuscripts. However, here we deepened the noise analysis by analyzing the spatial and temporal distribution of the noise. The new noise section (section 3) has two main subsections, one where we try to analyze the noise as a proxy of station performances and one where we conduct a first noise analysis. The noise is analyzed considering three different period ranges (short [section 3.2.1], medium [section 3.2.2], and long [section 3.2.3]). Where available we use additional data to explain the noise sources (e.g., ship traffic for the short period, wind and waves for the medium period). For the long period, we added a noise tilt analysis (lines 372-383). For all periods we added a correlation analysis between noise level and water depths (Figure 8 and lines 290-296,347-350,389-392). In addition, we deepened the analysis of the local earthquake and we found that it is a hybrid event (see new section 4.3).

Because most of the figures show results from individual OBS I lost track of where the instruments are on the map, and how that may impact their noise properties. There is some discussion of evaporite formations, there's variability in bathymetry. It would be helpful to have a map that shows those features, possibly that also shows variability of noise in different frequency bands. While the evaporites cover the entire study area, except for the Mabahiss Mons, there is no data available on the thickness variation. There may be relationships with oceanographic features that are currently unclear, but providing a figure that shows the spatial distribution of noise properties might be helpful for a broader audience. We added Figure 8 and a discussion of the geographical distribution of the noise levels (see lines 275-286, 344-346, 384-388) . This is mostly distinctive in at short period south of the Mabahiss Mons (see section 3.2.1). Noise is more homogeneous at periods between 0.2 s and 10 s (and it is correlated with the wind, wave, and water depth). At periods longer than 10 s, the differences are mainly instrumental. There's also no discussion of water depth, despite the fact that this is noted as an important factor in OBS noise properties (although it is possible that this effect may be below the noise floor especially on the Lobster OBS which has high noise at long periods). The range of water depths covered by our OBS network is narrow (750-1690 m) and the number of OBS is too small to conduct a robust statistical analysis. However, we still presented in Figures 8f and 8j discussed at lines 290-296,347-350,389-392.

I would also have liked to have seen a more quantitative comparison with other OBS deployments. While the resonant noise sources observed on previous Lobster deployments is discussed, it is unclear how the actual noise levels or performance of the instruments compares to those previous deployments. Because of the Red Sea setting this comparison could be quite illuminating. But I had trouble discerning if there were any hints of interesting trends, or if the noise properties were random. For example, a relationship between rope length and noise resonance is mentioned, but what that relationship is, and if it is consistent or different across deployments is not explained.

We added now a quantitative comparison of the noise level at short periods between the ZAFRAN and several other deployment and we found that our dataset is noisier and the noisiest stations are the southern ones, probably because closer to the ship's routes. See lines 266-274. We also rewrote the section on the rope signature, and we found a discrepancy in the period range in which this is observed (shorter in our case). We currently cannot explain these discrepancies. See section 4.2 and lines 508-523.

With the above points addressed, I think this manuscript will make a valuable contribution to the community, and I am excited to read about more results that come from this experiment in the future!

Line by Line:

Line 56: change "exposition" to "exposure". Thanks but we change completely the text of that sentence.

Line 59: seismometer type and the broader instrument design cannot be distinguished in Janiszewski et al., 2022. The latter should also be included here as having a significant influence. In this manuscript, the two can also not be disentangled, since the two OBS types both have different seismometers, and different instrument profiles and installation methods on the seafloor. This was clear to us but the text we wrote was not. This is now clearly stated in lines 234-236 and our analyses highlight the differences in the sensor type.

Line 70-72: In addition, Zhang & Olugboji 2021 use resonances near and above 1 Hz to identify the presence of contamination from sediment layers, and include proposed techniques to mitigate for these in receiver function analysis. It might be helpful to include a reference to this work. Thanks for this reference but we did not find the appropriate room in our paper for this.

