

Author's reply to Anonymous Referee's reports on manuscript egusphere-2024-4044

Dear Editor and dear Reviewers,

thank you once again for your time and effort invested in our manuscript. Our replies are given below reviewer's comments, and coloured in blue. The cited line numbers refer to the version of the manuscript with included track changes.

Report #1

The authors have addressed most of my concerns, and I believe the manuscript reads more effectively now. Starting from the original manuscript version, the authors have improved the paper by enhancing the description of the methodology and the discussion of the results, which is essential for the completeness of the paper.

Thank you for your positive comments.

My only remaining concern is about the analysis of the temporal distribution of extremes. At pages 100-101, the authors argue that “the frequent occurrences of extreme SLs in the past decade (IDs 20–27) were primarily driven by the increased number of extreme episodes in synoptic and planetary-scale components”. However, this conclusion is based on a qualitative, and in my opinion erroneous, analysis of figures 74 and 75. Three main findings can be highlighted by analysing the presented results:

- Figure 74 does not show a clear indication of an increase in events in the last two decades, but just the occurrence of two groups of events.

We agree with the reviewer. We have modified the text to account for grouping of episodes in the last two decades. Also, we calculated trends of intensity and frequency of episodes. Please see revised Sect. 5.

- Figure 74 shows that SLR indeed has a role by decreasing the number of events in the last decades.

We agree. The long-term sea-level variability is responsible for three episodes (out of twelve) recognised as extreme events. This is now clearly written.

- Figure 75 shows that no trends and no increase in frequency of intensity can be identified in the synoptic and planetary scale components.

We agree with the reviewer. We have changed the text accordingly.

Therefore, I recommend that the authors reformulate this section of the discussion.

We have revised the text as suggested; please see revised Sect. 5.

My minor suggestions for ameliorating the manuscript are listed here:

- Line 31: accumulated in the northern part of the basin [Done! Please see line 31.](#)
- Line 35: ... Adriatic, other processes ... [Done! Please see line 35.](#)
- I suggest merging the one-paragraph sections 2.2 and 2.3 into a unique “Literature and archival data review” section.

[Done as suggested. We moved the paragraph on literature before the paragraph about archival data to align with the new section title.](#)

Report #2

I am grateful to the Authors for replying to my comments. The answers are good and detailed; nevertheless, some issues remain in the final analysis of the events and the conclusions. See the PDF file.

[Thank you for your comments. We have calculated the trends as suggested and revised the text accordingly. Please see our responses below, marked in blue.](#)

[Comments from referee report: egusphere-2024-4044-referee-report.pdf](#)

Line 40: Ok, very good, just say: we define (refer), since this is your definition.

[Done. Please see line 37.](#)

Line 78: some regions in the eastern... (as you just say the western coast some rows before).

[Reformulated as suggested \(see line 78\). The sentence comparing the western and eastern coasts has been removed, as it contradicts the findings of Toomey et al. \(2022\).](#)

Lines 93–95: Ok, but you cited only barotropic components, even if in the appendix there is more. What about sea-level variations due to density gradients? What is usually caught by satellite SLA?

[We have strengthened the Introduction and Appendix to address this point. Please see our response to your comment on lines 89–90, 2343–2347.](#)

Line 100: You should say that this is a reasonable assumption.

[Done. Please see lines 100–101.](#)

Lines 2123: This figure is a bit misleading, since the time increase of the events from left to right is due to the SLR. But I would not know how to improve it.

[We agree that the increase in the number of occurrences is due to long-term sea-level variability. We have demonstrated this and discussed it in relation to Fig. 74.](#)

Lines 2152: Low pass filter?

Yes, we added it to brackets. Please see lines 2174–2175.

Lines 2155: months (a bit misleading, can be minutes). Maybe you can use days.

Done! 15 m has been changed to days ('456 d') throughout the manuscript and in Fig. 73 and added in Sect. 3.2 that this corresponds to 15 months.

Lines 2189: They seem blue to me.

Thank you! We changed color to grey. Please see the new Fig. 74.

Line 2194–2195: Since you computed them, you could give the SLR values that you found.

Done. We have added the range of (multi)decadal variability and the rate of the linear trend observed at TG Bakar; please see lines 2227–2228.

Line 2197: Why they are not the same of the grey(blue) ones? Explain better.

Done. We have revised the previous text to better explain the approach and clarify why the two sets of extremes differ. Additionally, we renamed 'detrended' extremes to 'adjusted' extremes, as the original term could be misinterpreted to imply that only the linear trend was removed. In fact, all variability on timescales longer than 5 years (that is, (multi)decadal changes and the linear trend) was removed from the measured sea level in order to extract 'adjusted' extremes. Please see lines 2223–2227.

Lines 2201–2202: Actually your statistics is not very robust, the cases are few and this is a qualitative analysis.

Lines 2218–2219: I do not agree. The graph allows only a qualitative analysis, however, making a linear regression from 50's (before you have data missing) to 2024, I guess the trend is not positive for the synoptic component. And probably is not significant even for the planetary component. Moreover, other papers noted that the storminess is decreasing in the Adriatic sea (e.g., <https://doi.org/10.1016/j.pce.2010.10.002>, <https://doi.org/10.1016/j.csr.2015.04.002>). If you assert the contrary you should cite these papers and make a more robust analysis. I suggest to add the linear trends (from 50's) or another quantitative analysis.

We agree with the points which you have raised. We have calculated trends and adjusted the text to reflect this. Please see revised Sect. 5.

Line 2224: You should extend a bit the discussion. You do not say anything about high frequency, tide. And not much about long-trend.

Done. We have added one-sentence summary for each component; please see the revised Conclusion.

Lines 2231–2233: I do not agree, see my previous comment.

Accepted and removed; please see the revised Conclusion.

Line 2236: as before.

We have added comments on all components; please see the revised Conclusion.

Lines 2239–2240: Please, report the IDs of the events.

Done!

Line 2302: Ok, but what about baroclinic circulation? What you see in the satellite SLA?

In the Introduction, we emphasized that *long-term sea-level variability* arises from both barotropic and baroclinic processes. In Appendix A, we addressed baroclinic changes related to BiOS, which act on decadal timescales. Please see the relevant additions in the Introduction (line 91) and Appendix A (lines 2343–2347).

Lines 2306–2307: Give the values that you found.

We clarified this part of the text as presented in Šepić et al. (2022); please see lines 2361–2364.