JOURNAL OF COASTAL AND HYDRAULIC STRUCTURES

Vol. 5, 2025, 42

DISCUSSION ON:
Robust validation of trends and cycles in sea level and tidal amplitude in the Dutch North Sea, JCHS 3(32)

Dewi Le Bars[1], Cees de Valk[2], Iris Keizer[3], André Jüling[4], Roderik van de Wal[5], Sybren Drijfhout[6] and Erwin Lambert[7]

Keywords

Sea level, climate change, statistics, wind, the Netherlands, tide gauges, climate scenarios

1 Introduction

The paper "Robust validation of trends and cycles in sea level and tidal amplitude in the Dutch North Sea" by Hessel G. Voortman published in the Journal of Coastal and Hydraulic Structures reconstructs mean sea level time series from tide gauge data and analyzes these data. It concludes that "Higher-order terms in time, expressing acceleration of the rate of rise, were found to be insignificant". This conclusion appears to contradict a paper that we published recently (Keizer et al. 2023). We show that this discrepancy arises from multiple issues in Voortman (2023): errors in the reconstruction of mean sea level from tide gauge data (section 2.1), an inappropriate framework for testing (section 2.2), errors and suboptimal choices in designing the statistical model (section 2.3). These issues invalidate the conclusions regarding the detection of an acceleration in sea level and exaggerate the mismatch between observations and Intergovernmental Panel on Climate Change's Sixth Assessment Report (IPCC AR6) sea level projections over the period 2015-2022 (section 2.4). Additionally, the author makes a number of false claims about Keizer et al. (2022), the preprint version of Keizer et al. (2023), which we point out and correct here (section 2.5).

[1] dewi.le.bars@knmi.nl, Royal Netherlands Meteorological Institute (KNMI), De Bilt, the Netherlands
[2] cees.de.valk@knmi.nl, Royal Netherlands Meteorological Institute (KNMI), De Bilt, the Netherlands
[3] iris.keizer@knmi.nl, Royal Netherlands Meteorological Institute (KNMI), De Bilt, the Netherlands
[4] andre.juling@knmi.nl, Royal Netherlands Meteorological Institute (KNMI), De Bilt, the Netherlands
[5] R.S.W.vandeWal@uu.nl, Institute for Marine and Atmospheric Research Utrecht and Department of Physical Geography, Utrecht University, Utrecht, the Netherlands
[6] sybren.drijfhout@knmi.nl, Royal Netherlands Meteorological Institute (KNMI), De Bilt, and Institute for Marine and Atmospheric Research Utrecht, Utrecht, the Netherlands
[7] erwin.lambert@knmi.nl, Royal Netherlands Meteorological Institute (KNMI), De Bilt, the Netherlands

Discussion Article. **Submitted:** 29 April 2024. **Reviewed:** 21 June 2024. **Accepted** after double-anonymous review: 2 December 2024. **Published:** 17 March 2025.

DOI: 10.59490/jchs.2025.0042

Cite as: Le Bars, D., de Valk, C., Keizer, I., Jüling, A., van de Wal, R., Drijfhout, S., & Lambert, E. DISCUSSION ON: Robust validation of trends and cycles in sea level and tidal amplitude in the Dutch North Sea. *Journal of Coastal and Hydraulic Structures 5 (42).*

ISSN: 2667-047X online

2 Methodology

2.1 Errors in the reconstruction of mean sea level from tide gauge data

Most studies of annual mean sea level (MSL) along the Dutch coast are based on data from the Permanent Service for Mean Sea Level (PSMSL, Holgate et al. (2013)), derived from annual MSL relative to the Amsterdam Ordonnance Datum or “Normaal Amsterdams Peil” (NAP) supplied by Rijkswaterstaat. These data are derived from tide gauge measurements which involved different sampling methods in different epochs: first at high-water (HW) and low-water (LW) only during daytime, then 6-hourly, 3-hourly, hourly, and finally 10-min sampling; see annex 1 of Hoek Ostende and van Malde (1989). These sampling issues, and particularly the differences between mean tidal level MTL (the mean of HW and LW) and MSL were discussed in Dillingh (2013) and Woodworth (2017), also specifically addressing the data of Rijkswaterstaat. Citing IAPO (1939), Woodworth notes that the Rijkswaterstaat annual MSL estimates were corrected for shallow water effects: "In both cases, it is stated that ‘shallow-water corrections’ were applied to make them comparable to MSL measured at regular intervals through the day. One can only assume that these corrections were made accurately so as to be comparable to MSL as recorded later, and certainly inspection of Dutch MTL/MSL time series shows no obvious decimetric offsets between periods of different measurement." Dillingh (2013) further elaborates on the corrections. Given that the issue had already been studied at the beginning of the 20th century (De Bruyn, 1900), it seems reasonable to assume that corrections to account for the difference between MSL and mean tidal level (MTL) made at the time were sound.

Voortman (2023) correctly pays attention to past changes in sampling as a potential cause of inhomogeneity of the annual MSL time-series but does not appear to be aware of the existing literature on this issue and in particular, of the fact that Rijkswaterstaat corrected its MSL estimates from sparsely sampled tide gauge data to account for tidal asymmetry. Instead of using the PSMSL data, Voortman (2023) derives new annual MSL estimates from the archived original tide gauge data of Rijkswaterstaat. For this purpose, the tidal analysis package UTide (Codiga, 2011), which can process both regularly and irregularly sampled water level measurements, is applied to annual subrecords of the Rijkswaterstaat data to reconstruct the selected astronomical components and MSL for each year.

This method is valid if the tidal analysis of the annual subrecords produces compatible results irrespective of the sampling applied. This assumption is checked by subsampling the densely sampled water level records of the year 2020 and running the tidal analysis. The results in Figure 5 show large deviations of MSL computed from HW and LW data relative to MSL computed from more frequently sampled data: the difference for Delfzijl is 25 cm, and 9 cm for Harlingen. This demonstrates that the tidal analysis method is not valid for deriving MSL from HW and LW data. However, surprisingly, Voortman (2023) still decides to use this method to process the data, despite these findings.

Additional proof that Voortman's tidal analysis method is not suitable to analyze long-term mean sea level trends is visible in Figure 4. The MSL from the tidal analysis shows clear discontinuities in both level and variance between periods of low frequency and periods of high frequency observations. Such discontinuities are not observed in the annual MSL values from PSMSL.

Another error made by Voortman (2023) when reconstructing the MSL record is that the reference level is not constant over time. For the period 2005-2021, the MSL from the arithmetic mean is lower than that from PSMSL (Figure 4). This is most clearly visible for Hoek van Holland and Vlissingen. This discrepancy arises from a change in the NAP reference level at that time described for example in section 2.5 of Dillingh (2013). This change in reference level results in an artificial drop in sea level in 2005 and therefore a reduction of sea level acceleration. For example, for Vlissingen, the drop is 3 cm, as described on the PSMSL webpage (https://psmsl.org/data/obtaining/stations/20.php; last access: 04.04.2024).

A robust reassessment of the corrections applied in the past by Rijkswaterstaat would be an interesting study in itself; yet a study aimed at analyzing long term trends and cycles in mean sea level, as was the purpose of Voortman (2023), should be based on the best available time series, which at present is evidently PSMSL.

Additionally, it is unfortunate that the tidal analysis method is not checked for a 6-hourly sampling, as between 1890 and 1920, many tide gauges were sampling at that frequency (see annex 1 of van den Hoek Ostende and van Malde 1989).

2.2 Testing procedure

Voortman (2023) applies a multiple testing procedure (using the Bonferroni correction for reducing the false discovery rate) to his time-series of annual MSL from 6 tidal stations. However, this framework is not suited to address the question in the paper: is sea level rise on the Dutch coast accelerating?