Line 72 - 75: It may be helpful to be more quantitative in the definition of microseismicity here. As currently written, it is not clear that the parenthetical "magnitudes smaller than 2" is a criterion used throughout the paper or related only to the discussion of contamination from noise. It would be helpful for better context for the Red Sea tectonics. The definition of microseismicity is independent of the Red Sea tectonics, although we expect to mostly record microseismicity. Usually in seismology, we define microseismicity earthquakes of small magnitude, often 2 is used as cut off, but in general, we consider microseismity events that are not felt (e.g., Rie Kamei, Nori Nakata, and David Lumley, (2015), "Introduction to microseismic source mechanisms," *The Leading Edge* 34: 876–878, 880.). However, for tectonic studies, these earthquakes are important because they allow you to image faults and in case of fracturing are important to understand how the rocks are responding to the anthropic process.

Line 153: Is the absence of the hydrophone a feature of this type of instrument design, or a choice made for this specific deployment? If the latter, explaining the rationale for the choice would be helpful details to include. The hydrophone is usually classified as a "dual-use" instrument and for this reason it was excluded by the initial plans to facilitate the already complicated process of importing the OBS in Saudi Arabia. However, when we realized that we still needed a dual-use instrument permit to import the releasers, the OBS was already prepared and packed in the container without the hydrophones. So, we decide to proceed without them.

Line 172: Since this manuscript is focused on the performance of the different instruments, some more detail on how the Lobster OBS seismometers are coupled to the seafloor in the previous paragraph would be helpful. Obviously they are deployed free fall, but from the description and photos, it is difficult to understand where the seismometer lands on the seafloor relative to the other instrumentation, and if it has any feet to help with coupling. Although the new version of the manuscript has a weaker focus on the comparison of the different instruments, we added a new figure (Figure 3) with two sketches of the DEPAS setup. The sensor doesn't land on the seafloor but is sitting on the anchor. See also lines 117-118.

Line 221: It would be helpful to explain what criteria are used here to assess the usability of the data for each component. One of the horizontal components of OBS01 and the two of OBS08 recorded a signal that contains only noise that doesn't correlate with the vertical components indicating that the sensor was not responsive to any ground motion.

Line 239: These two stations are shown as the same type of dashed line as LAVA and KHUF in Figure 4, not as a dot-dash line. Thank you.

Line 241: It also looks like BREEM has a secondary microseism peak that extends to higher frequencies, although it doesn't have an additional peak at ~ 2 Hz as seen on QUMAN. True, thank you. In new classification they are now in the same category A (see Figure 6 and lines 312-318).

Line 252: OBS08 is referenced here, but that is the station with mostly poor data quality and it is not labeled on Figure 4. More generally, for this discussion, I found it very difficult as a reader to jump back and forth between the discussion of the specific OBS noise properties, and then Figure 4 to see how the spectra look in these different groupings. In this paragraph, I am having a difficult time figuring out what feature in the spectra distinguish the DP and LP families. For example, on the vertical component, both OBS09 and OBS10 seem to have a peak at ~ 2 s, which then slightly decreased, but continues as a broad flat area of high amplitudes up to ~2 Hz. But they are put into different groupings. I am not sure how much of this is due to difficulty in seeing the figure or an unclear explanation of the groups. It might be helpful to use a form of ordered color map for the stations in Figure 4 as opposed to the unordered categorical map. OBS08 is not in the new Figure 6. We reclassified the noise levels and made a clearer description of the classes (see new Figure 6, lines 308-318). OBS09 and OBS10 are still in two different classes because OBS09 has two slightly separated peaks and OBS10 has a large single peak.

Line 256: Another aspect that is often discussed in relation to noise at short periods on OBS is wave heights. In particular, Kim et al., 2023 (Characteristics of Background Noise in the Oldest-1 Array Deployed on the Oldest Part of the Pacific Plate, BSSA) find correlations between significant wave heights and noise levels at periods < 5 s. They also find an anticorrelation between sediment thickness and noise levels at periods shorter than 0.5s, and only find strong correlations with sediments at periods > 2 s. I think this is somewhat similar to your proposal that thick evaporites provide an attenuation effect. However, an expanded discussion of the relationship to wave heights would be helpful, as these likely have an impact on the noise properties. Another reference that may be useful is Bromirski, P. D., R. E. Flick, and D. R. Cayan (2003). Storminess variability along the California coast: 1858–2000. J. Climate 16, 982–992. It is possible that the marine environment in the Red Sea might be quite different than the open ocean where these noise effects have typically been discussed in the past, so it would likely be useful to mention even if an analysis of wave models is outside the scope of this manuscript. We are glad of these comments and references that encouraged us to conduct a correlation analysis of the noise level and wave heights (and wind). We interestingly found a strong correlation for noise between 0.2 and 10 s (see lines 318-342 and Figure 10). These are however preliminary analyses, and a deeper noise source analysis will be part of another manuscript.

