

Response #1 to Anonymous reviewer 2 on manuscript submitted to OS “Monitoring surface gravity wave variability with State of Polarization sensing on a subsea telecommunication cable” by Pelaez Quiñones et al.

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We appreciate your constructive comments to improve the quality of our manuscript. Please find below a point-by-point reply to the matters raised in your review (*italics* are used for citing the reviewer’s comments). Proposed text changes for a tentative revised version are indicated in [brackets]:

- On the Main suggestions:

1. *“Restructure texts and figures to foreground consistency. The most compelling result of this work is arguably Fig. 7e, yet it doesn’t appear until near the end of the paper. Therefore, I strongly suggest restructuring the presentation to first establish the high level of consistency between SoP and in-situ observations, particularly by highlighting Fig. 7E early in the paper—at least before Fig. 5 which focuses on inconsistency. Adding a similar comparison for peak period, together with wave height, would further make the comparison even more convincing.” & “A clear narrative arc—first establishing robustness, then examining limitations—would greatly improve readability and impact”*

Reply:

- Following your suggestion, we propose to insert Fig. 7e into Sec 4.1. (between Figs. 4 and 5) as a separate figure. Therein, we can also include a subpanel with the full-observation time series of the peak period from the reference data (TPK and *TPK) and SoP, as you suggest. This is expected to complement the message of Fig 4 and (partly) Fig 5 in outlining the robustness of SoP early in the results (Sec. 4.1)

- “The correlation in Fig. 7a is worse for small S_1 ’ values, which is likely due to SoP noise levels. This is also evidenced in Fig. 7e that the value is almost never less than 0.1. Explicitly discussing and presenting the SNR limitation would clarify when SoP works or not.”

Reply:

- We agree that the specific SoP noise level value was not stated in the text and was instead only implicit in the estimated SNR (L212-3). We propose to add a paragraph in Sec. 4.4 with an estimation of the SoP noise floor and explain that this compromises the reliable recovery of small wave heights (below about 1 m) for the investigated fibre link and worsens correlations.

2. *“Separate observations from interpretation. The manuscript would benefit from a clear separation between observational results and interpretive discussion. Currently, these elements are interwoven in ways than obscure logical progression. Examples include:*

- L218 says Fig. 5 will be explored in sec 4.3, which is not truly discussed until sec 5.2 L440.”

Reply:

- We agree that some text in the results would fit better into the discussion section. The sentence in L218 might have provided the wrong impression that a discussion about the observed inconsistency followed in Sec. 4.3. Here, we only intended to point out that Sec. 4.3 contains a result (specifically, the anomalous wave height response direction in Fig. 7b) that is in agreement with that highlighted in Fig. 5. We propose to remove the sentence in L219-8: “...This suggests a scenario where the mechanical state of the cable (and the sensitivity of SoP) is affected or insensitive to certain wind and/or ocean current regimes, rendering S’1 temporarily less responsive to SGW.”, which is indeed an essentially interpretative text, and instead we could change the following sentence to: “[A similar] observation will be [presented] in [Fig. 7b].”

“- Fig. 6 is interpretation and is closely related to Fig. 8, but comes before Fig. 7 which is observation”

Reply:

- Fig. 6 contains plots presenting the SoP measurements with the modelled absolute dynamic pressure induced by the different components of SGWs in time along the bathymetry covered by the SoP sensor. This figure is only intended to present a comparison between observed and modeled results, while interpretations related to it are discussed in Sec. 5
- We propose to exchange the order of sections 4.2 and 4.3 to present Fig. 6 closer to Fig. 8.

“- The discussion on what SoP truly senses is embedded in describing observation (L235–250), which I elaborate more below. I recommend reorganization such at observation are presented first, followed by a dedicated interpretive discussion about mechanisms.”

Reply:

- L235-246 is not really meant as a discussion around the measurand of SoP, but is instead a description of the parameter used to approximate the spectral boundary between wind- and swell-dominated regime, i.e the wind-swell separation frequency. This is only necessary in order to split the SoP spectrum into high- and low-frequency components that can be properly compared with the separate wind and swell contribution estimates from the reference sensors.
- After proofing the results section for discussion-focused text, we propose to migrate the paragraph from L250-5 (Sec. 4.2) into the discussion section at the end of Sec. 5.1 to keep observations presented before the discussion section.