Multiple testing procedures are designed to avoid false positives when testing multiple hypotheses on a single dataset. Let α be the preset significance level (threshold for the p-value) for a single test. Then even if the n null-hypotheses are all true (so the p-value for each is uniformly distributed on [0,1]), there are on average $n\alpha$ false positives. To reduce the expected number of false positives, the Bonferroni correction reduces the threshold to α/n. This of course also reduces the power of each of the n tests. Voortman (2023) applies this procedure to the n=6 different hypotheses of a constant rate of sea level rise at Vlissingen, at Hoek van Holland, at IJmuiden, etc.

However, for the question of whether the rate of sea level rise on the Dutch coast is constant or increasing, there is only a single hypothesis to be tested, because sea level rise associated with the main drivers of change e.g. thermal expansion, ocean dynamic sea level, glaciers and ice sheets melt, land water storage and glacial isostatic adjustment (which we call “background sea level rise” below) are almost uniform along the Dutch coast (Slangen et al. 2014, Frederikse et al. 2016). For this reason, Keizer et al. (2023) analyze the average of the time-series of annual mean sea level from six stations: any common background sea level rise should contribute as much to this average as to the time-series from the individual stations, but the effects of noise and site-specific bias are reduced by the averaging, so it becomes easier to distinguish a spatially constant change in the rate of sea level rise.

In contrast, the series of tests performed by Voortman (2023) on time-series from individual stations do not test the hypothesis of a constant rate of sea level rise along the Dutch coast, and his conclusion that “Statistical tests show that acceleration of the rate of sea level is not significant up to 2021; the last year in the dataset” is therefore invalid. Instead, the six different hypotheses of “no acceleration at Vlissingen”, “no acceleration at Hoek van Holland”, etc. are treated as if they are different, so (ignoring other errors made in the analysis) the conclusion should have been that no individual time-series except for Den Helder shows a significant acceleration of the assumed parametric form.

Furthermore, these six tests each have low power, because the fluctuations in annual MSL at individual stations are much larger than the fluctuations in the average over six stations considered in Keizer et al (2023). Averaging the six time-series is a simple and effective method to boost power. Furthermore, averaging is also effective to reduce the impact of local bias in the annual MSL values for the individual tidal stations: a local bias is reduced by a factor of six. This is important in view of the impacts of engineering works on MSL (Dillingh, 2013) which Voortman (2023) gives examples of. However, in contrast to Keizer et al (2023), Voortman (2023) does not take any measures to reduce the impacts of these biases on the test outcomes.

Further reductions in power in Voortman (2023) beyond the reductions due to testing on individual time-series and the Bonferroni correction are due to the choice and parameterization of covariates in his regression model (no wind effect included, restrictive parametric shape for background sea level); see section 2.3 below. Low power hampers detection of changes in the rate of sea level rise similarly as low resolution of a telescope hampers the detection of stars. Finding only a small number of stars with such an instrument can evidently not be presented as a scientific finding. Similarly, the lack of significance of deviations from a constant rate of sea level rise reported in Voortman (2023) is not a scientific finding but the expected result of a method which lacks power.

Curiously, Voortman (2023) disqualifies the significant acceleration found for Den Helder for the stated reason that the sea levels at Den Helder and Harlingen were affected by the closure of the Zuiderzee, which is claimed to be the cause of rapid sea level changes observed in the reconstructed MSL time series of Voortman (2023) for these stations in Figure 11. However, these rapid changes are not seen in the PSMSL data (see Figure 4), so they must in fact be an artifact of the processing of the inhomogeneously sampled sea level data (see Section 2.1). This illustrates how faulty data processing and misinterpretation lead Voortman (2023) to draw erroneous conclusions from the test outcomes and shows that the reconstructed MSL time series of Voortman (2023) are unsuitable for the purpose of testing hypotheses about sea level rise.

2.3 Model choices

Wind influence on sea level

In Voortman (2023) the “wind effect” is defined as the difference between the calculated astronomical water level and the observed level at the times of observations, averaged over a year. However, it is not shown that this “wind effect” is the influence of the wind on sea level. At a given time, the difference between the astronomical level (reference level plus sum of astronomical components from the tidal analysis) and the observed level can be ascribed to the deviation of wind setup from its average (plus other terms, e.g. deviation of steric height from average). However, it contains no information about the time-average of the wind setup. If a constant perturbation is applied to the wind setup, this constant is added to every observation as well as to the astronomical level at the time of this observation (through the estimated reference level from the tidal analysis), so the difference between an observed level and the estimated simultaneous astronomical level is still the same as before the perturbation. Therefore, it is not possible to determine a mean wind-forced component from a tidal analysis covering the same period (only deviations from the mean can be estimated), and thus the assertion in Voortman (2023) that the mean wind effect on sea level for a particular year can be determined from the result of a tidal analysis over that same year is incorrect.

This is further illustrated empirically by the small size of deviations (apart from minor almost constant offsets) between Voortman’s annual mean sea level estimates and other estimates (among which PSMSL) over the period since 1950 in Figure 4. Voortman (2023) claims that the MSL obtained from tidal analysis does not contain the wind influence, while the others do, but they are all almost identical over this period. For comparison, a regression of the station-averaged annual mean sea level data on wind data and other covariates in Figure 2 of Keizer et al (2022, 2023) shows considerable interannual fluctuations due to wind in the fitted values, which match fluctuations in the MSL data very closely. The importance of wind in influencing sea level in the North Sea was earlier shown in Dangendorf et al. (2014).

Finally, in section 6.6.2, the author makes repeated claims about how the wind effect should have been calculated in the other studies (and refers to his own previous work). However, no argument is provided to support the method of this paper (see above), which is shown above to be invalid. In contrast, the linear regression on data of wind forcing (amongst other covariates) used by Keizer at al. (2023) is shown to be performing as good as a shallow water model in Stolte et al. (2023).

Long-period sinusoids

The fitting of two long-period sinusoids with different periods (nodal and lunar perigee) instead of one gives additional degrees of freedom. The estimates of the tidal signals for the different stations look quite different. This is an indication that the long-period sinusoids are not only fitting an astronomically forced signal, but also wind-driven and steric effects as well as site-specific unexplained fluctuations (noise). The fact that not even two long-period sinusoids can be estimated consistently from the time-series of individual stations demonstrates clearly that the noise in these series is high, and therefore, the tests performed by Voortman (2023) on them have low power.

Shape of sea level trend

Additionally, the power of the statistical tests is further reduced in Voortman (2023) by restricting the sea level rise to an assumed parametric shape (linear before some starting year, then a cubic polynomial), for which there is no scientific argument. If the assumed shape of the deviation from a straight line fits the data poorly over even part of the record, then its estimated contribution will be small and the residual will be large, hence the null hypothesis of a constant rate of sea level rise will not be rejected. In contrast, Keizer et al (2023) make no such shape assumption and estimates the time-series of background rate of sea level rise non-parametrically.

2.4 Comparison with IPCC AR6 projections

The comparison with the IPCC AR6 sea level projections is surprising. Reporting the uncertainty is key when comparing two uncertain quantities. For example, Figure 4(c) of Keizer et al (2023) includes 2.5 mm/yr and 3.5 mm/yr in the confidence interval for 2020 applicable for all 6 stations. This is lower than the median projected rates from NASA (2023) shown in Figure 12. However, these projections are determined by summation over contributions from different processes each with their own uncertainty estimate. For example, for the SSP1-2.6 scenario for IJmuiden, NASA (2023) gives a very wide 90% range of 1.8-7.7 mm/yr. Potential users of these projections are thus well informed about their high uncertainty, but Voortman (2023) neither includes the NASA (2023) uncertainty information nor provides uncertainty for his own estimates.

2.5 False claims about Keizer et al. 2022

Since the preprint Keizer et al. (2022) of our paper is commented on by Voortman (2023), we respond citing the preprint, but the final version of our paper is now published as Keizer et al. (2023). The differences between Keizer et al. (2022) and Keizer et al. (2023) do not matter for our responses.

p.5: "A recent paper by Keizer et al (2022) also reports an acceleration of relative sea level in the North Sea. That paper starts from the assumption that the global pattern of sea level rise suggested by Frederikse et al (2020) should show up in the North Sea. The lack of acceleration in the observations is explained by suggesting that a trend in the wind effect masks the "real" trend of sea level and thus masks the acceleration. But other assumptions could have been made. Chambers et al (2012) already showed that sea level patterns show regional difference with the North Atlantic deviating from the global pattern. The assumption that the North Sea follows local North Atlantic pattern would have been better."

