Post-revision response #1 to Anonymous reviewers 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 respective reviewer's comment). Replies involving modifications in the revised version of the manuscript are underscored, and specific text additions and/or changes are indicated in [brackets]

-------------------- On the comments by Reviewer 1 --------------------------

- On the Minor, general comments:

1."I really appreciated the whole discussion around S 1 towards the end of the paper, but still cannot understand why the Authors chose to monitor this parameter alone. Can they discuss this point? Concerning the lines between 432-434 and Appendix D, if I understood correctly, the Authors state that each variation affecting the Stokes vector positioned where one parameter is minimized, has maximized sensitivity on that parameter. This is true as, for example, the sphere's poles lying on S 1 are the points where this parameter is less sensitive. By monitoring one Stokes parameter alone, anyway, there could be another "fading" condition, where S 1 = 0, as stated in lines 440-443. This would completely hide variations induced by external events and, since they are extremely slow, maybe for a long period of time. Would employing a full polarimeter allow for a complete and increased sensitivity of SoP? Are there some other SoP-derived parameters that could be used to increase sensitivity? Coherent receivers' SoP estimate would provide for the whole Stokes vector."

Reply:

- The implementation of the S1 parameter alone is basically a limitation of the available receiver used in our experimental set-up, as it 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). We changed 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]."
- Indeed, as you suggest, a full polarimeter would likely allow for a more complete monitoring of SoP variations through the other 3 Stokes parameters. For instance, "sensitivity gaps" could potentially be addressed by combining the time series of the different parameters in a way that environmental signals that are "missed out" by one parameter could be recovered from another one. This may effectively improve the stability of the SoP sensitivity over time and perhaps even increase it. However, this is yet to be rigorously evaluated and outside of the scope of our manuscript since we have not yet studied how particular combinations of these parameters could produce a tentative, additional parameter with improved performance. At some point, we tested a commercial polarimeter measuring all Stokes parameters, but it unfortunately proved

unreliable for continuous logging and storage-limited for long time acquisitions, so basically unsuitable for the long-term campaign that we managed with our own SoP receiver.

- Based on your suggestion, we added to the sentences in L439-443: "... systems combining all Stokes vectors are likely to provide a higher degree of [stability and] linearity in the SoP response [as environmental signals that are randomly ill-recovered by some Stokes parameter(s) could be detected more properly by at least another.]"

2. "This sensing system is currently non-coexisting with telecommunication data. Despite the large number of interesting results the authors presented, I see few advantages to deploying the SoP monitoring system alone (i.e., installing the entire submarine fiber-optic cable just for SoP sensing), other than for telecommunication purposes. The S_1 time evolution is very difficult to interpret: a dataset consisting only of SoP data, without all the other sensors, would probably have been impossible to interpret as the authors did in this work. Given the mention of SoP monitoring with coherent transceivers that the Authors do around line 535, do they see this kind of system deployed in a "standalone" manner, or jointly working with the telecom system? In more practical terms, do the Authors see any added value in monitoring SoP alone, in this context? Would environmental data from sensors be needed to properly exploit their full potential?"

Reply:

- The presented dataset was acquired on a pair of looped dark fibres (with no other coexisting data) of an active telecommunication cable (other fibres inside the same cable were simultaneously used for telecommunication during the experiment). However, it is in principle also possible to use fibres with coexisting internet for sensing since natural SoP fluctuations are also carried by standard telecommunication signals, as described in the second paragraph of Sec. 5.3. We added: "…U-looped [dark] fibre [pair]... " in L125 and "...of the cable (Figs. 1 and 2a). [The other fibres in the same cable were allocated for telecommunication purposes.]" in L128.
- We agree that deploying a long subsea cable for monitoring SoP alone can be complex and costly. The possibility of joint work with telecom systems is thus often advantageous and generally sought as these are long-term installations typically requiring little or very sporadic maintenance. On the other hand, SoP is one out of several fibre-optic sensing systems (e.g. DAS, DTS, DSTS,…) that are compatible with the same FO cables, each system with their own capabilities, applications and potential to complement one another. We have changed L535-8 to: "SoP sensing appears particularly promising for time-resolved, [continuous] real-time monitoring of specific wave events (e.g. extreme waves or tsunamis) in difficult-to-access regions where limited or no other (temporal or permanent) oceanographic sensors might be available [or in locations of human interest (e.g. for marine energy harnessing and other offshore installations)]. This could prove valuable to [monitor and] protect offshore as well as coastal infrastructure."
- Existing ground truths currently remain critical if rigorous interpretations related to absolute magnitudes are sought with FO sensing, but not necessarily so for more relaxed analyses in terms of measurand accuracy. For example, with SoP data alone we have shown that it is possible to identify surface gravity wave events and anomalies based solely on their characteristic frequency band and dispersive trends, as shown in Fig 4. It's hard for us to tell at this point whether a more detailed knowledge on the response of SoP and future advances will be sufficient to turn SoP into a popular stand-alone sensor. Alternatively, hybrid architectures

(e.g. SMART cables with built-in conventional sensors) might eventually close the gap between fully telecom and fully FO-sensing cables.