3. “Expand analysis of what SoP truly measures. A central unresolved question for fiber wave sensing is to understand whether waves induce strain via direct pressure loading or seafloor compliance, which remains controversial (e.g. Williams et al., 2022; Tonegawa & Araki, 2024; Liu et al., 2025). The authors' long-term SoP data, paired with in-situ observations, provides a unique opportunity to potentially resolve this issue. I therefore encourage a significant expansion of the discussion in L235–250 and L380–395”

Reply:

- Certainly, it would be ideal for the manuscript to be able to contribute in some way to the debate around the specific mechanism(s) converting wave motion into fibre strain at the seabed. The key reasons that these mechanisms are not discussed in the text are that:
 - Although fibre straining could be expected to show some degree of parity among the different types of FOS measurements, we are unsure about the extent by which the (axial) deformation mechanism(s) prompting phase delays in distributed systems like DAS are directly comparable to those explaining the (radial) birefringence variations that characterize SoP. The former type of (phase) measurement is well-known to be highly directive and sensitive to vibrations. In contrast, according to Mecozzi (2024), SoP is theoretically nearly orientation-insensitive, responsive to pure twisting and suggested to be “relatively insensitive to vibrations but highly sensitive to strain induced by the direct action of pressure variations caused by ocean swells”. Thus it seems unclear to us whether SoP sensitivity observations can be safely extrapolated onto a more general fibre response mechanism that is also valid for other types of FOS systems.
 - There are a few practical constraints in our set-up that further complicate the derivation of solid evidence about these detailed mechanisms: 1) there is a single in-situ waveheight measurement at the open sea surface (where the cable is not coupled to any substrate), far from the coastal, swell-dominated region (at the wave model boundary) where in turn the tentative signals likely related to fibre strain *at the seafloor* originate (L380–395). The latter signals would be the right target to investigate the proposed fibre strain coupling mechanisms, but this region lacks colocated ground truths. 2) the fact that only very general information was shared by the operator about the cable burial, its structure, material, et al, further complicates the interpretation, so that details around the static and dynamic mechanical condition of the cable are speculative. 3) The more general fact that each SoP sample effectively integrates strains arising from a full directional spectrum of SGW acting simultaneously over different cable parts leads to a notable ambiguity if no range-resolved benchmarks exist.

In summary, we consider that more advanced developments like multiphysics modelling (e.g. FEM) coupled with spatially-resolved reference sensors (ideally in proper laboratory conditions) that are outside of the scope of our message are needed in order to properly resolve the issue around the specific mechanism(s) acting (together) at the seabed to produce the measured fibre strains and their relative contribution(s). We propose to add part of the above discussion into Sec. 5.1 to clarify this.

“Examining the amplitude ratio between SoP and in-situ observations for the same wave frequency as a function of wave directions (Fig. 7b). Directional dependence would support the compliance mechanism”

- Figs 7c,d already illustrate the suggested type of analysis in the sense that the amplitudes of SoP and in-situ wave heights are presented along with the corresponding wave directions at each sample point, for the two available frequency bands from the in-situ data and wave model (i.e. SW and WW). Constant amplitude ratios of SoP and in-situ wave heights would lie along parallel slopes in Fig. 7c,d. The lack of any clear wave direction clustering along such straight slopes indicates a lack of directional dependence that could also stem from the unstable amplitude response of S1’ discussed in L440-50. Furthermore, we reason that the orbital water flow induced by a SGW (oriented along a vertical plane parallel to its propagation direction) would also lead to some directionality if lateral cable

bending/shearing or lateral Poisson effect contributions could also be considered feasible fibre strain mechanisms. This is because SGW particle motions are predominantly horizontal at the seabed for intermediate-depth to shallow waves. As discussed in L445-53, the anomalous response direction highlighted in Fig 7b is more likely a result of a very specific geometrical constraint of the cable set-up, as it corresponds to a unique, narrow beam oriented along a specific azimuth not matching the orientation of the cable or its perpendicular.

“Interpret the scaling from S 1 to wave height of 0.017%/m. Does this scaling have any physical meaning, such as related to cable or seafloor mechanical properties?”