This paragraph is misleading about the assumption made in Keizer et al. (2022). In Keizer et al. (2022) and Keizer et al. (2023) we argue that given the global sea level rise acceleration and the understanding of its drivers we expect sea level acceleration to also be happening in the Netherlands. This is a hypothesis based on physical understanding of the climate system, but it is not an assumption in our statistical analysis.

p.17: "Woodworth's suggestion to use the equilibrium tide for the nodal cycle is followed by several authors studying sea level in the North Sea (Keizer et al., 2022; Steffelbauer et al.,2022), failing to recognize that the North Sea is a shallow shelf sea."

We did not assume the nodal cycle to follow the equilibrium tide in Keizer et al. (2022). We fit a sinusoidal with unknown phase and amplitude.

In fact, we did not know why the equilibrium tide has a different phase and amplitude than the fitted sinusoidal at the time of writing, but in a paper Bult et al. (2024) argue that it is because of the effect of M2 tidal variations on steric sea level.

p.22: "The starting point of Keizer et al. and of Steffelbauer et al. is that the wind effect needs to be filtered out to reveal the "real" sea level over time. Keizer et al. explicitly state that they expect to find an acceleration but that it is masked by the wind effect."

Here, the author appears to interpret a scientific hypothesis as proof of prejudice. In Keizer et al., we state that we expect an acceleration (a hypothesis based on physical grounds), but nowhere do we write that we expect it to be masked by wind: (partial) masking of the acceleration is a result of our analysis. Furthermore, we do not filter out the wind effect. We estimate the wind effect by incorporating wind (or alternatively, a proxy for wind) explicitly as covariate in the regression model. In other versions of the model, we do not include wind. The results are compared and interpreted, clearly indicating that a proper incorporation of the wind effect significantly reduces the uncertainty in the MSL trend.

p.22: "Keizer et al. finally neglect the meridional wind, explaining that the two wind components are highly correlated and that Frederikse & Gerkema showed the meridional wind to be less important. In the light of my findings regarding that paper, this assumption by Keizer et al. is in serious doubt. And ignoring the meridional wind contradicts empirical facts, with the disastrous storm surge of 1953 as a dramatic example of the importance of meridional wind."

In fact, we reported the results with zonal wind only in Keizer et al. (2022) but we report the results with both zonal and meridional wind in the published version Keizer et al. (2023), with almost the same outcomes and no influence on our conclusion that sea level rise is accelerating in the Netherlands. Furthermore, the influence of wind on sea level during a single storm should not be extrapolated to the influence of wind on the annual mean sea level. For example, the effective wind fetch required to produce a high storm surge is much larger than for a typical annual mean wind-driven local fluctuation of the coastal water level, which gives a different response to wind direction. On longer time scale Ekman dynamics relating wind stress and Coriolis becomes dominant compared to the direct wind stress pressure gradient balance and gives a different response to wind direction.

p.22: "Setting aside the objections against the methods used, it remains unclear how the authors concluded that the wind was masking an acceleration. For an accelerating sea level to show up as a linear trend, it is necessary that the wind effect is equally accelerating with an opposite sign. The results shown in Steffelbauer et al. (2022, fig. 2) and Keizer et al. (2022, fig. 3a) do not reveal that required pattern."

This comment is based on a misinterpretation of the conclusion in Keizer et al, as the masking of the acceleration is clearly shown in Figure 4 to be caused by noise (unexplained fluctuations in the signal) rather than an acceleration of opposite sign. This was previously described in Calafat et al. (2013) and in Haigh et al. (2014). In any statistical test of significance, a reduction of the noise in the signal will make a trend or acceleration easier to detect; if the effect of interannual variation of wind is not explicitly included in the model, it remains unexplained, i.e., noise. There is no need for the wind to be accelerating with an opposing sign. Analogously, if the M2 tidal effects on sea level were not removed by averaging sea level over a year, even the long-term sea level rise along the Netherlands would not be statistically significant.

p.23: "I am not able to fully explain this finding of Keizer et al. but a closer inspection of the code repository of Steffelbauer (2022) gives some insight."

As stated above, contrary to Steffelbauer et al. (2022), we do not use the equilibrium tide to estimate the influence of the nodal cycle on sea level. The author's statement implies a relation between the potential discrepancy in the code of Steffelbauer et al. (2022) and the results of Keizer et al. (2023). These two are completely independent, so this statement falsely implies that the code of Keizer et al. contains inconsistencies.

p.23: "Both Keizer et al. and Steffelbauer et al. appear to statistically compare slopes of trends on two sides of a breakpoint"

This is wrong, we do not consider any breakpoint in Keizer et al. (2022). There is nothing in Keizer et al. (2022) that even suggests this.

3 Conclusions

In this discussion paper, we raise numerous issues with the assumptions and methods of Voortman (2023) invalidating any conclusions about the non-acceleration of sea level rise along the Dutch coast. In section 2.1, we show that the tidal analysis method from Voortman (2023) is not suitable to reconstruct MSL from HW and LW data and that the PSMSL data are corrected for tidal asymmetry and NAP change and at present constitute the most credible dataset of MSL along the Dutch coast. The claims of Voortman (2023) about errors in the annual MSL data from PSMSL due to inadequate sampling are not substantiated and reflect a lack of awareness of the relevant literature. Additionally, the errors in the MSL reconstructions of Voortman (2023) invalidate any conclusion about changes in sea level trend based on this dataset. In section 2.2, we show that the tests performed by Voortman (2023) are not appropriate for testing the hypothesis that there is no acceleration of sea level rise along the Dutch coast, and that not finding a significant acceleration is in fact built into the analysis method. In section 2.3, we show that the assumption of Voortman (2023) that the wind influence on MSL can be computed from the difference between the sea level obtained from tidal analysis and observed sea level is wrong. We argue on theoretical grounds that this method cannot work, and we also show it empirically from the time series comparison in his Figure 4. In section 2.4, we argue that the comparison between sea level rate of change in the

AR6 sea level scenarios and rates derived from the tidal analysis is flawed because of the lack of uncertainty information. Finally, in section 2.5, we rectify a long list of false claims that Voortman (2023) makes about Keizer et al. (2022).

Maintaining a high scientific standard is important for all publications but it is especially important for work that directly influences decisions related to the adaptation to sea level rise in the Netherlands. In view of all the inaccuracies, inappropriate assumptions and methods, invalid conclusions and unfounded claims in Voortman (2023), we are of the opinion that this paper should not be part of the literature on sea level rise, as it may misinform practitioners. Furthermore, we expect correction or removal of all false claims made regarding Keizer et al. (2022, 2023).

Acknowledgements

We are grateful to the Journal of Coastal and Hydraulic Structures for opening the possibility to publish discussion papers in the journal. We would also like to thank the two anonymous reviewers.

Funding

This study was financially supported by KNMI and the knowledge program on sea level rise from the Dutch ministry of infrastructure and water management. AJ, SD and RvdW are supported by the Dutch Polar Program DP4C consortium (NWO). EL was supported by the project HiRISE (grant no. OCENW.GROOT.2019.091, NWO).

Author contributions (CRediT)

D. Le Bars and C. de Valk wrote the initial draft based on input from all co-authors. All co-authors edited the initial draft to finalize the text.

Data access statement

No additional data was used in this discussion paper.

Declaration of interest

The authors report no conflict of interest.

References

Bult, Sterre V., Dewi Le Bars, Ivan D. Haigh, and Theo Gerkema. "The Effect of the 18.6-Year Lunar Nodal Cycle on Steric Sea Level Changes." Geophysical Research Letters 51, no. 8 (2024): e2023GL106563. https://doi.org/10.1029/2023GL106563.