3. "Imagine using several fiber cables to monitor SoP, for example, exploiting coherent telecommunication hardware, in different areas. To my understanding, each cable installation is different. The Authors underline this in several parts of the paper (e.g., lines 375-380, Sec. 5.2, and 6), and it looks truly critical to have the fiber sensitive to different kinds of events. It seems to me that, if the cables are not deployed for sensing purposes, but "randomly" installed for telecommunications, it is a very long and complicated process to understand what SoP can "see", and kind of impossible to do it without the aid of external sensors. My question is then: how difficult is it to scale this over the submarine communication network? It seems extremely complex and long. Could there be a situation where SoP data are almost completely silent because, for example, the cable is not that loose or suspended, and nothing couples to it? Do the Authors envision some future ad-hoc installations to maximize the sensitivity to certain kinds of events (and maybe minimize it for others)?"

Reply:

- We agree that cable installations can differ to variable extents in coupling, burial, geometry, et al that could make their response sensitivity and data quite different in terms of signal content (this could also results from uniformly calibrated sensors in different geographical regions, exposed to different kinds of environmental conditions and events) and in some cases, even preclude any signal of interest from being detected as you suggest, e.g. if the cable is simply too deep to measure SGWs. Relatively straight-forward limitations like the latter example could be extrapolated for most cables from a few reference measurements.
- At the moment, the detailed, quantitative sensitivity of each cable relies heavily on reference sensors and is normally assessed empirically via ground-truthing as there is a lack of predictive models for the sensitivity of a given cable. What could make the process particularly long and complicated is a lack of available data from calibrated monitoring probes sufficiently close to parts of the cable, as these would have to be deployed and retrieved for that purpose. The situation is not aided by the fact that detailed information about deployed telecom cables is often inaccessible, so scaling doesn't appear so simple at the moment. However, if relative event recognition alone is sought without absolute measurand quantification, the scaling seems less daunting. As an example, arrays of several telecom cables have been proposed to extend the coverage of seismic networks and to estimate source regions of earthquakes, without implying that their magnitudes can always be recovered accurately from cables alone. Some of the aforementioned practical generalities and considerations are discussed in Secs 5.3 and 5.4 in the manuscript.
- Certainly, ad-hoc installations with higher sensitivity to certain events (e.g. close to the water surface for gravity waves) are quite possible but it seems difficult to foretell reliably (or positively) about their future as long as the cables used for FO-sensing remain limited to those owned by private companies if the latter don't see prioritizable commercial interests in such applications.

- On the Figures:

Figure 4. The plot shows a very interesting match not only between the SoP acquired and the different data coming from the sensors, but also with the models. Nevertheless, I found it difficult to

read, particularly the spectrogram: the overlap between plots hides some features. I am aware of the fact that it will probably take more vertical space, but would the Authors consider, to enhance clarity and visualization, vertically stacking the line plots in 4a, easing the spectrogram? Maybe overlapping the in-situ data with the models only. In this way, the time correspondence would be preserved, and the spectrogram would be easier to read, sacrificing space for text.

Reply:

- We have overlain only the full spectrum peak periods from the in-situ and model data, as these seem to be the ones that provide the best match with the data anyway.

Figure 6. It seems like the legend in the grey box containing the numbers 10 and 1 (dotted line) is not explained in the text. Am I missing something? If not, please add the proper details to the manuscript.

Reply:

- The last sentence in the caption indicates that the dotted lines correspond to contour levels in kPa, but this could be easily overlooked. For more clarity, we changed the last sentence: "Pressure values and dashed contours [(labels in grey box)] in kPa".

Figure 7. I found it confusing to label the plots with $S1'$, when the actual plotted value is the RMS $S1'$. I think it would enhance clarity to rename labels. I am referring, for example, to Fig. 7e: in the text, it is specified (lines 313-314) "Fig. 7e depicts the entire normalized, hourly RMS time series of $S1'$ alongside…", but the labels just show $S1'$. Stokes parameters can have (also, as shown in Fig. 3) negative values. By labeling Fig. 7e with $S1'$ only, it looks a bit odd.
Moreover, the triangles mentioned are basically invisible. I am not sure if it makes sense to highlight the data gaps or just mention them in the text and keep the same marker.