Line 268: This is just on the vertical components though. On the horizontals they look more comparable with the OBS. Correct, this revised sentence is now at line 230- 232. This may also be relevant to the point made in line 248 about tilt noise. To first order, it does seem like the Lobster OBS are more prone to tilt noise since their vertical component noise is a similar magnitude as their horizontal component noise. Whereas the Fugro OBS have low vertical noise, while still having the problem of high horizontal noise levels, which is typical of OBS that still experience significant current noise without the coherent noise on the vertical component (i.e. tilt noise). However, here the difference between the Fugro and Lobster OBS is suggested to be the instrument self-noise (although, then I think I would have expected this to be consistent across components). It might be helpful to calculate the coherence between the vertical and horizontal components (e.g., Crawford and Webb, 2000; Bell et al., 2015) to assess if this difference is due to tilt or instrument self-noise. We calculated the coherence between the horizontal and vertical components at periods larger than 10s and we found to be very low for both DEPAS and Fugro OBS so we believe that strong noise in the DEPAS is due to the high self-noise (see text at lines 371-382).

Line 284: It might be interesting to see if there is a change in tilt behavior that matches the increase in vertical noise. That is, an increase in the vertical-horizontal coherence or change in direction of tilt. Bell et al., 2015 identify OBS that have sudden changes in tilt, presumably after they relevel. Since we confirmed that the high noise of the DEPAS OBS is due to the highnoise, as also observed in previous experiments, we decided to not focus on this point.

Line 290: "understanding" to "to understand". Thanks

Line 295: It would be helpful to give more detail on the difference between the "tightened rope" and the "free floating rope". This is mentioned briefly in the introduction, but some more detail could be added, possibly in Section 3.1 when the design of the OBS instruments is discussed. How is the rope tightened? When it is floating, how long is it? Is it floating vertically above the instrument? The rope is attached to a buoy, is it floating on the surface? Above the instrument? These details would be helpful for the reader to better understand these particular instruments. We agree that these are important points for this paper, so we added Figure 3(b) and the text in lines 123-125. The buoy is attached to the OBS with about 10 m long and 18 mm thick polypropylene rope. We deployed six DEPAS OBSs by wrapping the rope and the buoy with a fabric fixed to the releaser and the other six with a free rope and buoy.

Line 298-301: I am a bit confused by this discussion relative to Figure 6. The high noise from 8-10 Hz appears to be present on OBS07 but not OBS02. The noise levels at frequencies greater than ~ 8-10 Hz don't appear to be that different between OBS07 and OBS05 and OBS03. OBS02 certainly has higher noise levels, but it looks fairly different than OBS07 (taking the different sampling rates into account). It might be helpful to include daily spectrograms from more OBS instruments. Given the ones shown, I am not sure I am clearly seeing the patterns discussed in the text. We re-made Figure 6 (now Figure 11) and we show more spectrograms that help to recognize the signals we believe are associated with the rope strumming. This is highlighted on the spectrogram of the OBS02 of Figure 11 but it also visible in the other OBS with the free rope (right column). It is characterized by stronger amplitudes at specific frequencies and energy bursts of short duration repeated in time. Conversely, the spectrograms of the OBS with tightened rope look "clean" and "smooth" in the same frequency range (central columns of the new Figure 11). See also the new section 4.2.

Line 300-301: What is meant by the characteristics of the rope? This part could be explained in more detail. How does the rope vary between the different OBS, and how is that manifested in peak frequencies of noise? When we wrote this paragraph, we reported the info found in Stahler et al, which made sense to us since there can be some differences in the lengths of the ropes, because they are manually measured and cut, and possible differences in the number of knots randomly present along the rope. Nevertheless, this sentence has been removed in the new manuscript.

