Review of the article titled "Monitoring the record-breaking wave event in Melilla harbour (SW Mediterranean Sea)" by Lorente, P., et al. 2023

The manuscript "Monitoring the record-breaking wave event in Melilla harbour (SW Mediterranean Sea)" by Lorente, P., et al. 2023 uses different database such as reanalysis, forecasting model, radar tide-gauge and in situ coastal buoys, to describe an oceanic extreme event that occurred in the Melilla port during April 4th and 5th, 2022. It also analyses the extreme regime in the Alborán Sea. The impacts of extreme wave events on harbours and the need to revise the level of security within them regarding the new climatic conditions are interesting points to study. However, the reviewer considers that the article needs crucial improvements throughout the manuscript before being considered for publication in the journal State of Planet.

Many thanks to the anonymous Reviewer-1 for the detailed review and the number of useful tips provided. Please find below a thorough point-by-point response with the hope of improving the quality of the document to make it acceptable for final publication.

It is worthwhile mentioning that we have successfully accomplished 37 from the 39 suggestions provided by the Reviewer-1, which constitutes the 95%. The two remaining open questions are related to:

i) the examination of correlations with different climatic indices influencing the area (which is out of the scope of the present contribution and deserves a detailed exploration in the frame of a future complementary paper).
ii) the provision of spectra (which might be addressed in the next iteration with both reviewers)

Just in case the Reviewer-1 is not familiarized with the 8th Ocean State Report initiative, we would like to clarify that it is characterized by some specific limitations in terms of length (up to 3000 words) and maximum number of figures (4). Therefore, we have tried hard to fulfil all the Reviewer-1´s requirements but always adhering to the journal´s premises.

OVERALL COMMENTS

[Comment 1] The abstract should be rewritten to provide a more comprehensive explanation of all the values presented by the authors.

OK, 100% accomplished. The abstract has been completely rewritten to better clarify the main results derived from the present study.

[Comment 2] One of the main shortcomings of the manuscript is the explanation of the different datasets used. To consider the article for publication, a comprehensive restructuring of the data section is necessary to address the following issues:

a. What is the source of the data?

b. What is the period during which they were used?

c. What are the temporal and spatial resolutions?

d. When and why were these data used? All this information can be included in Table 1.

I suggest including the following columns in Table 1: Variables (SWH, wave period, wave direction, etc.), temporal resolution, spatial resolution, and time span.

OK, 100% accomplished. We fully agree with this comment: data section has been completely restructured. The suggested columns have been inserted in a new Table 2 in order to provide a thorough answer to the questions above shortlisted by the Reviewer-1. We could not use Table 1 for this purpose as Table 1 has a mandatory format (compliant with the Ocean State Report guidelines) that must be fully respected for final acceptance and publication.

[Comment 3] The time span for the different datasets should be standardized. Sometimes the time period is from 1993 to 2022, while other times it is from 2010 to 2022, or from 2008 to 2022, or even from 2015 or 2011 to 2022. This inconsistency extends throughout the article, including the methods section and various figures. If standardization is not possible due to the different scales analysed, it must be specified why and reference the database being used.

OK, 100% accomplished. We fully understand the Reviewer-1´s confusion at first sight. We have standardized instrumental datasets as shown in the **new Table 2** with the aim of clarifying the situation:

i) **1993-2022** is the time span for both the **MED wave reanalysis and ERA5 reanalysis**.
ii) **2015-2022** is now the time span for **all in situ observational data** (from both the tide-gauge and the coastal buoy). Data section, Methodology section and diverse figures have been updated accordingly. Please, also accept our apologies for the typo “2010-2022” along the entire manuscript, which was meant “2008-2022” in the first version of the manuscript. In the new revised version of the manuscript, the time span employed and cited is always 2015-2022.

[Comment 4] Why is the “wave forecast model” of Puertos del Estado used? Would not it be more consistent to use the same database for atmospheric and oceanic variables (such as ERA5)?

OK, 100% accomplished. For consistency reasons, all those maps (of sea level pressure, wind at 10 m height and significant wave height) covering the regional domain (from Canary Islands to Ireland) are now based on ERA-5 reanalysis. Accordingly, the wave forecast model of Puertos del Estado has been deleted from the table of products used (Table 1 and Table 2).

Notwithstanding, Figure 1b is nowadays based on MED reanalysis outcomes because: i) Reviewer-2 has requested to plot the wave direction in the vicinity of Melilla harbour; ii) MED has higher horizontal resolution than ERA-5 reanalysis (as shown in Table 2) so a larger amount of wave vectors can be plotted; iii) Furthermore, MED reanalysis provides finer details of the SWH field over Melilla harbour area, including the shadow effects at the lee of Ras Taksefi Cape; iv) since the MED wave reanalysis is forced with ERA-5 atmospheric fields (as stated in section 2.2.2), the consistency of this approach is ensured.