Reply:

- We agree. The labels have been renamed to indicate that these plots of Fig 7 are in fact RMS values and not the original time series.
- Upon closer inspection, the few triangles are visible if one zooms the figure enough (all markers had to be kept small and with some transparency just to be able to tell their colors apart properly in heavily-clustered areas). These gaps are also mentioned in the text.

- On the Specific comments and typos:

1. Lines 30-35. I am aware that the concept of birefringence is very well known, but it would benefit the clarity of the paper, particularly to readers outside the photonics area, to maybe add some references.

Reply:

- We have added at the end of L30-35: "[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]"

2. Lines 50 to 55, when talking about surface gravity waves (SGW). In contrast to birefringence, SGW phenomena are not known to telecommunication readers. I know that the journal is outside that area, but since the subject treated is quite transversal, I think it would enhance the paper to provide a short definition and/or some references.

Reply:

- We have cited the following textbook as a general reference: {Massel, S. R.: Ocean Surface Waves: Their Physics And Prediction, chap. 4, Advanced Series On Ocean Engineering, World Scientific Publishing Co Pte Ltd, 3 edn., 2017} in L46: "...monitor ocean waves, [i.e. surface gravity waves (SGW), Massel, 2017]."

3. Can the Authors cite something related to the V-shape of the SGW, referred to between lines 180-183?

Reply:

- Certainly, we have added these two references in L180-183:
  - Ferretti, G., Zunino, A., Scafidi, D., Barani, S., and Spallarossa, D.: On microseisms recorded near the Ligurian coast (Italy) and their relationship with sea wave height, Geophysical Journal International, 194, 524–533, https://doi.org/10.1093/gji/ggt114, 2013
  - Colosi, L., Pizzo, N., Grare, L., Statom, N., and Lenain, L.: "Observations of Surface Gravity Wave Spectra from Moving Platforms", Journal of Atmospheric and Oceanic Technology, 40, 1153 – 1169, https://doi.org/10.1175/JTECH-D-23-0022.1, 2023

4. Line 336. Fig. 8k seems not to exist. Is this a typo?

Reply:

- Yes, this has been changed to "Fig. 8h."

5. Please check the acronyms used. It seems to me that sometimes they are not defined.

Reply:

- We have double-checked, and except for L119:"…[electromagnetic] (EM)...", all remaining acronyms seem to be properly defined in the main text and in the appendix table.

6. Please mention that R 2 is the determination coefficient.

Reply:

- We have checked, and the caption of Fig. 7 currently reads: "...with linear regression (including determination coefficient R2 and p-value of t-distribution…"

In addition to the aforementioned changes, a few minor typos, wording and technical precisions are highlighted in the tracked changes version of the revised manuscript. Changes related to comments from reviewer 2 are specified below.

-------------------- On the comments by Reviewer 2 --------------------------

- 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 have inserted Fig. 7e into Sec 4.1. (between Figs. 4 and 5) as a separate figure (Fig. 5 in the revised text).
- We have also included in the same figure a subpanel (b) with the full-observation time series of the peak frequency from the reference data (1/TPK and 1/*TPK) and SoP, as you suggest. The captions of Figs. 5 and 7 have been adapted accordingly to these changes and description text has been added in the second-last paragraph of Sec. 4.1: “[The peak frequency of S1' estimated over hourly time windows is shown in Fig. 5b and only evidences a moderate match with the reference data values, with errors being above a quarter of the considered bandwidth in at least 20% of the observations. Correlations are slightly better at lower frequencies during the more SGW-energetic winter season, while deviations become more marked during the Summer low in SGW activity. The 0.17~0.18 Hz constant peak is unrelated to SGW activity and corresponds to one of the dominant components of the modulated SoP noise line spectrum previously evidenced from Fig. 4a. This instrumental noise is dominant during nearly half of the entire time series and is capable of intermittently obscuring the weakest SGW signals, significantly biasing peak frequency estimates]”. The last paragraph of Sec 5.2 and the conclusions also comment on the accuracy of peak frequency retrieval with SoP. 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 have added as a last paragraph in Sec 4.2.:“From Fig. 7a, the threshold RMS S1' values below which correlations with wave heights are no longer evident lies at about 1% of the observed maximum and corresponds to the mean of the 10th percentile of S1'. This threshold also corresponds to observation times with wave heights below 1 m, such that the reliable recovery of small wave heights below this limit for the investigated fibre link is compromised and can thus affect correlations during times of very low SGW activity."

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 removed 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 changed 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 exchanged the order of sections 4.2 and 4.3 to present Fig. 6 closer to Fig. 8. In order to preserve consistency in the order of introduction of the $S1'_{WW}$ and $S1'_{SW}$ parameters which are used in both sections, the text in lines L237-46: "An accurate separation of the wind and swell wave contributions to… thus validating the calculation of this proxy from data at Yme." has been preserved as a separate paragraph in the same Sec. 4.2.