Calafat, F. M., and D. P. Chambers. "Quantifying Recent Acceleration in Sea Level Unrelated to Internal Climate Variability." *Geophysical Research Letters* 40, no. 14 (2013): 3661–66. https://doi.org/10.1002/grl.50731.

Chambers, Don P., Mark A. Merrifield, and R. Steven Nerem. "Is There a 60-Year Oscillation in Global Mean Sea Level?" *Geophysical Research Letters* 39, no. 18 (September 2012). https://doi.org/10.1029/2012GL052885.

Codiga, Daniel L., 2011. Unified Tidal Analysis and Prediction Using the UTide Matlab Functions. Technical Report 2011-01. Graduate Schoolof Oceanography, University of Rhode Island, Narragansett, RI. 59pp. https://www.po.gso.uri.edu/codiga/utide/2011Codiga-UTide-Report.pdf

Dangendorf, Sönke, Francisco M. Calafat, Arne Arns, Thomas Wahl, Ivan D. Haigh, and Jürgen Jensen. "Mean Sea Level Variability in the North Sea: Processes and Implications." Journal of Geophysical Research: Oceans 119, no. 10 (October 2014). https://doi.org/10.1002/2014JC009901.

De Bruyn, H.E. (1900) On the relation of the mean sea level and the height of half-tide. In: Huygens Institute-Royal Netherlands Academy of Arts and Sciences (KNAW), Proceedings, vol 2, 1899–1900. KNAW, Amsterdam, pp 189–196.

Dillingh, Douwe (2013), "Veranderingen in gemiddelde zeeniveaus in de Nederlandse kustwateren", Deltares, https://open.rijkswaterstaat.nl/publish/pages/128513/veranderingen_in_gemiddelde_zeeniveaus_in_de_nederlandse_kustwateren_2013.pdf.

Frederikse, Thomas, Riccardo Riva, Marcel Kleinherenbrink, Yoshihide Wada, Michiel van den Broeke, and Ben Marzeion. "Closing the Sea Level Budget on a Regional Scale: Trends and Variability on the Northwestern

European Continental Shelf.” Geophysical Research Letters 43, no. 20 (October 28, 2016): 10,864-10,872. https://doi.org/10.1002/2016GL070750.

Frederikse, Thomas, Felix Landerer, Lambert Caron, Surendra Adhikari, David Parkes, Vincent W. Humphrey, Sönke Dangendorf, et al. “The Causes of Sea-Level Rise since 1900.” *Nature* 584, no. 7821 (August 2020): 393–97. https://doi.org/10.1038/s41586-020-2591-3.

Holgate, Simon J., Andrew Matthews, Philip L. Woodworth, Lesley J. Rickards, Mark E. Tamisiea, Elizabeth Bradshaw, Peter R. Foden, Kathleen M. Gordon, Svetlana Jevrejeva, and Jeff Pugh. “New Data Systems and Products at the Permanent Service for Mean Sea Level.” Journal of Coastal Research 29, no. 3 (May 2013): 493–504. https://doi.org/10.2112/JCOASTRES-D-12-00175.1.

IAPO (1939) Monthly and annual mean heights of sea level up to and including the year 1936. In: Publication Scientifique No.5. Report of the International Association of Physical Oceanography, Liverpool, p 256. http://www.psmsl.org

Keizer, Iris, Dewi Le Bars, Cees de Valk, André Jüling, Roderik van de Wal, and Sybren Drijfhout. “The Acceleration of Sea-Level Rise along the Coast of the Netherlands Started in the 1960s.” EGUsphere [preprint], https://doi.org/10.5194/egusphere-2022-935, 2022.

Keizer, Iris, Dewi Le Bars, Cees de Valk, André Jüling, Roderik van de Wal, and Sybren Drijfhout. “The Acceleration of Sea-Level Rise along the Coast of the Netherlands Started in the 1960s.” Ocean Science 19, no. 4 (July 6, 2023): 991–1007. https://doi.org/10.5194/os-19-991-2023.

Haigh, Ivan D., Thomas Wahl, Eelco J. Rohling, René M. Price, Charitha B. Pattiaratchi, Francisco M. Calafat, and Sönke Dangendorf. “Timescales for Detecting a Significant Acceleration in Sea Level Rise.” *Nature Communications* 5, no. 1 (May 2014): 3635. https://doi.org/10.1038/ncomms4635.

Hoek Ostende, E.R. van den; J. van Malde (1989): De invloed van de bepalingswijze op de berekende gemiddelde zeestand, nota GWAO-89.006, Rijkswaterstaat, Dienst Getijdewateren, november 1989. https://open.rijkswaterstaat.nl/publish/pages/60493/gwao_89006.pdf

NASA. (2023). Sea level projection tool—NASA. Sea Level Projection Tool - NASA. https://sealevel.nasa.gov/ipcc-ar6-sea-level-projection-tool.

Slangen, Aimée B. A., M. Carson, C. A. Katsman, R. S. W. van de Wal, A. Köhl, L. L. A. Vermeersen, and D. Stammer. “Projecting Twenty-First Century Regional Sea-Level Changes.” Climatic Change 124, no. 1–2 (May 2014): 317–32. https://doi.org/10.1007/s10584-014-1080-9.

Steffelbauer, David B., Riccardo EM Riva, Jos S. Timmermans, Jan H. Kwakkel, and Mark Bakker. "Evidence of regional sea-level rise acceleration for the North Sea." Environmental Research Letters 17, no. 7 (2022): 074002.

Stolte, Willem, Fedor Baart, Sanne Muis, Marc Hijma, Marcel Taal, Dewi Le Bars, and Sybren Drijfhout. “Zeespiegelmonitor 2022,” 2023. https://www.deltares.nl/expertise/publicaties/zeespiegelmonitor-2022.

Voortman, Hessel G. “Robust Validation of Trends and Cycles in Sea Level and Tidal Amplitude in the Dutch North Sea.” Journal of Coastal and Hydraulic Structures 3 (December 28, 2023). https://doi.org/10.59490/jchs.2023.0032.

Woodworth, Philip L. “Differences between Mean Tide Level and Mean Sea Level.” Journal of Geodesy 91, no. 1 (January 1, 2017): 69–90. https://doi.org/10.1007/s00190-016-0938-1

JOURNAL OF COASTAL AND HYDRAULIC STRUCTURES

Vol. 5, 2025, 42

Closure to discussion on: Robust validation of trends and cycles in sea level and tidal amplitude in the Dutch North Sea

Hessel G. Voortman[1]

Abstract

In a discussion paper (Le Bars et al., 2025), seven authors argue that a paper on the analysis of sea level, tides and wind setup in the Dutch North Sea (Voortman, 2023) is flawed. They even suggest that fundamental errors were made and suggest that the paper should be retracted.

In this response I refute all their arguments, showing that many claims are unsubstantiated, the scientific literature is not correctly used, or arguments are used which are given in the same paper that is criticized.

[1]Hessel@HesselVoortman.nl; HVEC, Nijkerk, The Netherlands

Closure to the discussion by Le Bars et al.. **Submitted:** 29 January 2025. **Accepted** after an editorial check: 2 February 2025. **Published**: 17 March 2025.

Keywords

Hydraulic structures, mean sea level, sea level rise, tidal amplitude, harmonic analysis, statistical testing

1 Introduction

Voortman (2023) introduces harmonic analysis as a tool to get insight in temporal changes in Mean Sea Level, tide and wind setup. The method is applied to six locations in the Dutch coastal zone. The method provides crucial information for practitioners for the design of works in the coastal zone. Although tidal analysis is an old and well-established method, Voortman appears to be one of the first to recognize this application of tidal analysis. One of the results of Voortman (2023) is that no (statistically significant) acceleration of the rate of sea level rise is found at the Dutch coast.