[Comment 5] Another deficiency of the manuscript is the lack of consistency in calculating the 99th percentile. The authors use both the annual and monthly 99th percentile, as well as climatology (the average of each of the months, e.g., January, February, etc.), interchangeably, even though these values are statistically different.

OK, 100% accomplished. In order to avoid any confusion, Annex 3b and 3c have been removed so, now, only the monthly 99th percentile is used in the manuscript (Figure 4).

[Comment 6] The methods section should be rewritten and restructured, as the method described as "the percentile method" is essentially the peak over threshold (POT) method. Why was the 99th percentile threshold chosen as a reference instead of other values?

OK, 100% accomplished. The methods section has been reformulated.

[Comment 7] The use of tables is excessive in the manuscript, making it challenging for the reader to follow the narrative. Tables 2 and 3 should be integrated into the introduction section to improve readability. Additionally, Table 4 should be removed, as the results presented there are better visualized in Figures 1 and 3.

OK, 100% accomplished. We fully agree, as the manuscript must be shortened and synthesized to make it compliant with the Special Issue guidelines, this convenient suggestion is more than welcome. The number of Tables has been shortened from 6 to 4. We have integrated all the information gathered in Table 2 and Table 3 into the manuscript body. Table 4 has been rewritten and renamed as Table 2. The new Table 3 has been inserted following the Reviewer-2´s suggestion ([Comment 21]: "*This table (in Figure 1) could be moved to the "Table" section, allowing that more information could be added*"). Finally, all the references to these tables have been updated accordingly.

[Comment 8] The figures should be renumbered according to their order in the manuscript.

OK, 100% accomplished. The figures were revised and corrected.

[Comment 9] A climatic analysis is recommended, including an examination of correlations with different climatic indices influencing the area and an analysis of temporal variability using for example, wavelet-type tools.

Albeit not accomplished, mentioned in the conclusions as future work. We agree with the Reviewer-1 that a climatic analysis along with the exploration of those climatic indices affecting the study would definitively provide added value to the present investigation. Within this framework, a previous study by Morales-Márquez et al. (2020) already reported that in the Mediterranean Sea, the dominant modes are the East Atlantic (EA) and East Atlantic–Western Russia modes. In particular, the interannual variability of extreme waves during wintertime is dominated, to a large extent, by the negative phase of EA, with a larger effect in the western Mediterranean basin. Therefore, it would be great to build upon these previous results (which were derived from a high-resolution global NCEP hindcast) and contrast them against the potential outcomes derived from the use of the 30-year regional wave reanalysis used in our investigation.

However, this ongoing Special Issue of the Ocean State Report is rather restrictive in terms of length (only four figures and 3000 words are allowed) and we are afraid we do not have enough space to compute the suggested analysis that certainly deserves a detailed exploration in the context of a future complementary paper. Since it is important to underline the necessity to conduct this future investigation, a paragraph has been introduced in section 5 (Conclusions):