Line 303: Is 1000 s correct or is it a typo for 10000 s? If 1000 s, it would be helpful to adjust Figure 6 as it's not possible to observe the signal before 1000 s. We meant 10 000 s. In addition, is it expected that the signal would only be visible at higher frequencies? For a teleseismic event, I would have expected more energy at lower frequencies, but noise levels at frequencies < 10 Hz look consistent throughout. I am also a bit surprised that a mb 5.0 teleseismic earthquake is clearly visible on OBS. It might be helpful to use the same earthquake to show and spectrogram data and time series (e.g., Figure 8) for the reader to better understand the data. Thanks for this comment. This was an oversight as we agree that a teleseismic event would not be visible at those high frequencies and distances (as we also highlight that the teleseismic events are visible in the long spectrograms at low frequencies). As we see several of these long-lasting short-period events, we have now a dedicated section (4.1).

Figure 6: Edit last sentence of caption for clarity. It also might be helpful to label on the diagram where seismic energy from the teleseismic earthquake, or other noise features are present on the spectrograms. See comments above, but I am having trouble linking the discussion of features in the text to this figure. We revised Figure 6 (now Figure 11), we removed the last sentence because no longer relevant and we added a few boxes in the figures to highlight the signal discussed in the text.

Line 311: Were there any local earthquakes recorded by the land stations in the region that could or could not be identified on the OBS instruments? This is a very interesting point. However, a complete answer would imply a full seismicity analysis which is not the purpose of the paper. From what we know about the region, earthquakes mostly occur within the OBS network or in land a few tens of km east of the Saudi coast (see Figure 1). We expect that the OBS would capture eqs in the Red Sea that the land station cannot, but we don't expect the OBS to capture the onshore eq as they usually have a magnitude smaller than 2. In any case, most of these small eqs are not visible in Figures as the old Figure 5 (temporal variation of PPSD), therefore a full seismicity analysis is required.

Line 333: If I am looking at Figure 8 correctly, the vertical component for SOUTH (fifth blue trace from bottom) looks to have significant long period noise relative to the land stations. In contrast, its horizontal trace (red) looks high quality. This is in contrast to the text, as well as what I would expect from the station given its significantly lower noise levels in the spectra on the vertical (Figure 4). I think there either may be a mistake in the figure, or the figure is not clear and I am misinterpreting, or the text needs to be adjusted to match the observations. Thanks for spotting this. The components of the Fugro OBS were switched, in the sense that what was called E-W in the raw data we found to be the Z, the Z is the N-S and the N-S is the E-W. Although this was known before sending the first version of the manuscript, we made a mistake in creating the figure. The new figure (now Figure 13) is now correct.

Line 334: Is the self-noise of the instrument relevant here? I would have thought the background environmental noise, which is much higher than the instrument self-noise would have been the more important factor for regulating quality of recorded seismograms. Yes, we believe that the very high selfnoise of these sensors is important here.

Line 335: Are there any correlations between the performance of the data and the noise levels observed in the spectra? It's not obvious to me that there is a clear relationship between average noise levels at long periods and the seismograms. For example, while OBS01 does have some of the highest noise levels at long periods on component 2, OBS02 and 09 also have high noise levels on their spectra, but the data quality is significantly better for the seismograms. Presumably this says something about noise variability since the spectra is averaged over the whole deployment, and this seismogram is a snapshot of a transient event. An expanded discussion of this would be useful. I am also a bit confused by the assertion that these records are noisier than expected from the spectra in Figure 4. Most of the horizontal components are significantly noisier than the vertical components, and well above the NHNM. Personally, I am impressed by the quality of some of the horizontal records given the spectra. To address this point, we added Figure 9 where we show the monthly variations of the noise levels and we corrected the text commenting on the quality of the teleseismic waveform because it is true that we cannot compare directly the yearly- averaged noise level with the quality of the seismogram for only one earthquake. Although even monthly noise level may not be adequate to predict the quality of the teleseismic waveform for one event, we see that in this case there is more correspondence. See for examples lines 485-487.

Figure 8: What filter is applied to this data? This should be included in the caption. Waveforms are filtered between 5 and 100 s and now this info is added in the caption.

Line 350 – 355: Following above comments, I would be curious how much of this is due to temporal variability. These discussions would be strengthened with a more quantitative analysis comparing the seismograms to the noise spectra. More generally, the same comments from the above apply to this summary section. As currently written, I am not sure all of the figures match the text. Following this and the previous comment, we added Figure 9 where we show the monthly variation of the noise levels and we commented it at lines 297-304, 351-355,392-395.

