

# ***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 #3**

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The authors present a comprehensive analysis of wind and pressure effects on mean sea level along the Dutch coastline. They use a regression approach to estimate the effects from a combination of tide gauge records and wind/pressure observations. They demonstrate that wind is the most important factor in explaining the inter-annual variability and that it strongly effects both, the long-term trend itself as well as its uncertainty. The paper is generally very well written and easy to follow, although the topic is no more particular novel. Similar results have been obtained by, for instance, Marcos and Tsimplis (2007, <http://onlinelibrary.wiley.com/doi/10.1029/2007GL030641/full>), Calafat et al. (2013, <http://onlinelibrary.wiley.com/doi/10.1002/grl.50731/full>), Dangen-dorf et al. (2013, 2014, 2015), and Frederikse et al. (2016a, b). What is novel here

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is the use of observational records for winds and pressure. However, before making this to a particular relevant point, I would like to encourage the authors to demonstrate the added value compared to reanalysis data as in earlier works. Hence, I recommend a revision of the ms before being suitable for publication in ESD. Please find below a few more specific comments: 1. The issue of the standard error size was not first mentioned by Zervas (2009), but already discussed in Douglas (1991) and in particular acknowledged for the North Sea including Dutch stations (and the effect of an atmospheric correction) in the already referenced literature. 2. One of your foci is on the important issue of uncertainty of trend estimates. However, I am missing information on how you calculated the uncertainties. Did you calculate them based on uncorrelated noise? If yes, this needs to be corrected in a revised version of the ms. As shown by Dangendorf et al. (2015, <https://www.nature.com/articles/ncomms8849>) and many other studies in the recent years, sea level records show a high degree of autocorrelation, which can only be accurately accounted for by adjusting the degrees of freedom. While traditionally this has been undertaken using a simple AR1 process, Dangendorf et al. (2015) have shown that in case of the North Sea this results in a serious underestimation of the true uncertainty. Hence, I suggest that the authors should at least use an ordinary AR1 process (since they use annual data, see Bos et al. <https://academic.oup.com/gji/article-lookup/doi/10.1093/gji/ggt481>) in describing the uncertainty and discuss the underestimation related to that. 3. One of the novel aspects in the ms is the use of wind and pressure observations. However, I am missing a comparison to equivalents from reanalysis data to demonstrate the real effect. This could be, for instance, made by comparing the amount of explained variability for the different products. I would suggest that the observations should probably show a bit better agreement than reanalysis products. 4. Along with point three I would like to suggest making use of one the available tide+surge model outputs for the North Sea, which should give in general a proper description of the effects of atmospheric forcing. You could for instance compare your estimates using the same reanalysis wind/pressure forcing as in the model and compare the results. 5. Freshwater Dis-

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charge: The authors suggest that once of the missing components could be freshwater discharge. While I agree that this could be one particular factor, this is incomplete. The first factor, which comes into my mind, is the steric effect (including its ocean bottom pressure component), which has been estimated, for instance, by Frederikse et al. (2016, <http://onlinelibrary.wiley.com/doi/10.1002/2016GL070750/pdf>).

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