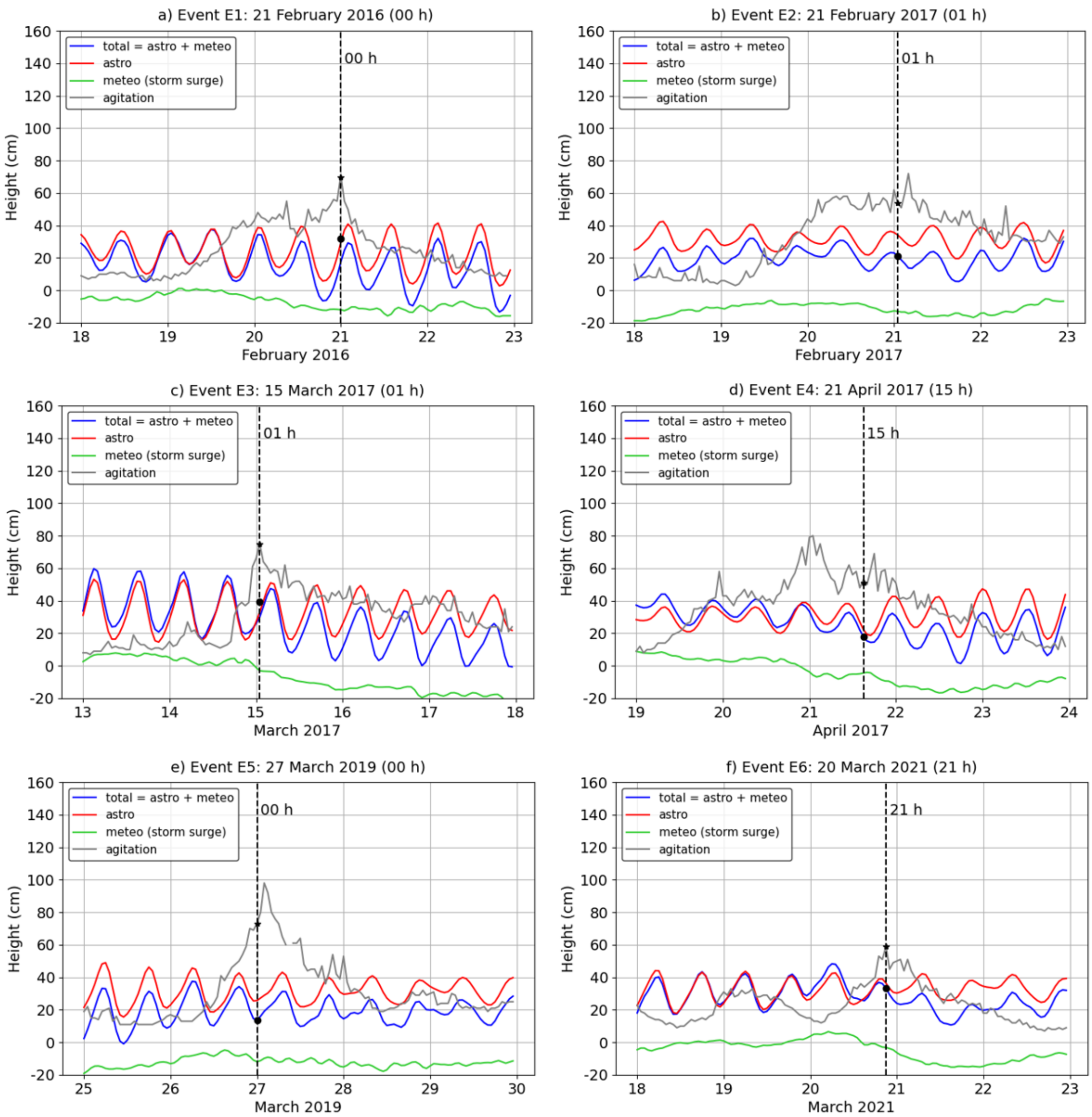
"*Complementarily, additional efforts should be devoted to assessing the dominant modes of extreme waves variability and their relationship with the most important climatic indices. As previously reported by Morales-Márquez et al. (2020), the interannual variability of extreme waves basin during wintertime in the western Mediterranean is dominated by the negative phase of East Atlantic Oscillation. Within this framework, ancillary investigations could enhance the prognostic skills of extreme wave events and benefit the adaptation plans in the entire Spanish harbour system.*"

Reference:

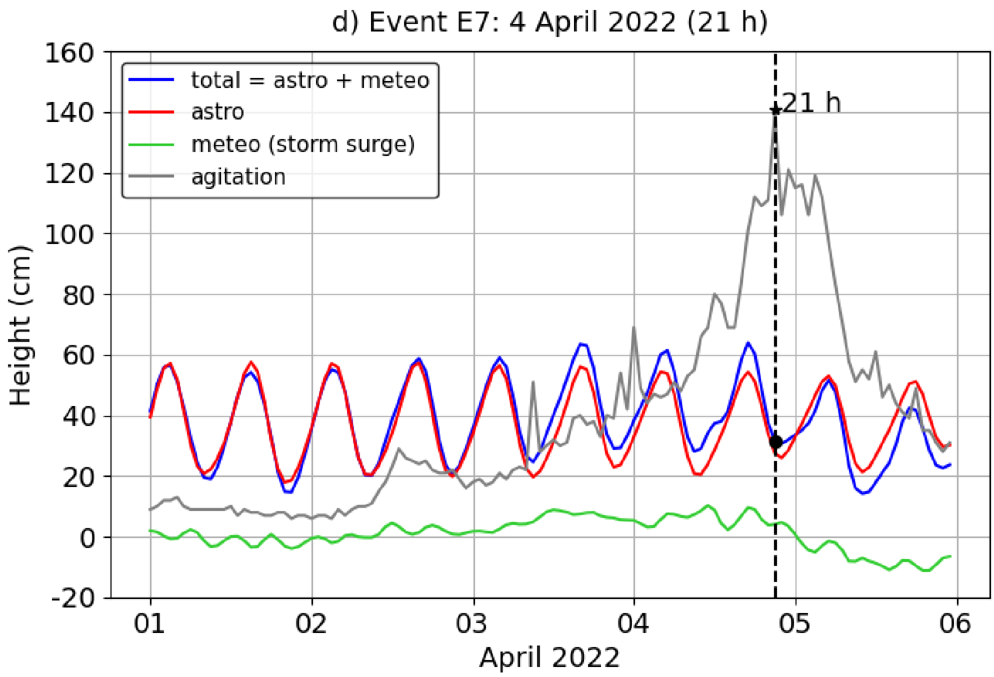
Morales-Márquez, V., Orfila, A., Simarro, G., and Marcos, M.: Extreme waves and climatic patterns of variability in the Eastern North Atlantic and Mediterranean Basins. Ocean Sci., 16, 1385–1398, doi: 10.5194/os-16-1385-2020, 2020.

