

Review of “The acceleration of sea-level rise along the coast of the Netherlands started in the 1960s” by Keizer et al., 2022 for Ocean Science

Summary

In this manuscript, Keizer and colleagues use a new statistical method (GAM) to infer sea-level trends and identify acceleration in the time series. Based on six tide gauges along the Dutch coast, they identify that SLR has been accelerating since 1960s, and that this acceleration is masked by wind influence on sea level. Their study highlights not only the importance of including the influence of wind and of the nodal cycle on sea level, but also show how GAM can be used to infer on the rate of sea level in other locations. This is a relevant study, that deserves to be published. I do think some things should be clarified or better presented before final publication.

General Comments

- Periods used: It was unclear to me how the authors chose the periods shown in Figure 3 and Figure 4. For example, in Figure 3b, the trend until 1928 and for 1928-2020. This split doesn't coincide with the splits in Figure 4, and neither with the different periods of the two atmospheric reanalyses used. And in Figure 4, the authors give trends for 4 periods of 20 years, but it was not explained why 20 years, and why those specific divisions also. For example, why no trend over 1959 to 2000? Would be interesting to see the trend of the acceleration.
- I think would be good to add a table to the manuscript, with the trends for the different GAM models and the different periods. Right now, the trends are given in Figure 4, and through some places in the text, but would be easier to follow in a table. And would also be good to know the trend for the entire period (1900-2019) for comparison, and a trend for before the acceleration (before 1960s) and after, not only on 20 years interval.

Line-by-line comments (Specific comments)

- Abstract L4: “covering the period 1890-2000”. I think it was supposed to be until 2019. As it's mentioned in L11 and in the rest of the manuscript.
- Figure 3: It was unclear to me if these are trends of wind, wind influence on sea level (which would be just sea-level trends), or just sea-level trends.
- L16: This line should be part of the previous paragraph (usually abstracts are a single paragraph).
- L22: sea-level rise, with hyphen (to be consistent with the fact that you always used a hyphen when sea level was a modified (e.g., in the same line “sea-level projections”)
- L26: Should add that the contribution of Greenland is much smaller than the globally averaged contribution to the North Sea, it's not the case for most of the Southern Hemisphere (e.g., see Figure 4 of Camargo et al., 2022).
- L50: Should change the order of the verb: “have been used as predictive variables by various authors”

- L80: Would be good to refer here to Figure 1a.
- L80: I was also wondering why you didn't include Maassluis tide gauge, like in Steffelbauer et al (2022).
- L107: (Wood, 2020) not Wood (2020)
- L155: "see Section 2 of Cox and Reid (2004)" should be between parathesis.
- Figure 2: I missed a bit more of discussion about Figure 2, there are only 4 lines about it... Also, why was TrNcPd time series excluded from this figure? I think would be good to show the time series for the 4 GAMs, or at least explain why you decided not to show it.
- L185-186: It was unclear if the authors actually tested if the increased degrees of freedom (DoF) increase the standard error of the trend enough to justify that using only one predictive variable for the wind is better than using two. Table 1 seems to suggest that the increasing the DoF is worth it, since it gives a significant decrease in the deviance. So it seems a bit contradictive then to use the increased DoF as a reason to not use more than once wind variable. I was curious if the TrNcPd model would perform better than with TrNcZw, if you had used both boxes as proxies, as suggested in Dangendorf et al (2014b), instead of taking the difference between the boxes as it was performed here.
- L195-196: Why are your results in contraction with the ones of Dangendorf et al (2014a) Do you mean that they get a negative trend while you have a positive one in Figure 3b? Are you then comparing your trends from 1836-1928 and 1929-2020 with the ones from 1953-2003 and 1900-2011 from Dangendorf et al (2014a)? If that is the case, it's not very consistent to compare trends of such different periods. Then the difference in the results would not be only the results of an update in the atmospheric reanalysis, but also from the fact that you are comparing different periods.
- Section 4.3: Was a bit confusing how you start with Figure 4, discusses Table 2, and then moves back to Figure 4. I would suggest have it clearly separated, to be less confusing for the reader (and maybe moving the paragraph starting in L231 to before L215, to introduce the acceleration in the 1960s).
- L210-212: Would it be possible to give a number when you talk about lower/higher uncertainties here? Maybe the average width of the confidence interval for the time series?
- L212: Refer here to Figure 4f.
- L222-227: You talk here in percentages, and table 2 is in probability. Just a suggestion, but maybe you should have Table 2 in % as well, to be consistent.
- L228-230: This is a very important conclusion of your results. You should emphasize this in the conclusion (sorry if you have done it and I missed it).
- L231: typo: 1960".
- Figure 3 caption: would be good to add to the caption the line colors as well: "(TrNcZw, orange line) ... (TrNcPd, blue line)". Also, when referring to Figure 1, should be Figure 1b.
- Table 2 caption: I guess this is more of a style choice, but I would say that the explanation that starts with the "For example, ..." should be in the main text, and not in the caption of the table.

- L241: “Calafat and Chambers (2013) and Steffelbauer et al (2022).
- L246: “results in Calafat and Chambers (2013) and Steffelbauer et al (2022).” (not between parenthesis).
- L259-262: This is a very long sentence (4 sentences) ... Also, 1 paragraph sentences should be avoided.
- L267: Maybe also good to refer to the work from Dangendorf et al (2021) here?
- L275: Is the 1.5 the rate of the acceleration period (1975-2000)? That is not shown in Figure 4c. Here also would benefit from having a table with the trends for the different periods (as suggesting in General Comment 2). Also the reason why you divided by 25 (I assume is the number of years from 1975-2000) was not super clear, it would be good to have this better explained, as this is a very important application of the results.
- L285-286: This sentence implies that you did test the model using more than one predictive variable for the wind (which was unclear before). If you did test it, I would suggest having these results in the supplementary information, so that the reader can actually see it.

References:

Camargo, C. M. L., Riva, R. E. M., Hermans, T. H. J., & Slaggen, A. B. A. (2022). Trends and uncertainties of mass-driven sea-level change in the satellite altimetry era. *Earth Syst. Dynam. Discuss.*, 13, 1351–1375. Retrieved from <https://esd.copernicus.org/preprints/esd-2021-80/>

Dangendorf, S., Frederikse, T., Chafik, L., Klinck, J. M., Ezer, T., & Hamlington, B. D. (2021). Data-driven reconstruction reveals large-scale ocean circulation control on coastal sea level. *Nature Climate Change*, 11(6), 514–520. <https://doi.org/10.1038/s41558-021-01046-1>