

Interactive comment on “Inter-annual variability of mean sea level and its sensitivity to wind climate in an inter-tidal basin” by Theo Gerkema and Matias Duran-Matute

Anonymous Referee #1

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The authors studied the statistical relationship between the wind and annual mean sea level in the Dutch Wadden Sea. They found that the interannual variability of mean sea level there can be largely explained by the west-east wind component. They also reported that correcting observed values of annual mean sea level for meteorological factors can reduce the margin of error greatly. Findings from this study could help us understanding variations in the regional sea level over the Dutch Wadden Sea. However, there are a number of issues that I feel should be addressed in a revision, as detailed bellow.

Major comments: 1. The authors limited their analysis to the statistical relationship between the wind and annual mean sea level, e.g., simple correlation and regression.

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However, mechanisms for their relationship are also important for people to understand regional sea level change and its possible link with climate change. I suggest the author further explore the mechanistic relationship between the wind and mean sea level in the Dutch Wadden Sea at interannual time scale, to make this study scientifically more interesting. The hydrodynamic model used in Duran-Matute et al. (2014) could be an ideal tool to investigate mechanisms for the statistical relationship reported in the present manuscript.

2. Figure 1 depicts the location of tide-gauge and weather stations. To help readers have a better understanding of the setting, it would be beneficial if the authors can plot the climatological annual mean wind and/or sea level from observation or reanalysis on top of the current map.

3. I suggest the authors describe the data sets used in the present study and methods in more details, so that readers can reproduce results in the manuscript. For example, the temporal resolution and any preprocessing of the sea level data should be provided in Section 2.1. Also, more details should be provided on how the confidence interval and the (effective) sample size is calculated.

4. Figure 5 shows the annual mean wind energy at weather station Vlieland, based on which the authors conclude that the annual mean wind energy for all directions combined stays fairly constant throughout the 20 years. I find this statement unconvincing, at least with current analysis. I suggest the authors replot their Figure 5 with a better (smaller) vertical scale. The current one is misleading. The authors further conclude that “it is not so much the total wind energy that varies between years, but rather the share each of the directions gets from this total” (Page 8, Line 5). This statement is also superficial and unsubstantiated.

5. When conducting multiple regression in Section 3.4, the authors treat the west-east and south-north winds and the annual mean atmospheric pressure as three independent variables. This can be easily examined by calculating their correlation at the

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interannual (or yearly) timescale.

6. With the 20-year data, year 1996 is very different from other years and looks like an outlier in a statistical sense. I wonder how the results (statistics) change if data from 1996 is removed. This practice is useful for checking the robustness of the present findings, especially considering the limited sample size of the current data.

Minor comments: 1. There are other grammatical (e.g., Page 4, Line 9, “100-yrs record”) and spelling mistakes (e.g., Page 1, Line 5, “theannual”) scattered throughout the manuscript and I would suggest scanning the text more thoroughly to rectify these.

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