[Comment 10] The manuscript neglects the value of tides, even though the tidal range in the Mediterranean can reach up to 1 meter. However, it has been proven that the 99th percentile of the IG is 0.28 m, and of the agitation range is 0.38 m, which is within the order of magnitude of tides in the Mediterranean. Therefore, a sensitivity study of the tidal value in the port should be conducted before neglecting this factor.

OK, 100% accomplished. In order to better explain why the impact of both astronomical tides and storm surges on harbour agitation was not considered, we have computed the figure shown below (which is the new Annex 4 in the manuscript). Timeseries of sea level height (blue line) and port agitation (grey line) observations corresponding to the 6 extreme wave events detected before the study case. Observations were provided by Melilla tide-gauge. Astronomical tides and meteorological residuals are represented by the red and green lines, respectively. The vertical dashed black line indicated the peak of the wave storm for each of the 6 events analysed.



Equally, the figure for the E7 event (which is the new Figure 2d in the manuscript):



For the 7 extreme events E1-E7, the following conclusions can be derived:

1) The maximum tidal range observed in Melilla harbour (blue line) is around 40 cm.
2) The surge (green line) due to the storm is negligible for E7 event (below 10 cm), with the meteorological residual being even negative during the six previous episodes (E1-E6).
3) The meteorological residual tends to decrease during the 5-day tome window selected for each event.
4) The evolution of harbour agitation is independent from the tidal phase as the peak of agitation is not coincident with high tides.

Therefore, the paragraph in the manuscript:

"The impact of the last two elements on harbour agitation was not taken into account since: i) the Mediterranean Sea is a microtidal environment with tidal ranges below 1 m (Samper et al., 2022); and ii) the low-pressure core was located in the western side of the Strait of Gibraltar so the storm surge affecting Melilla harbour was negligible (Figure 2, a)."

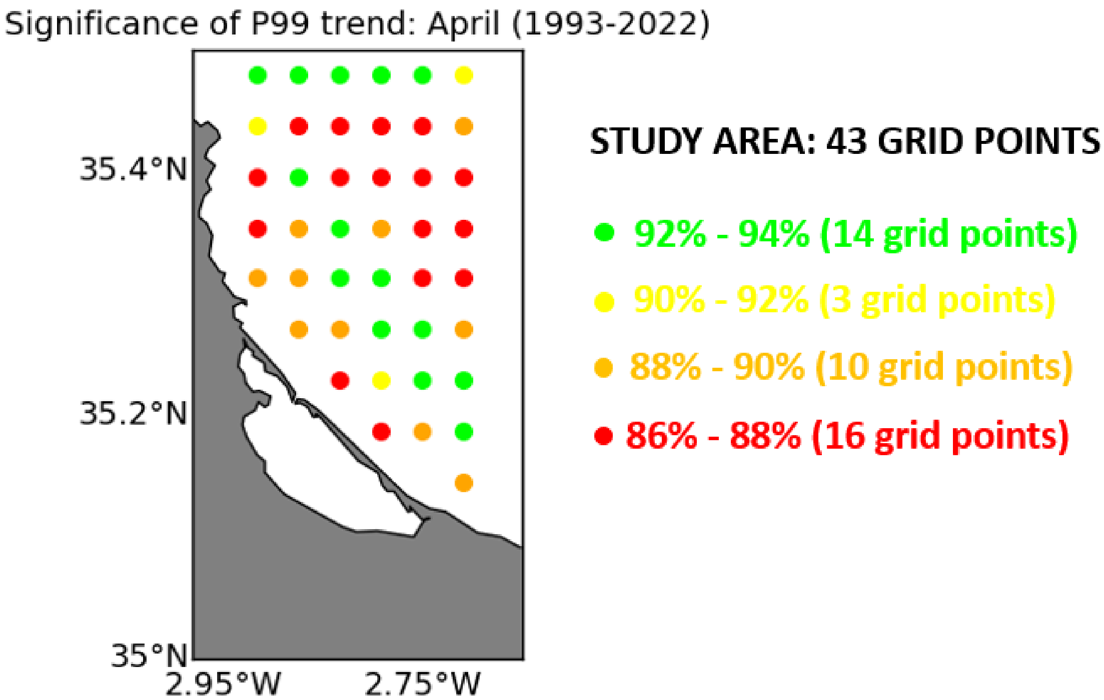
…has been replaced by:

"The impact of the last two elements on harbour agitation during the seven extreme events was not taken into account due to a number of factors, namely: i) Melilla harbour waters are characterized by a maximum tidal range of 40 cm; ii) The evolution of harbour agitation is independent from the tidal phase as the peak of agitation is not coincident with high tides (Figure 2d and Annex 3); iii) the low-pressure core was located in the Gulf of Cadiz (western side of the Strait of Gibraltar, Figure 2a) so the storm surge affecting Melilla harbour was negligible (below 10 cm) for E7 event (Figure 2, d); iv) the meteorological residual was even negative in the rest of previous extreme events analysed (Annex 3)."

[Comment 11] The third major deficiency in the work is the study of extreme event trends in Melilla port. In Figure 4, it can be seen that for the area marked with a black rectangle, most of the pixels do not show a significant trend for April or July (the two months selected for a comparison between P99 and P50 in Annex 4). In my opinion, it cannot be concluded that the regression line is significant based on the time series shown in Figure 4 of the manuscript; the series exhibit too much variability.

OK, 100% accomplished. The statistical significance at the 90% confidence interval was assessed with the Mann-Kendall test (Mann, 1945; Kendall, 1962), in accordance with similar works previously published (Caloiero and Aristodemo, 2021; Barbariol et. al, 2021). While the trends were statistically significant at the 90% confidence interval for April, June, and October, **in the case of July the observed downward trend was only significant at the 80% confidence interval, as already mentioned in the manuscript (section 4.5, line 304)**. In order to clarify this issue and avoid any misunderstanding, we have removed those panels associated with the month of July. Equally, we have removed Annex 4 from the manuscript (following the Reviewer-1´s suggestion) as the maps do not show any significant trend in the vicinity of Melilla harbour.

With regards to the panels associated with the month of April, the Reviewer-1 claims that "***most of the pixels within the black rectangle do not show a significant trend and therefore, it cannot be concluded that the regression line is significant***". As there are indeed 43 grid point (or pixels) inside the selected black rectangle, we show below the confidence interval associated with each grid point:



More specifically, with numbers:

93.7 %	93.6 %	93.4 %	93.7 %	92.3 %	91.3 %
91.7 %	87.8 %	87.9 %	87.9 %	87.9 %	89.6 %
87.3 %	92.6 %	87.8 %	87.7 %	87.9 %	88.0 %
87.5 %	88.4 %	92.8 %	88.8 %	87.8 %	87.5 %
89.2 %	89.6 %	93.1 %	92.3 %	87.7 %	87.9 %
NaN	88.4 %	89.9 %	93.1 %	93.9 %	88.4 %
NaN	NaN	87.5 %	91.0 %	93.6 %	92.3 %

NaN	NaN	NaN	87.9 %	89.9 %	92.9 %
NaN	NaN	NaN	NaN	NaN	89.2 %

Although it is true that there are 26 grid points with a significance interval **below** the 90% and only 17 grid points with a significance interval **above** the 90%, the spatially-averaged confidence interval for the 43 grid point selected is 90.6% and hence the upward trend in the study area can be considered, on average, statistically significant.

With regards to the statement "***the series exhibit too much variability***", we must highlight that, as already specified in the manuscript, trends were calculated using the Sen´s slope estimator of 99th percentile as it was unequivocally proved to be not subject to the influence of extreme values (outliers) and therefore is more consistent than simple linear regression methods (Sen, 1968), as already indicated in the first version of the manuscript.

We guess that perhaps the Reviewer-1 expressed just a subjective opinion based on his/her personal perception after a merely visual inspection. Finally, we would like to highlight that the Reviewer-2 found this approach and the results derived absolutely consistent.

References:

Barbariol, F., Davison, S., Falcieri, F.M.., Ferretti, R., Ricchi, A., Sclavo, M. and Benetazzo, A.: Wind Waves in the Mediterranean Sea: An ERA5 Reanalysis Wind-Based Climatology. Front. Mar. Sci., 8:760614, doi: 10.3389/fmars.2021.760614, 2021.

Caloiero, T. and Aristodemo, F.: Trend Detection of Wave Parameters along the Italian Seas. Water. 13(12):1634. doi:10.3390/w13121634, 2021.

Sen, P.K. Estimates of the regression coefficient based on Kendall's tau. J. Am. Stat. Assoc., 63, 1379–1389, 1968.