In a discussion of Voortman (2023), seven authors argue that the paper contains several fundamental errors and should be retracted (Le Bars et al., 2025). Their claim rests on four groups of arguments:

- The reconstruction of mean sea level using harmonic analysis is flawed
- The statistical testing procedure is incorrect
- The statistical model (regression model) is inappropriate
- The interpretation of the paper by Keizer et al. (2022) is wrong

In this response I will show that all comments by Le Bars et al. are unsubstantiated, rest on reading the scientific literature in reverse *or* are points already addressed in Voortman (2023).

2 Methodology

2.1 Mean sea level derived with tidal analysis of tide gauge data

Le Bars et al. (2025) start with an exposé on the quality and history of the dataset maintained by the Permanent Service for Mean Sea Level (Holgate et al., 2013). Like Le Bars et al. I recognize the value of the databases of PSMSL and acknowledge this in section 2.1 of Voortman (2023). And it is true that most of the studies to date base themselves on the data by PSMSL, as suggested by Le Bars et al. (2025). That does not imply that PSMSL is the *only* relevant data source for the analysis of sea levels.

Le Bars et al. (2025) appear to suggest that the reconstruction of yearly mean sea level by tidal analysis is not valid. Their only argument for that statement appears to be that the method is sensitive to the sampling frequency of the underlying data and that differences may arise from that fact.

The low sampling frequency is a characteristic of the older data in the available observations of sea levels and could indeed have an effect on the outcomes of a tidal analysis. For that reason, this aspect of the method is extensively studied and the results reported in Voortman (2023). For instance, all results are plotted with open symbols and closed symbols, informing the reader of Voortman (2023) when the underlying data was low-frequency (only high and low water levels recorded) or high-frequency (recorded at least eight times a day). Recognizing that the underlying sampling frequency may affect the trends found from that data, Voortman (2023) shows trends for the periods 1890-2022 and 1945-2022 respectively. Finally, Voortman (2023) explains that using data of 1945 and beyond only circumvents the sampling problem and the largest construction works in the Dutch coastal zone. But Voortman (2023) carefully explains that there is no guarantee that all potential problems are circumvented.

In summary, all potential pitfalls that Le Bars et al. (2025) point out are already reported in Voortman (2023) and dealt with to the best ability.

2.2 Choice of the dataset

Temporal averaged sea level is a reconstructed quantity, not an observed one. It needs to be calculated from observations of water levels. One of the contributions of Voortman (2023), is showing that tidal analysis is a method that can be used to derive mean sea level, together with information on the tides and wind setup. When suggesting a new way of analyzing sea level data, it is important to compare the new method with other, established methods. And Voortman (2023) does that by comparing the result of the tidal analysis with the PSMSL data and the temporal average in the same six locations. Differences between the methods are found and Voortman (2023) goes through considerable lengths to find reasons for the deviations.

The existence of deviations between my analysis and the PSMSL data is sufficient for Le Bars et al. (2025) to conclude that my data is of poor quality. They argue that I should have used the data provided by PSMSL (Holgate et al., 2013) because it is supposed to be of better quality. Le Bars et al. (2025) fail to provide arguments for the statement that the PSMSL data is of better quality. Their comment appears to rest on the fact that the PSMSL data is widely used, which is true but not a criterion for superior quality. A second argument is that there are discontinuities in my dataset, specifically at the locations Den Helder and Harlingen in the period 1925-1932. The existence of discontinuities in my data lead Le Bars et al. to the conclusion that tidal analysis is unsuitable for deriving mean sea levels and that the data derived by that process is of poor quality.

But the crucial point here is whether the discontinuities are an artifact of the method or a reality in the field. And it turns out they are the latter. The locations Den Helder and Harlingen are at two sides of by far the largest tidal closure ever undertaken in the Netherlands, the Zuiderzee project. It was constructed in the period 1925-1932, precisely coinciding with profound changes of mean sea level and tidal shape in Den Helder and Harlingen (Figure 1). The effects I find are consistent with the hydrodynamic response of an estuary that suddenly loses a significant portion of its tidal prism (Battjes & Labeur, 2017). The effects are also consistent with contemporary projections (Lorentz, 1926) and with present-day observations (Stuur, 2022; Wang et al., 2018). So the clear drop of mean sea level at these two stations is a real effect that was expected at the time and is observed ever since. Le Bars et al. (2025) therefore draw an erroneous conclusion from the presence of discontinuities in my data.

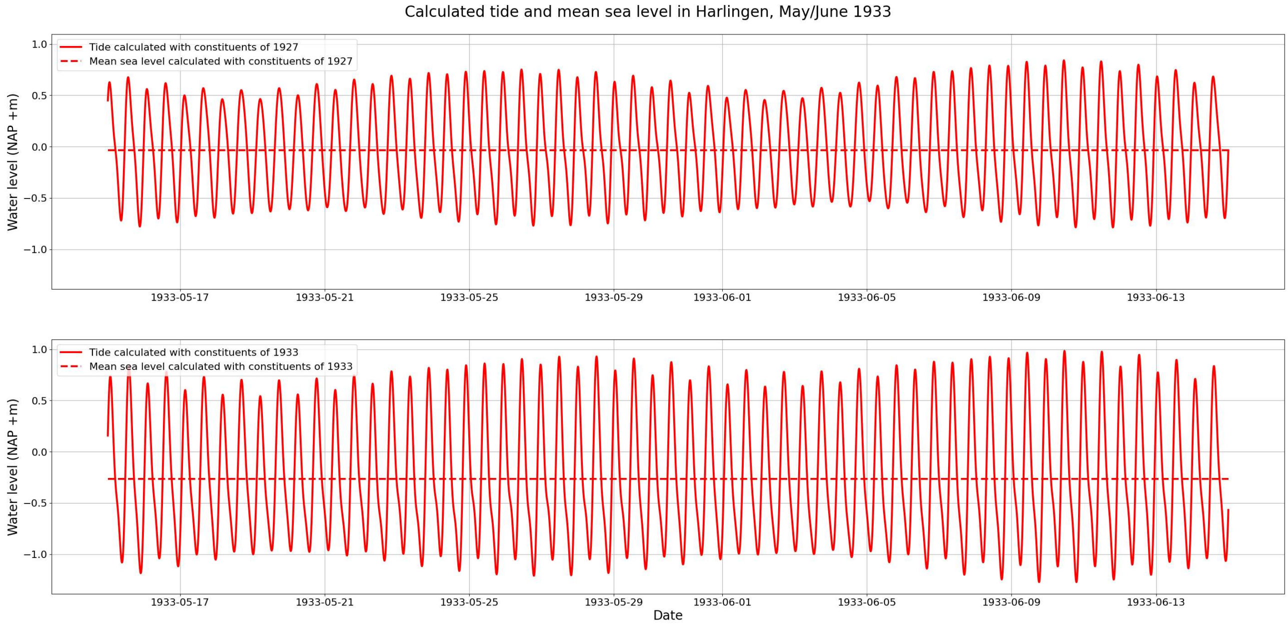

Figure 1: Calculated tide in May/June 1933 in Harlingen, calculated with the tidal constituents prior (1927) and after (1933) the closure of the Zuiderzee. Mean Sea Level dropped 24 cm and the spring tidal amplitude (M2+S2) increased with 40 cm, following the closure, causing a discontinuity in the time series of annual mean sea level.

Voortman (2023) investigates other, less profound changes to the coastline and finds that these may have influenced sea level and tides as well. But because some of these changes coincide with a change in sampling strategy, a clear picture is not found and Voortman (2023) faithfully reports this.

According to Le Bars et al. (2025) I should have used the PSMSL data and not my own, arguing that the PSMSL data is appropriately corrected for the sampling effect. But no evidence is brought forward by Le Bars et al. to substantiate that claim. They reference reports illustrating that the effects are long-known but that does not prove that the corrections were indeed applied to the PSMSL-data. And the sea level at Den Helder and Harlingen described previously suggests that *not* all corrections were applied. The drop of mean sea level in those two locations is real and *not* present in the PSMSL-data.