Reply:

- It would be useful to relate this factor to specific properties about the cable or the seafloor. However, at the current state, this value only represents a transfer constant between SoP and SGW heights that should be empirically verified by other experiments. We argue that this is a first-order estimate of the combined sensitivity of the SoP sensor and the integrated link. We propose to incorporate the aforementioned in L466 the of Sec. 5.1, e.g.: “[Notice that the scaling value from S1’ to wave height found in Sec. 4.3 effectively represents a first-order estimate of the mean sensitivity of this parameter to SGW heights (in the 0.03-06 Hz band) for the SoP sensor and cable link combination here considered (weighted by the specific cable spans that are most sensitive) and it should be empirically verified by other experiments.]”

4. “Provide a clear conclusion for the reason of mismatch. In L440–450, the authors discuss four potential causes of mismatch, but the discussion remains speculative, lacking in-depth analysis and a clear conclusion. Some discussion can incorporate sections 5.3 and 5.4 to reduce repetition. Overall, the authors should provide at least a rank of likelihood on these four reasons, rather than leaving the discussion open-ended”

Reply:

- Our initial purpose here was to present an exhaustive list of probable mechanisms that could cause physical anomalies to pass undetected by SoP. We acknowledge that less-relevant mechanisms can easily distract from the concrete case under study. Based on our observations and the S1 sensitivity derivations (Appendix D), we consider that the cause for mismatch is more closely related to mechanism (3) and/or (4), which are closely related. We propose to rework paragraph L440-50 in Sec. 5.2 to rectify this and add additional insights on the secondary mechanisms, including ranking in order of likelihood of contribution.
- Overall, a dominant cause of the mismatches can only be weakly constrained with the available data. This is due to the lack of in-depth knowledge about the properties of the cable and spatially-resolved co-located measurements along the several-km cable. However, we intend to expand this discussion based on the replies to the specific comments below.
- We initially intended the last paragraph of Sec. 5.4 to serve as a way of general summary for the whole discussion, but this may indeed turn the text a bit repetitive. Instead, we propose to merge some of the sentences of that paragraph into Sec. 5.3 (which largely addresses the same issues) and move the first paragraph of Sec. 5.4 to the previous section, effectively merging the two subsections together.

“For mechanism (1), why the authors use only S1 ? The authors emphasize a few times (e.g. L97, L442, L545) that any single Stokes parameters can give unreliable result and all 3 parameters should give a more complete description, but they never explain why they chose to use only S1”

Reply:

- The implementation of the S1 parameter alone is due to the receiver used in our experimental set-up, which was designed to detect relative variations of that specific parameter only. This is largely due to its inherent design simplicity and cost-effectiveness (the anatomy of the SoP sensor is described Sec 3.1). For a revised version, we intend to be more emphatic and change L101-2 to: “... and was designed to analyze the S1 Stokes parameter [alone owing to design simplicity and low cost. Thus, this work will exclusively focus on this parameter].”
- SoP sensitivity is generally dependent on the absolute value of the Stokes vector (its long-term component), and not only on the magnitude of its stationary oscillation (the short-term oscillatory component). Theoretically, we would expect the smallest short-term S1’ variability in the following scenarios: 1) during times when the long-term trend of S1’ stabilizes at its max/min values, as explained in Appendix D; or 2) if the Stokes vector oscillated along perpendicular planes to the S1 axis in the Poincaré domain (including the more specific case of the S1=0 plane discussed in L440-3), in a way that its short-term S1 component remained invariant even if it slowly drifts over time scales larger than SGW variability. The latter state is generally less likely to remain continuously for extended timespans, as the Stokes vector has a marked stochastic component. Furthermore, S0 (the magnitude of the Stokes vector) remains nearly constant over time, constraining such short-term oscillations along concentric circles around the S1 axis, which appears even less likely.
- Upon further comparison of the ‘mismatch’ events lasting several days (highlighted in blue in Fig. 7e) with the long term trends in S1’, we found no indication of the first of the two aforementioned cases, as the long-term S1’ is markedly unsteady and the mismatch times do not coincide with max S1’ trends. It should be noted that our sensor was designed to measure relative S1 variations but is not as reliable to retrieve absolute values due to its architecture (thus the naming S1’ instead of S1, as explained in L107-9).
- We propose to include this discussion in Sec. 5.2 and add a supporting figure showing the long-term S1’ trends during the months where mismatches took place.

“For (2), is there any evidence to support this, such as does the wave model show any large spatial variation for undetected wave events like Fig. 5?”