[Comment 12] In this work, the analysis of wave height is detailed, while the analysis of wave period is given less attention, even though, for agitation activity, the period is more relevant than the wave height (Eq. 4). This is why in event E7, the agitation is so high compared to the time series, as the period at that time is significantly higher than in the rest of the time series. This fact should be given more emphasis, and the atmospheric conditions that could have caused this remarkable event should be explored.

OK, 100% accomplished. Although we absolutely agree with the Reviewer-1, we also must confess that the specific format of this Special Issue did not provide room for deeper analysis of wave period. In this line of thought, we already described in the conclusions (L370): "long-term historical changes in wave period and directionality are receiving increasing attention and should be further analysed to assess their specific impact on harbours operability".

In order to follow the Reviewer-1´s suggestion and mitigate this shortcoming, several sentences have been introduced along the manuscript with the aim of emphasising the relevant role played by the wave period in the harbour agitation, especially during E7 extreme event:

Section 4.4, L264: "*Finally, it should be noted that although the SWH values registered outside the port during E7 event were the extremely high (7.32 m), the record-breaking harbour agitation (1.4 m) was primarily triggered by the unprecedented wave period (9.3 s), as readily derived from equation [4]. Therefore, it has been evidenced that compound events (i.e., multiple extreme* *events that occur simultaneously or in close sequence) are of particular concern for harbour operability, as their individual effects may interact synergistically."*

Section 5, L380: "*Likewise, offshore wave period also plays a primary role in the modulation of harbour agitation, as derived from equation 4 and the results exposed in Figure 3d. As a consequence, any potential increase in both wave period and SWH could lead to the so-called compound extreme events, which are considered to be a major risk of climate change since they can cause more significant damage than individual extreme events (Velpuri et al., 2023)."*

Finally, with regards to the atmospheric conditions that induced this remarkable event, we humbly believe that they were already explored successfully in Figure 2 (a-b), Annexes 1 and 2 along with the section 4.3 entitled "Driving atmospheric conditions". Therefore, no additional investigations have been provided in the revised version of the manuscript.

[Comment 13] The conclusion section could focus more on how ports need to revise their security protocols based on studies of extremes in the surrounding area, taking into account the analysis of return periods.

OK, 100% accomplished. Although this issue was partially addressed in the last paragraph of the Conclusions section, additional sentences have been added to better underline the need to revise security protocols taking into account the updated return periods:

"Special attention should be focused on the thorough revision of security protocols and the implementation of mitigation plans within the harbour territory based on the updated return periods presented in this work. The design lifetime risk should be recalculated accordingly as coastal structures in the vicinity of the harbour must resist growing stresses during their lifespan and operations, such as wave overtopping, floodings or resonance, to name a few. While the current port layout configuration must be adapted to the increasing frequency and magnitude of these stressors, future maritime facilities at Melilla harbour should be wisely designed and constructed taking into account these outcomes in order to withstand extreme wave regimes imposed by the changing marine environment (Vanem et al., 2019)."

2 SPECIFIC COMMENTS

[Comment 14] L41. Modify the order of the tables according to when they appear in the text.

OK, 100% accomplished. Corrected in the manuscript. Table 1 is nowadays mentioned in the text before Table 2.

[Comment 15] L44. Provide the link to the ECCLIPSE website.

OK, 100% accomplished. The link to ECCLIPSE website was already added in the reference list (line 456), following the journal guidelines. Further details can be found at: https://www.state-of-the-planet.net/submission.html#references, where it is clearly stated that:

- Webpages
  - Title
  - URL
  - Access date

- Year (if not the same as access date)

Example: Copernicus Publications: https://publications.copernicus.org/, last access: 25 October 2018.

[Comment 16] L55. Infragravity waves have a period ranging from 25 seconds to 5 minutes, as indicated by [Munk, 1950].

OK, 100% accomplished. Corrected in the text. Reference added to the list.

[Comment 17] L59. Table 4 could be omitted as it is redundant with figures 1 and 3.

OK, 100% accomplished.

[Comment 18] L60. In the study area, significant wave heights (SWH) exceed 7m, the same order of magnitude than in the Gulf of Lion.

OK, 100% accomplished. The paragraph has been reformulated.

[Comment 19] L110. When does the multi-year wave product reanalysis end and the interim dataset begin?

OK, 100% accomplished. The multi-year wave product of the Mediterranean Sea Waves forecasting system contains a reanalysis dataset (from 1 January 1993 to 31 December 2022) and an interim dataset covering the period after the reanalysis until one month before present (i.e, from 1 January 2023 to 1 October 2023). In the present work, only the reanalysis dataset was used.

[Comment 20] L129. Why if there are buoy data from 2008, do the authors choose to use them only from 2010?

OK, 100% accomplished. Sorry, this was a typo. The data used covered from April 2008 to December 2022. Now the time span has been standardized for all the instrumental datasets: 2015-2022.