There are additional reasons for basing my paper on my own analysis of the data provided by Rijkswaterstaat (2022). Again, these reasons can be found in Voortman (2023), so here a short summary should suffice. Voortman (2023) investigates mean sea level, tides *and* wind setup in unison. The PSMSL provides mean sea levels in monthly or annual averages and is therefore not suitable for the analysis of tides. The information on sea level, tides and wind setup could only be found on the basis of the original Rijkswaterstaat data and not on the basis of the processed PSMSL data.

Curiously, Le Bars et al. (2025) propose to investigate the corrections that Rijkswaterstaat applied to the data provided to PSMSL. I fully agree with them on that point. Le Bars et al. also propose to investigate the effect of six-hour sampling in addition to the sampling options already analyzed in Voortman (2023) and this is welcomed as well. But why do Le Bars et al. make these suggestions if their initial claim is that the data stemming from tidal analysis is of poor quality and unusable?

So in summary, Voortman (2023) proposes to use tidal analysis to gain insight in mean sea level, tides and wind setup and appears to be one of the first to do so. The analysis reveals a number of changes in the water levels and tides at the Dutch Coast that are consistent with expectations, both present-day and contemporary. Tidal analysis is therefore a useful addition to the tools available for analyzing observed sea level data. And Le Bars et al. (2025) implicitly acknowledge this by making a few valuable suggestions for further development of this method. Their claim that the PSMSL data is of superior quality remains unsubstantiated.

2.3 Testing procedure

Sea level rise along the Dutch coast is largely driven by the background process of global sea level rise. Thus the observations at the six locations analyzed in Voortman (2023) are dependent. Testing a single process in six different locations constitutes a multiple testing procedure. To avoid a too large probability of finding “false-positives”, the rejection level in a statistical test should be corrected for the fact that multiple tests are made (Stefan & Schönbrodt, 2023). Voortman (2023) chooses the Bonferroni correction (Bland & Altman, 1995; Bonferroni, 1936). Alternatives are available; see for instance Ott & Longnecker (2016).

Le Bars et al. (2025) explain the dependence between the six locations and then turn the argument on its head. By inappropriately suggesting that my six timeseries constitute six independent datasets, they argue that no correction to the rejection level should have been made. This argument of Le Bars et al. is clearly incorrect.

Le Bars et al. (2025) comment that a number of my choices “lower the power” of the testing procedure. And in fact, that is true. The question is not whether my choices lower the power of the testing procedure but whether the choices are appropriate in view of the available data. Auto-correlation (dependence of observations in time) is a fact and Le Bars et al. (2025) and I agree that there is strong spatial dependence between the six locations as well.

Lowering the rejection level in view of spatial dependence (Bonferroni, 1936) and using the *effective* number of datapoints in view of temporal dependence (Bence, 1995), as is done by Voortman (2023) is therefore appropriate. Together, they address the dependencies in space and time present in the data. A lowering of the power of the test is a result that needs to be accepted. The emphasis that Le Bars et al. (2025) place on the *power of the test* is teleological; making the desire to design a high-power test leading over the goal of establishing an appropriate description of the data.

Surprisingly, Le Bars et al. (2025) explain that in Keizer et al. (2022) the uncertainties around the signal were reduced by averaging over the six locations. However, the reduction of uncertainty by averaging is much less or even absent when dependence (commonly referred to as correlation) exists between the six sets, which is the case here. The estimate of uncertainty depends upon the number of *independent* observations in the set and this number reduces in the presence of dependence (Bence, 1995; Voortman, 2023). A smaller *effective* number of observations increases remaining uncertainties. In the presence of dependent data, this cannot be avoided. The reduction of the uncertainties by a factor 6 claimed by Le Bars et al. suggests that dependence was fully ignored. By ignoring dependence, Keizer et al. (2022) overestimate the number of independent observations and thus inappropriately enhance the power of the testing procedure, increasing the risk of falsely detecting an acceleration of the rate of sea level rise.

In section 5.2 of Voortman (2023) I describe my findings regarding the trends of relative mean sea level for all six locations, weighing a few options for the trend analysis and showing the differences. My reluctance to accept the result of Den Helder is misused by Le Bars et al. (2025) to repeat their argument that my analysis is flawed. I covered this topic in the previous section; the drop in mean sea level found in Den Helder and Harlingen was foreseen (Lorentz, 1926) and is consistent with theory (Battjes & Labeur, 2017) and observations (Stuur, 2022; Wang et al., 1995). The discussion by Le Bars et al. fails to provide an objective criterion why PSMSL data should be favored over the data derived from a tidal analysis of the original observations made by Rijkswaterstaat (2022).

So in summary, the design of a statistical test involves a number of choices to be made. The choices made in Voortman (2023) are appropriate in view of the temporal and spatial dependencies present in the used data. The comments of Le Bars et al. (2025) rely on an inappropriate interpretation of Voortman (2023). And in presenting their argument, they reveal additional weaknesses in the work of Keizer et al. (2022).

2.4 Model choices

2.4.1 Wind influence

Any observed water level at sea can be considered a combination of astronomical tide and wind effect (setup or setdown). For the analysis of sea level rise, we aim to find an estimate of Mean Sea Level (MSL) without the effect of wind. Conceptually, there are two ways to arrive at that result:

- Take a time-averaged value of observed sea level, estimate the wind effect using a model and re-analysed wind fields and subtract the calculated wind effect from the observation to find the corrected value of mean sea level.
- Take the available observations of sea level, reconstruct the tidal part of the signal and take the thus found mean level as the corrected value of mean sea level.

The first way is applied in virtually all contemporary research into sea level rise. Mostly using PSMSL as the data source.

The second approach is less frequently used but has a few desirable characteristics. It requires only water level observations and no wind information (observed or re-analysed) and it provides a wind corrected estimate of Mean Sea Level, tidal shape *and* wind setup. A downside is that it does require high-frequency observations. A registration of time and level of high and low water can be considered a minimum, although the estimate of sea level and tides can be influenced by the sampling frequency if the frequency is less than 8 times per day. All this is explained in Voortman (2023) and analyses of sensitivities are shown where appropriate.

The technique of tidal analysis is well-established, with roots dating back to the likes of Newton and Laplace (Doodson, 1921; Pugh & Woodworth, 2014). Nevertheless, the fact that tidal analysis can provide an estimate of mean sea level appears to have gone unrecognized until Voortman (2023). The essence of Voortman (2023) is therefore showing how tidal analysis can be used to generate information on historic sea levels and tides; information that is crucial to practitioners. Voortman (2023) applies well-established methods for the analysis of the tides (Codiga, 2011; Doodson, 1921; Pugh & Woodworth, 2014). Precisely because the method proposed by Voortman (2023) estimates the tidal part of the observed signal, it is possible to construct a time series of the wind effect by simply taking the difference between observed level and calculated tide for every point in the dataset. Once that series is established, the calculation of the annual average is straight-forward.

In their comment, Le Bars et al. (2025) recognize that tidal analysis is a well-established concept. Nevertheless they argue that the resulting estimates of wind setup are flawed. It appears that Le Bars et al. (2025) overlook that the definition of wind setup in Voortman (2023) is in full agreement with well-established definitions (Pugh & Woodworth, 2014), a fact explicitly reported in Voortman (2023). My paper clearly explains that the wind effect is calculated for every datapoint separately, based on the reconstructed tide for a given year and location. Clearly, taking the average to get to the yearly mean wind effect is straightforward and should not be a topic for debate.

The resulting yearly mean wind effect is small and according to Le Bars et al. (2025) this illustrates that the proposal of Voortman (2023) is wrong. But that result is not surprising at all. The wind effect in the North Sea is small most of the time. The yearly average value of the wind setup in the North Sea is determined predominantly by peaks of short duration during westerly and north-westerly storms (Figure 2).