Reviewer B

This manuscript focuses on the geophysical investigation of the Zabargad Fracture Zone, which plays a crucial role in understanding the formation of the Red Sea rift. With new data from OBS, researchers hope to gain a better understanding of the tectonic evolution of the Red Sea and expand our knowledge of OBS capabilities and limitations. The article presents the deployment process, data collected, and noise analysis conducted. The ZAFRAN seismic network deployed twelve Lobster OBSs, two Fugro OBSs, and four land stations to collect seismic data in this region. The comparison between land and OBS stations revealed that noise between 1 and 10 Hz was due to ocean-generated seismic noise rather than OBS resonance.

The study compared the data quality of two different OBS types, offering insights to other research groups working with OBSs, and discussed the complexity of interpreting seismic noise recorded by OBSs compared to land stations due to exposure to marine elements and various noise sources.

I think the manuscript is well-written and communicates aspects of OBS studies which are not often documented. I am unfamiliar with the Zabargad Fracture Zone and will not comment on the scientific significance of that. I think that this paper is worth publishing, I have only a few comments that I would like to see addressed:

Thank you for your encouragement.

- L. 45 and 53: seem to miss a reference at the end of the sentence. As we could not find an appropriate reference for the sentence in line 45 (now line 53-54), we rephrase it like this “while OBS’s enable the collection of data in previously unexplored regions, interpreting and removing seismic noise recorded by OBS’s is more challenging compared to noise recorded inland.”

- L.87: I don't think "packages" is suitable here. We change the word “package” with “setup”.

- L.144: it would be nice to also have a rough estimate for interstation distances for the land stations. We added this sentence "The OBS inter-station distances ranged from 17 to 42 km whereas the onshore stations were more widely spaced (33-110 km)" at lines 106-107.
- Fig. 4: the colour bar is missing for the left column; the figure resolution seems low, and details are hard to see with the dark background. Thanks for spotting and highlighting these. Figure 4 has been split in two (now 5 and 6). We increased the resolution of the plots and we changed the color scale of the plots that were in the left column and added the color scale.
- Fig. 5: It would be helpful to mark the events which are referred to in the text (e.g. L. 275-279). Also, the levelling attempts could be in another colour to make them clearly visible. Figure 5 is now Figure 7. We marked the teleseismic and local earthquake as well as the microseismic noise. We also added dark bars to point at the calibration events.
- Fig. 6: The caption is referring to the wrong stations in the upper panel. Thanks for spotting this. We remade this figure (now Figure 11) and we ensure that the caption is correct.
- Fig. 7: OBS08 is not in the figure but referred to in the text (L. 325); It would be helpful to mark the different phases (like in Fig. 8). How far is the local earthquake away (maybe add epicentral distance to the station information/text)? What is the span of the time axis? We remade Figure 8 (now Figure 12) and added the x scale (time). We meant to write OBS09 instead of OBS08. We now corrected it (see lines 452-453). We added the epicentral distance of each station on the waveforms.
- Fig. 8: How were the waveforms filtered? Figure 8 is now Figure 12. Waveforms are filtered between 5 and 100 s and now this info is added in the caption.
- Generally, I am missing a table with all the station information overview, e.g. lat, lon, deployment depth, OBS type, rope type, clock drift, etc., similar to Stähler et al. Thanks for suggesting this. We added with all the relevant information in a table in the supplementary material.
- Except for the noise, local and teleseismic events, were any other kinds of signals recorded, e.g. anthropogenic noise, animals, storms? This question was important in restructuring our article. We have now a section just dedicated to the notable signals (section 4). In this section, we discussed a long-lasting high-frequency tremor (likely to be anthropic), we deepened the analysis of the local earthquake that was found to be hybrid and we revised the part of the free rope signature. We also correlated the total wave height and wind with the noise levels between 0.2 and 10 s (see lines 318-338). We did not find so far any signal associated to animals.

Handling Editor (specific, minor comments):

- In the Data and Code Availability section, and/or elsewhere in the manuscript, it might be good to use the official FDSN citation for your 5Q network.