[Comment 21] L137. Which spiking method did you use? Were the gaps small enough to ensure that the time series was not totally distorted after processing?

OK, 100% accomplished. Only small gaps (not larger than 6 h) in observational dataset were linearly interpolated. The quality control, defined by the CMEMS in situ team (Copernicus Marine In situ Team 2017), was based on a battery of automatic checks performed in real time to flag and subsequently filter inconsistent values. Some of the tests are listed in the table exposed below (and extracted from Lorente et al., 2019), where the spiking test is succinctly described:

Table 2. Automatic quality-control checks defined by the Copernicus Marine In situ Team and performed in real time to in situ wave measurements.

Check	Description
Global range test	Gross filter based on observed values for waves. It needs to accommodate all of the expected extremes encountered in the study region.
Spike test	Based on the difference between sequential measurements. For the significant wave height, wave period and wave peak period, a value is flagged when the difference exceeds 3 m, 4 s and 15 s, respectively, for the Atlantic ocean.
Stuck value test	A wave parameter should not remain in the same value for more than 12 hours with more than 50% of data not null and valid.
Rate of change in time	Based on the difference between the current value with the previous and next ones.

Obviously, the thresholds used in the spike test for the Mediterranean partially differ from those above exposed for the Atlantic Ocean: for the significant wave height, wave period and wave peak period, a value is flagged when the difference exceeds 3 m, 4 s and 10 s, respectively, for the Mediterranean Sea. An additional sentence has been added to the manuscript to better clarify this.

References:

Copernicus Marine In situ Team. 2017. Copernicus in Situ TAC, Real Time Quality Control for WAVES. Toulouse, France: Copernicus in situ TAC, 1–19. doi:10.13155/46607.

Lorente, P.; Basañez Mercader, A.; Piedracoba, S.; Pérez-Muñuzuri, V.; Montero, P.; Sotillo, M.G.; Álvarez-Fanjul, E. Long-term skill assessment of SeaSonde radar-derived wave parameters in the Galician coast (NW Spain). Int. J. Remote Sens. 2019, 10, 9208–9236

[Comment 22] L140. Pearson correlation coefficient.

OK, 100% accomplished. Specified in the manuscript.

[Comment 23] Eq 2 and 3. Why do you use the sample variance instead of the population variance?

OK, 100% accomplished. Apologies, this was a typo. We obviously used the population variance. The equation has been modified accordingly.

$$\overline{\mathrm{x}} = \frac{1}{\mathrm{N}}\sum_{\mathrm{i}=1}^{\mathrm{N}} \mathrm{x_i} \quad (1)$$

$$\sigma = \sqrt{\frac{1}{\mathrm{N}}\sum_{\mathrm{i}=1}^{\mathrm{N}} (\mathrm{x_i} - \overline{\mathrm{x}})^2} \quad (2)$$

$$\text{Correlation} = \frac{1}{\mathrm{N}}\sum_{\mathrm{i}=1}^{\mathrm{N}} \left(\frac{x_i - \overline{\mathrm{x}}}{\sigma_x}\right)\left(\frac{\mathrm{y_i} - \overline{\mathrm{y}}}{\sigma_{\mathrm{y}}}\right) \quad (3)$$

Update: Reviewer-2 has suggested replacing these three equations by a reference:

"[Comment 11] *Lines 141-143, equations (1),(2),(3) are the well know definitions of mean, standard deviation and correlation. Is it really necessary to introduce them here? Or could you just give a reference of a statistical or methods book/paper*".

Therefore, we have replaced the equations by a reference in the manuscript.

[Comment 24] L155. The correct reference was Stockdon et al. (2006), not Inch et al. (2017).

OK, 100% accomplished. Replaced in the text.

[Comment 25] L160. Specify the data that were used.

OK, 100% accomplished.

[Comment 26] L173. Specify the time span.

OK, 100% accomplished.

[Comment 27] L180. Why do you consider data for wave directions only for the period between 2011 and 2022?

OK, 100% accomplished. As already stated in section 2.4: the Datawell scalar buoy was replaced by a Triaxis buoy able to provide directional information in 2011. Text amended to better clarify it.

[Comment 28] L186. How do you calculate the exceedance threshold and the time between two independent storms?

OK, 100% accomplished. That paragraph has been expanded to better clarify the approach adopted.

• With regards to the exceedance threshold, we followed the approach proposed by Harley (2017) and Fanti et al. (2023) for coastal storm analysis: the most pragmatic approach is to simply set the threshold according to the 95th percentile of the significant wave height dataset.

References:

Harley, M. Coastal storm definition. In Coastal storms: processes and impacts 1–21 (John Wiley & Sons, 2017).