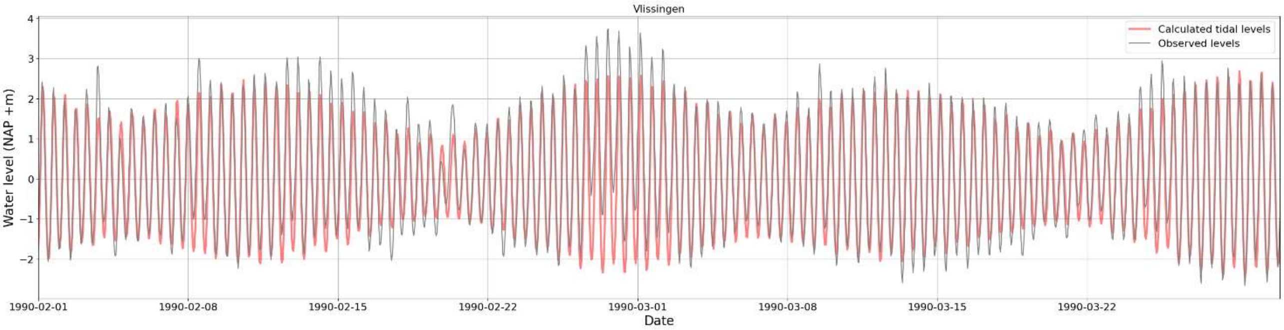

Figure 2: Observed water levels and calculated tidal water levels at Vlissingen in February and March 1990. A storm at the end of February and into early March is clearly visible, illustrating the large magnitude and short duration of the storm-driven wind setup. The difference between tidal level and observed level is small most of the time, leading to a small yearly average of the wind setup.

Averaging a few short peaks wind peaks over a full year clearly leads to a relatively low average value. Le Bars et al. (2025) and Keizer et al. (2022) found large year to year variation of the wind setup, matching fluctuations of Mean Sea Level (MSL) very closely. There being a difference between the results of two studies is no basis for judging one of the two wrong. Moreover, Keizer et al. apply the well-established way of finding mean sea level, estimating the wind effect from re-analysed wind fields and subsequently finding Mean Sea Level by subtracting that wind effect from the observations. Then finding that wind effect and MSL vary in unison is no surprise; it is a feature of the method used by Keizer et al. (2022).

In my paper (Voortman, 2023) I showed the wind correction introduced by Frederikse & Gerkema (2018) to be an erroneous application of an otherwise correct formula presented in Pugh & Woodworth (2014). Since the formula is applied wrongly, any result matching reality must be assumed to be a coincidence. In their comment, Le Bars et al. implicitly acknowledge that they use the method of Frederikse & Gerkema (2018). To illustrate validity, they include a reference to a model study, partly of their own making (Deltares, 2023). As every scientist knows, model quality is judged by comparison to observations, not by comparison to another model that may very well partly rely on the same assumptions as the tested model.

In summary, arguing from an erroneous image of wind setup in the North Sea, Le Bars et al. (2025) argue that the method proposed in Voortman (2023) is wrong. Their arguments fail; the method used in Voortman (2023) is in full agreement with the literature and the resulting small and trendless year-averaged wind setup is readily explained from the fact that wind setup in the North Sea is negligible most of the time, with the yearly value determined by a small number of short storm-driven peaks.

2.4.2 Long-period sinusoids

Voortman (2023) included two long-period sinusoids as a result of my habitual desire for being complete. Considering the global scale of these sinusoids, I expected them to be in phase over the six locations studied. I checked and found them to be in phase, as is reported in section 5.2 of Voortman (2023). The combined shape of the long-period sinusoids is different over the six locations, as rightly observed by Le Bars et al. (2025). This difference in shape is caused by a difference in amplitude over the six locations. The difference in amplitude is not surprising, considering that the surroundings of the six locations differ considerably from each other.

So the claim that “.. not even two long-period sinusoids can be estimated consistently..” made by Le Bars et al. (2025) rests on the erroneous expectation that the *shape* should be the same over the six locations.

2.4.3 Shape of sea level trend

According to Le Bars et al. (2025), there is no scientific argument to be made for my use of a cubic model to describe the sea level over the years. This argument is surprising in a number of ways. First of all, a cubic model is proposed by Katsman, Drijfhout and Hazeleger in a Technical Report published by KNMI (Katsman et al., 2011). A second degree polynomial is used in Deltares (2023) with the same result as Voortman (2023); no statistically significant acceleration is detected. Second-order polynomials are also proposed in Visser (2015). So if the presence of a model in other publications constitutes a scientific argument, there is scientific argument abound for choosing a polynomial of the second or third order to describe mean sea level in time.

The second surprise is that Le Bars et al. (2025) reason from an incorrect image of the workings of regression when they state that the introduction of a third-degree polynomial would lead to a larger value of the summed residuals. Clearly, a linear model is a subset of a third-degree model as can readily be seen when comparing the full and reduced models used in Voortman (2023). If the linear model is appropriate, the regression procedure simply finds the coefficients of the higher-order terms to be (near-)zero. The sum of residuals will always be the smallest possible, as this is the very essence of linear regression. In fact, figure 11 in Voortman (2023) clearly shows that the linear and the cubic model lead to the same fit in IJmuiden and Vlissingen. With the summed residuals being equal for the two fits, the F-test favors the simpler of the two model. This procedure is in accordance with the proposals of Visser (2015) and is clearly explained in section 3.2.2 of Voortman (2023).

Again, the argument made by Le Bars et al. (2025) that my paper is in error stems from an erroneous understanding of Voortman (2023) on their part.

2.5 Comparison with IPCC AR6 projections

The focus of Voortman (2023) is the introduction of tidal analysis to gain insight in the historical development of mean sea level and tidal amplitude. The paper in its present form already contains a lot of information. Uncertainties were not reported for the simple reason of brevity. A comparison based on median rates is perfectly acceptable when comparing models, provided this is adequately reported as is done in Voortman (2023).

2.6 False claims about Keizer et al. 2022

It is unfortunate that the authors feel that I make misleading statements about Keizer et al. (2022). It is my firm conviction that science progresses by an open exchange of ideas and comments and I made several efforts to contact dr. Le Bars and ms. Keizer, well before Voortman (2023) entered the broader scientific process. I responded to the M.Sc.-thesis of ms. Keizer (Keizer, 2022), following an online article by KNMI (2022). The online article presented ms. Keizer's work as a confirmation that in the North Sea the acceleration is masked by the wind effect. That suggestion was made previously by KNMI in the report "Klimaatsignaal'21" (KNMI, 2021). I brought my doubts regarding ms. Keizer's M.Sc-work to the attention of dr. Le Bars and ms. Keizer in an e-mail correspondence (H. G. Voortman, persoonlijke communicatie, 2022). At the time, dr. Le Bars and ms. Keizer apparently did not deem it necessary to correct my interpretation of their work.

In the discussion paper, Le Bars et al. (2025) repeatedly single out sentences from Voortman (2023) to illustrate my erroneous interpretation of Keizer et al. But as stated above, my interpretation of Keizer's M.Sc-work is based on publications by the authors themselves. Futhermore, in the review of Voortman (2023), the reviewers pointed out a few instances where I indeed interpreted the work of others too rashly. These comments were faithfully processed. No such comments regarding Keizer et al. (2022) were made by the reviewers. It appears that in the discussion paper, Le Bars et al. (2025) dial back from the message clearly given earlier (KNMI, 2022). In view of providing correct information to practitioners, one would expect scientists to be consistent in their publications. Regardless whether it is peer-reviewed scientific work or information for the public.

Section 2.5 in Le Bars et al. (2025) repeats a number of comments that are also given in the preceding section of their paper. I consider these points answered above. Two comments in section 2.5 are new. The first concerns their use of the wind correction proposed by Frederikse & Gerkema (2018). By elaborately explaining the effects of meridional and zonal wind, Le Bars et al. (2025) admit that they use the wind correction proposed by Frederikse & Gerkema (2018), omitting my finding that this is an incorrect use of an otherwise correct formula given in Pugh & Woodworth (2014). They even admit that in the preprint of 2022 (Keizer et al., 2022), only the zonal wind was used. So my summary of Keizer et al. (2022) in Voortman (2023) is fully correct. The fact that a different version of the same paper was published *after* Voortman (2023) had entered the review process is immaterial.