  In the Data and Code availability section we specify the FDSN network code with which will be possible to find the data on the geofon database. We prefer to not add the network code every time we mention the station code to not overburden the text.

- L53: "Seismic noise recorded by OBSs is higher and more complex to interpret than the noise recorded inland". I agree that OBS noise is complex, but is it always higher? I guess it depends on frequency. For example, at certain frequencies OBSs might well be quieter than temporary ocean island stations, or land stations in the presence of significant anthropogenic noise. So maybe this sentence should be caveated, maybe for example to explicitly make the comparison to “high-quality, observatory-grade permanent land stations”? We changed that sentence that sounded like a strong statement to a consideration of the fact is more challenging: “while OBS’s enable the collection of data in previously unexplored regions, interpreting and removing seismic noise recorded by OBS’s is more challenging compared to noise recorded inland.”

- L72: Remove “the one of the” for brevity. Thanks. However that sentence was removed in the new version,
- L86: “… we design and deployed the first OBS network”. This part of the sentence needs a qualifier, such as adding “in the region/area” at the end of the sentence. Thanks. However that sentence was removed in the new version.
- L128-129: Would moderate-large earthquakes pose a threat from ground shaking or the tsunami risk, or both? This is an interesting comment in fact in the first version of the manuscript we mention the tsunami hazard in the non-technical summary. We add the mention to tsunami to this sentence because we cannot exclude the possibility. See lines 58-59.
- L141 & L342-3: I believe that “wideband” and “broadband” mean two different things in the seismic instrumentation community, so maybe some clarification or revision is needed here? We refer now only to broadband through the text
- L144-145: Why do the land stations collect information from rays travelling deeper in the mantle? Because we expect (and we are interested in) the seismicity within the network. Thus, land stations are located far the sources and record body waves that travel deeper.
- L156: Maybe remove “Besides” at the start of this sentence? The full sentence was revised (line 125).
- L160: Change “send” to “sent”. DOne (line 129).
- L189: Is “roughness” the correct word here; maybe “open sea environmental conditions” makes more sense? Thanks (Lines167-168).
- L195: Maybe change “to avoid poor outcropping geological material (mostly loose sand)” to “ensure good coupling with bedrock”? We changed the sentence to “Selecting locations closer to the coast was not possible because of coastguard permit limitations and lack of solid bedrock (lines 171-172).
- L205: Maybe a citation is needed here about temperature effects on long-period noise? We removed this sentence in the new version.
- L219: Remove space in “OBS 04”. Done
- L251-254: I feel that this section this missing some figure cross-references, along with some labels/annotations in the figures to help direct the reader. We revised the classification that is now discussed at lines 308-318 and figure 6
- L290: Change “… help understand better phenomena…” to “…better help understand phenomena…”. Thanks, The new version of the manuscript does nit have this sentence.
- L302-304: Is a cross-reference to a figure missing here? Yes, it was. However, the new version of the manuscript does not refere to the earthquake in Papua New Guinea.
- L309-310: Please label/annotate these features in the figures themselves. In the new figure 7 we annotate the local and teleseismic events and the microseism noise.
- Figure 2: Please check sub-panel labels – one seems to be missing, and out-of-order. Done
- Figure 4: Following the comments of all reviewers, we split Figure 4 into two figures (Figure 5 and 6)

- Please ensure that the x-axis range is limited to the period-binning range of the PPSDs. This is now corrected in the new Figure 5
- Panel (b) is quite complicated. Would it help to use a degree of transparency when plotting each coloured line? Also, is it worth plotting both horizontal components, or maybe instead perhaps just taking the average noise level for both horizontals? The new figure (Figure 6) that should be much easier to read.

Figure 5: Why is there an apparent data gap near the end of December 2021? If you mean in the land station, this is due to sky coverage = lack of power. The solar panels that come with the Nanometrics kit are quite small, even for places like Saudi Arabia. Some other random lines were due to a bug in the script used to make the plots. Thanks

- Figure 6:
  - I'm not sure what "reaches" means in the caption text. Thanks. We removed the sentence
- Figure 7: This figure is now Figure 12 and include much more material
  - Please add x-axis tick labels and state in the caption what the numbers mean beneath each station code. Done
  - The location of this earthquake should be given in a sub-panel, or the map in Figure 1. The location is now in Figure 1.