Fanti, V., Ferreira, Ó., Kümmerer, V. et al. Improved estimates of extreme wave conditions in coastal areas from calibrated global reanalyses. Commun Earth Environ 4, 151 (2023). https://doi.org/10.1038/s43247-023-00819-0

• With regards to the time between two independent storms, there is some subjectivity in how a time series is partitioned into separate storms. The broadly accepted criteria used to define independent storms typically state that the time between the wave height peak of two adjacent storms must be larger than some minimum value. Such minimum value in the North Atlantic is usually chosen considering that the average lifetime of extra-tropical cyclones is 3 days (Trigo et al. 1999). For instance, the most intense activity period of Storm Gloria ranged between 20 and 23 January 2020 (Amores et al., 2020). Within this context, Mackay and Johanning (2018a and 2018b) showed that values of storm peak separated by 5 days were effectively independent: "Given these observations, defining storms as local maxima in SWH in a 5-day window appears to be sufficient to ensure independence. In this context, changing the minimum separation affects the isolation of lower peaks which have little influence on the extremes. A separation time of 5 days is also reasonable based on physical arguments, since peaks separated by 5 days will correspond to waves generated from separate low-pressure systems". Therefore, in the present work, storms were defined using a minimum temporal separation of 5 days between adjacent peaks, as suggested by Mackay and Johanning (2018a and 2018b).

References:

Amores, A., Marcos, M., Carrió, D. S., and Gómez-Pujol, L. Coastal impacts of storm gloria over the northwestern mediterranean. Nat. Hazards Earth Syst. Sci. 20, 1955–1968. doi: 10.5194/nhess-20-1955-2020, 2020.

Mackay, E. and Johanning, L. Long-term distributions of individual wave and crest heights, Ocean Eng., 165, 164-183, 10.1016/j.oceaneng.2018.07.047, 2018a.

Mackay, E. and Johanning, L. A generalised equivalent storm model for long-term statistics of ocean waves, Coastal Engineering, 140, 411-428, doi: 10.1016/j.coastaleng.2018.06.001, 2018b.

Trigo, I.F., Davies, T.D. and Bigg, G.R. Objective climatology of cyclones in the Mediterranean region. J Clim 12(6):1685–1696. doi:10.1175/1520-0442(1999)0122.0.CO;2, 1999.

[Comment 29] L233. Could you provide spectra to demonstrate how the infragravity waves dominate the energy during the analysed events?

Not accomplished. We might provide spectra in the next iteration with both reviewers. At the present stage, we have not provided them since: i) the total number of Figures (4 + 5 additional annexes) is already significantly high; ii) Reviewer-2 has not required that ancillary information.

[Comment 30] L235. It is not possible to see all these results in Table 6. Could you display them graphically?

OK, 100% accomplished. All these results are now exposed in Figure 3a (blue line). Furthermore, since the thresholds for port management are also indicated in this panel, Table 6 has been removed from the document to make easier for the reader to follow the narrative.

[Comment 31] L243. Would you mean "20 minute time-series"?

OK, 100% accomplished. Corrected in the manuscript.

[Comment 32] L253. Instead of "the 655 hourly", it would be clearer to mention the time span.

OK, 100% accomplished. Clarified in the text.

[Comment 33] L268. How do you calculated the "monthly P99"? Is it the P99 of all the January data (February, March, etc.)? Or is it the mean value of all the P99 from all the January, February, etc. months?

Yes, the first option: the 99th percentile value for January was computed considering all January hourly data comprised between 1993 and 2022 (green line in Annex 3c) and comprised between 2009 and 2022 (blue line in Annex 3c). Notwithstanding, Annex 3b and 3c have been removed, in line with [Comment 5] from Reviewer-1.

[Comment 34] L313-321. These points should be included within the introduction section.

OK, 100% accomplished. The key points outlined in L313-321 have been also inserted in specific parts of the introduction.

[Comment 35] L336. It is not the "percentile's method", it is the peak over threshold.

OK, 100% accomplished. "Percentile´s method" was replaced by "POT method" in line 328. Line L336 was reformulated accordingly.

[Comment 36] L421. Berta, et al. (2020) should appear after Bensoussan, et al. (2019).

OK, 100% accomplished.

[Comment 37] Annex 3. Adjust all the colorbars, as P99 seems smaller than P50.

OK, 100% accomplished. We guess Reviewer-1 means Annex 4. The colorbar has been modified to solve this issue.

[Comment 38] Annex 5. Consider removing this annex because the most of the pixels show non-significant trend values.

OK, 100% accomplished. Removed from the manuscript! The references to this Figure have been also deleted.

[Comment 39] Walter H Munk. On the wind-driven ocean circulation. Journal of meteorology, 7(2):80–93, 1950.

OK, 100% accomplished. Added to the references list.