"- 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 have migrated the paragraph from L247-55: “Alternatively, the horizontal or vertical orbital particle displacement… which are in turn the only capable of interacting with deeper ocean bottoms (Fig. A1).” (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) birrefringence 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 subtrate), 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 added the following paragraph at the end of Sec. 5.1: "[This work focused on the comparison between SoP measurements and reference data, while the specific physical mechanism(s) (e.g. condensation, shearing, torsion, bending) behind the transfer from wave motions into the fibre (cf. Liu et al., 2025 for a review on the matter) are largely set aside due to the lack of detailed information about the cable and the physical fields distributed along its full span. Compared to phase-recovering FO sensing techniques like DAS, which are predominantly sensitive to axial fibre strain, the stress-induced (radial) birrefringence variations that characterize SoP are reportedly nearly omnidirectional (Mecozzi et al., 2024). Regardless that the sensitivity of phase-sensitive systems is comparatively higher, this suggests that some of the fibre deformation modes triggered by wave-induced hydrodynamic pressure and flow could potentially result in measurable SoP anomalies that would go unnoticed by the former. More advanced developments such as multiphysics simulations coupled with spatially-resolved reference sensors (ideally in controlled environments) appear key to properly resolve the specific mechanism(s) acting at the seabed to produce the measured anomalies (and their relative contributions).]"

"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 steem 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 incorporated the aforementioned in L466 the of Sec. 5.1: "[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 interrelated. We have significantly reworked paragraph L440-50 in Sec. 5.2 to rectify this and added additional insights on these mechanisms based on the discussion below. The paragraph is now split into two (paragraphs 3 and 4 in the revised manuscript).
- 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.
- 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 have merged some of the sentences of that paragraph into Sec. 5.3 (which largely addresses the same issues) and moved 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). In the revised version, we changed 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 . 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 have added a supporting figure showing the long-term trend of S1' and highlighted the months where mismatches took place (Fig. F1) and included the above discussion in paragraph 3 of Sec. 5.2 in the revised manuscript.

"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 have clarified that this point is rather the least likely to explain the lack of sensitivity over the course of several days in our set-up at the beginning of paragraph 4 of Sec. 5.2 in the revised manuscript.

"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 ~100° 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.
- Paragraphs 3 and 4 of Sec. 5.2 in the new manuscript are significantly reworked versions of the first version of the corresponding discussion, and these address the aforementioned mechanisms in deeper detail

- 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 have added the following at the end of L30-35 to make the well-stablished 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 have added 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 transient false positive SoP outliers, i.e. high SoP anomalies at times of relatively low waveheights at Yme. From Fig. 5a, the latter are less common than false negatives and cover shorter time spans. These false positives could arise from different noise sources, namely SoP system self-noise, 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 perturbations in the node room itself. 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 removed the paragraph of L540-55 and merged some of the mentioned points into previous paragraphs (see response to main comment #4). With this reordering, the discussion of Sec. 5.3 is now more balanced in terms of the inherent limitations and advantages of SoP and its title has been renamed to “Operational considerations for SoP measurements”. For instance, we have added the following in the first paragraph of Sec 5.3 in the revised manuscript: “...water regions with high SGW spatial variability. [Alternative fibre loop-back architectures in repeatered cables can improve the spatial resolution up to the separation between repeaters (Marra et al., 2022; Yaman et al., 2025)]”. Similar additions have been included in paragraphs 4 and 5 of the same subsection to hightlight ways to circumvent SoP limitations.

- 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 $-V1$ on top of $V2$ to show they are almost identical?

Reply:

- We have attempted to overlay V1 and V2 in a single subplot but the plot becomes excessively crowded. Separating subplots by filtering frequency band can make the figure too confusing. We have added the vertical range values also on the right hand side to emphasize that axis scales are not the same. The size of the lowest subplot is now proportional to its value range.

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:

- We have overlain only the full spectrum peak periods from the in-situ and model data, as these seem to be the ones that provide the best match with the data anyway.

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

Reply:

- We have moved figure 7e as a separate figure in Sec. 5.1 (see response to main comment #1)

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

Reply:

- We have exchanged the order of former Secs. 5.2 and 5.3 so that these figures are closer together (see response to main comment #2)

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

Reply:

- A paragraph discussing this was added at the end of Sec 5.2. (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:

- We have moved figure 7e as a separate figure in Sec. 5.1 (see response to main comment #1)

In addition to the aforementioned changes, a few minor typos, wording and technical precisions are highlighted in the tracked changes version of the revised manuscript.