Reply:

- This mechanism is purely theoretical, as the output SoP is the net superposition of all intermediate states along the fibre, there is a random probability that the net S1 parameter fluctuations (the axial rotation of the oscillation plane of light in the fibre) is compensated destructively along the fibre in some cases. However, this net canceling is certainly only relevant for relatively short timespans and/or short sensitive cable spans due to its random nature. We propose to clarify that this point is rather the least likely to explain the lack of sensitivity over the course of several days in our set-up.

“For (3), do the authors observe temporal variation in response? I think Fig. 7a answers this that the scaling of 0.017%/m appears stable over time except for weaker waves with lower SNR on SoP. This actually highlights the stability of SoP response. If the cable is buried ~1m deep (L130), how can current affect static tension? Furthermore, the authors do have current measurements to evaluate if change of current always correspond to response such as Fig. 5.”

Reply:

- Fig. 7a shows indeed some variability in sensitivity over time, with a determination coefficient below 50% and errors of ± 2 m on average (20% of the max. wave height and nearly 40% of the rms). This may not be negligible for wave height monitoring applications requiring certain degree of accuracy, as it is normally expected that scatter and errors are kept at only a few percent.
- About the currents, we outline that although the cable lies on the seabed (mostly buried under sediments, as stated in L130), some (unknown) sections are exposed and could even remain partially lifted as the cable transverses valleys and other topographic features. This is particularly expected in the coastal area and across the Norwegian trench (first ~60 km in Fig. 2b). Furthermore, from L131-2, the cable rises vertically from the bottom and into the Yme platform at the water surface. All these sections could experience variable deep current-driven tension. The available current measurements are from a buoy at the water surface but there is a data gap during January 2024 (when the major misdetections took place, see Fig. 7e) in several of the parameters, including current data. To our best knowledge, there is no other current meter or in-situ sensor data available along any span of the underwater part of the cable.

“For (4), S1' also measures large waves (Fig. 7b), which requires a blocker to appear and disappear in time. This seems rather ad hoc. As discussed in the previous point, the directional dependence needs further analysis to rule out.”

Reply:

- A key observation from the study is that the clustered SGW events corresponding to false SoP negatives (those highlighted in Fig. 7e) coincide with wind waves at $\sim 100^\circ$ heading (light blue outliers in Fig. 7c). This supports that mechanism (1) is insufficient to provide a satisfactory explanation and that some geometrical constraints should be present. One of the possibilities discussed in L449-50 is that a ship or other infrastructure related to or part of the Yme platform could be recurrently situated at a certain position that shelters the cable. For instance, the adjustable height system of the (*jack-up*) platform could explain this: as the platform can be heightened by several meters, the tubed cables (these plunge into the water on one of the sides of the platform) might be fastened to the structure in some way to avoid excessive shaking. Alternatively, these might simply become more exposed to waves from all directions when heightened, whereas if the platform remains closer to the water level, its structure could block SGW coming from the opposite side (see here for context: <https://www.vesselfinder.com/vessels/details/8765280>).
- After having discarded mechanism (1) and since the two misdetection clusters are rather anomalous in comparison to the rest of the dataset, we conclude that (3) or (4) are the most likely, but the exact causal cannot be determined with confidence due to lack of available information on the cable state or platform operation and geometry.

- On the Secondary suggestions:

1. *“Improve accessibility for ocean scientists. Sections 2, 3.1 and Fig. 1 appear rather technical for ocean scientists. On the other hand, how birefringence is quantitatively related to strain, which is how the waves are sensed ultimately, is never made clear. Moreover, how is birefringence variation related to refractive index change (L90–95)? Equations like those (16) and (19) in Mecozzi et al., 2021 would be very helpful. A clear physical chain from wave pressure => strain => birefringence => SoP variation would help broaden accessibility.”*

Reply:

- We agree that these sections might contain theoretical and technical information that is possibly not standard to most ocean scientist, but these form the basis to understanding the principles of the sensor and the meaning of the Stokes parameters and SoP, which are core to the topic. We have tried to keep both sections as self-contained and basic as possible. On the other hand, we hope that these aspect might prompt some readers to dive more into this emerging topic, which is of particular relevance for ocean sciences.
- About birefringence, we propose to add the following at the end of L30-35 to make the well-established connection between anisotropy and birefringence more clear and refer the reader to the work by Mecozzi, which contains a detailed theoretical overview: “[Birefringence variations in the optical fibers induced by environmental perturbations dominate the SoP signal (Mecozzi et al., 2021). This birefringence arises from strain-induced anisotropy, i.e., differential changes in the refractive index along orthogonal axes of the fiber. These anisotropic perturbations lead to relative phase (or time) delays between light components oscillating along different polarization axes, ultimately resulting in a net evolution of the SoP as the environmental straining conditions change]”. The proposed discussion about the wave-to-strain coupling in Sec. 5.1 should close the gap in the cause-and-effect chain.

2. *“False positives vs. false negatives. The manuscript emphasizes false negatives of missed waves, but how should false positives be interpreted? For example, in Fig. 4b, the wave on SoP from Dec 16th to 18th is almost as strong as the 22nd and 25th.”*

Reply:

- Figs. 7a,c,d show a few outlier events where high SoP values occur that also correspond to low SGW values on top of the false positives. We propose to add the following text to highlight these false positives in L453: “[It is also worth noting that, in addition to the aforementioned false negatives, Figs. 7a,c,d also show the comparatively less dominant presence of a few apparent false positive SoP outliers, i.e. high SoP anomalies at times of relatively low waveheights at Yme. These could arise from cable perturbations at locations outside of the joint coverage of the wave model and in-situ sensors, including the shallow cable span between the coastline and the node room. In addition, the superposition of Stokes vector rotations resulting from simultaneous wave events at different cable spans is expected to generally raise the mean SoP variability level. In contrast, such addition of non-deterministic signals with three degrees of freedom seems unlikely to produce mean destructive interference]”.

3. *“Discuss more how to address limitations. L540–555 can have more discussion on how to address those key limitation of SoP, notably for distributed sensing (e.g. Costa et al., 2023).”*

Reply:

- We have decided to remove the paragraph of L540-55 and merge some of the mentioned points into previous paragraphs (see response to comment #4). We also agree that the discussion could highlight more the potential ways to circumvent some of the inherent limitations of SoP, especially Sec. 5.3. This could be added into a revised manuscript.

- Figure-specific comments:

Fig. 3: Use identical vertical axis scale to avoid confusion that (c) is the same as (b). Does it make sense to plot $-V_1$ on top of V_2 to show they are almost identical?

Reply:

- We propose to overlay V_1 and V_2 in a single subplot (V_1 and V_2 directly might be sufficient to show their high similarity given their anti-symmetry). The vertical range of each subplot is marked on the left hand side of each subplot. We can also add the vertical range values on the right hand side to emphasize it.

Fig. 4: Too many lines in (a). In particular, the top blue line does not seem to correlate to any observation. Consider picking at most two lines to show, such as the in-situ ones.

Reply:

- Our suggestion: to overlay only the full spectrum peak periods from the in-situ and model data, as these seem to be the ones that provide the best visual match with the data.

Fig. 5: Should appear after Fig. 7e.

Reply:

- Agreed. See response to main comment #1.

Fig. 6: Better presented closer to Fig. 8.

Reply:

- Agreed. See response to main comment #2.

Fig.7: (a) Explicitly discuss noise floor of SoP likely worsens correlation.

Reply:

- Agreed. See response to main comment #1.

Fig.7: (d) The authors argue that swell correlation is worse than wind wave because swell affects both near-coast and offshore. Would correlation improve using model predictions with spatial weights of Fig. 6c (and formulations of e.g. Mecozzi et al. 2021)?

Reply:

- This is in fact something that was attempted during the data analysis: to find a range-averaged time series from the model, weighted by the expected relative amplitudes from the model from Fig. 6c, but this did not improve correlations. The exact way that the weighting is performed, added to the actual range-dependent sensitivity function of the cable (which is unlikely uniform) may be crucial for such attempt to provide useful results.
- We think that simulated SoP data from e.g. the formulations in Mecozzi et al., 2021 would be of high relevance for a dedicated study. If comparison with measured data is intended, it seems crucial that a detailed knowledge about the mechanical state, coupling and structure of the cable, as well as spatially-resolved ground truths along a cable are available.

Fig.7: (e) This is the strongest validation figure and should be moved earlier in the manuscript.

Reply:

- Agreed. See response to main comment #1.