In Voortman (2023) I clearly explain that Keizer et al. (2022) fit the long-period sinusoids, after initially attempting the use of the equilibrium tide given by Frederikse et al. (2016). In fact, my paper clearly explains how that finding led me to investigate in detail the equilibrium tide given in Frederikse et al., revealing that the data file provided by Frederikse contains amplitudes of the equilibrium tide that are dramatically smaller than the theoretical estimates (Woodworth, 2012), the empirical estimates (Baart et al., 2012; Deltares, 2023; Keizer et al., 2022; Voortman, 2023) and even the amplitude reported in the paper itself (Frederikse & Gerkema, 2018). Further, Le Bars et al. confirm my finding that Steffelbauer et al. (2022) *did* use the file with the amplitudes of the equilibrium tide of Frederikse et al. (2016) and thereby confirm that my findings regarding Steffelbauer et al. (2022) are correct.

3 Conclusion

Le Bars et al. (2025) challenge my paper in several ways. I thank them for doing so publicly. Only in open and civilized debate is progress to be found. The authors make very strong claims that dominant parts of my method would be invalid and would even warrant retraction of my paper. I consider those conclusions unjustified for the reasons given in the current Closure.

Tidal analysis is a valid method for analyzing sea levels that has a long and well-established history. The method establishes the astronomic part of sea level observations. Provided the time series used is long enough (at least one year), the thus established mean sea levels can be assumed to be corrected for wind effects.

I showed Dutch tides to be severely influenced by construction works along the Dutch coast. I consider the effects I found, such as the sudden drop of the mean sea level in Harlingen, to be real. They are consistent with the tidal dynamics in the region, with contemporary projections and with theory. The absence of these rapid changes in the PSMSL data suggests that these effects were either overlooked *or* corrected in a currently unknown manner.

Voortman (2023) shows openly the problems with the older data, where sampling frequency was low. The paper shows multiple analyses of trends for that reason, to investigate the effect. Part of the comments of Le Bars et al. rely on points of attention brought forward by myself. My paper provides a timeseries of mean sea level of which every step of the analysis is known and documented. This cannot be said about the PSMSL-data.

The authors admit that a thorough analysis of all the corrections that were made in the Dutch part of the PSMSL data would be worthwhile, implicitly admitting that there *may be* relevant errors in that data. To label the PSMSL-data as the most reliable under these circumstances is premature at best. No objective criterion is given by Le Bars et al. *why* the PSMSL-data should be favored, other than an implicit reference to consensus and custom.

The authors appear to be looking for a statistical detection method with high power and make numerous suggestions to increase the power of the method or rather to detect an acceleration. Here our strategies differ fundamentally. I attempted first and foremost to include everything I deemed necessary for an honest analysis of the data and I attempted to report all these choices in the paper. Every choice can be challenged and alternatives can be proposed. But that does not mean my choices are wrong or invalid. Specifically, I deem it necessary to appropriately account for spatial and temporal dependence of the observations. The result is lower power of my method, but not doing so would, in my opinion, constitute unwarranted power with the risk of falsely detecting an acceleration of the rate of sea level rise.

Maintaining a high scientific standard requires an honest debate in public, such as the one we are having now, avoiding unwarranted harsh claims and refraining from attempts to have a paper retracted, openly or otherwise. And if a topic is politically important, such as adaptation to sea level rise in the Netherlands, similar care should be taken in communication with the public. I expect communication about scientific findings to be consistent, irrespective whether it is peer-reviewed or intended for the general public.

No scientific paper is ever the final answer to a problem, and Voortman (2023) is no exception. For instance, the problem with low sampling of the older data was circumvented but not resolved. And of the numerous construction and maintenance works along the Dutch coast, I could only analyze the very clear case of the Zuiderzee closure. But if open research questions are a valid reason to retract a paper, little academic literature would remain.

Acknowledgements

Dr. G.C.M. Gaal and Dr. J. Cleveringa commented on a draft version of this response. I thank them for their effort. I thank the editors of JCHS, Dr. B. Hofland and prof. dr. N. Goseberg for opening JCHS for an exchange such as the one we have now. And I thank again the anonymous reviewers of Voortman (2023) for executing a thorough, at times frustrating but in the end constructive and helpful review.

References

Baart, F., van Gelder, P. H. A. J. M., de Ronde, J., van Koningsveld, M., & Wouters, B. (2012). *The Effect of the 18.6-Year Lunar Nodal Cycle on Regional Sea-Level Rise Estimates*. https://doi.org/10.2112/JCOASTRES-D-11-00169.1

Battjes, J. A., & Labeur, R. J. (2017). *Unsteady Flow in Open Channels*. Cambridge University Press; Cambridge Core. https://doi.org/10.1017/9781316576878

Bence, J. R. (1995). Analysis of short time series: Correcting for autocorrelation. *Ecology*, *76*(2), 628-639. https://doi.org/10.2307/1941218

Bland, J. M., & Altman, D. G. (1995). Statistics notes: Multiple significance tests: the Bonferroni method. *BMJ*, *310*(6973), 170-170. https://doi.org/10/fxf4gm

Bonferroni, C. (1936). Teoria statistica delle classi e calcolo delle probabilita. *Pubblicazioni del R Istituto Superiore di Scienze Economiche e Commericiali di Firenze*, *8*, 3-62.

Codiga, D. (2011). *Unified tidal analysis and prediction using the UTide Matlab functions*. https://doi.org/10.13140/RG.2.1.3761.2008

Deltares. (2023). *Zeespiegelmonitor 2022*.

Doodson, A. T. (1921). The harmonic development of the tide-generating potential. *Proceedings of the Royal Society of London. Series A, Containing Papers of a Mathematical and Physical Character*, *100*(704), 305-329. https://doi.org/10.1098/rspa.1921.0088

Frederikse, T., & Gerkema, T. (2018). Multi-decadal variability in seasonal mean sea level along the North Sea coast. *Ocean Science*, *14*(6), 1491-1501. https://doi.org/10.5194/os-14-1491-2018

Frederikse, T., Riva, R. E. M., Kleinherenbrink, M., Wada, Y., Van den Broeke, M. R., & Marzeion, B. (2016). Closing the sea level budget on a regional scale: Trends and variability on the Northwestern European continental shelf. *Geophysical Research Letters*, *43*(20), 10,864-10,872. https://doi.org/10.1002/2016GL070750

Holgate, S. J., Matthews, A., Woodworth, P. L., Rickards, L. J., Tamisiea, M. E., Bradshaw, E., Foden, P. R., Gordon, K. M., Jevrejeva, S., & Pugh, J. (2013). New data systems and products at the permanent service for mean sea level. *Journal of Coastal Research*, *29*(3), 493-504.

Katsman, C., Drijfhout, S., & Hazeleger, W. (2011). *Tijdsafhankelijke klimaatscenario's voor zeespiegelstijging aan de Nederlandse kust*.

Keizer, I. (2022). *Long-Term Wind Influence on Sea-Level Change Along the Dutch Coast*.

Keizer, I., Le Bars, D., De Valk, C., Jüling, A., Van de Wal, R. S. W., & Drijfhout, S. S. (2022). The acceleration of sea-level rise along the coast of the Netherlands started in the 1960s. *EGUsphere*, 1-21.

KNMI. (2021). *KNMI Klimaatsignaal '21—Hoe het klimaat in Nederland snel verandert*.

KNMI. (2022). *KNMI - Zeespiegel Nederlandse kust stijgt nu sneller door klimaatverandering*. https://www.knmi.nl/over-het-knmi/nieuws/zeespiegel-nederlandse-kust-stijgt-sneller-door-klimaatverandering

Le Bars, D., De Valk, C., Keizer, I., Jüling, A., Van De Wal, R. S. W., Drijfhout, S., & Lambert, E. (2025). Discussion on: Robust validation of trends and cycles in sea level and tidal amplitude in the Dutch North Sea, JCHS 3 (32). *Journal of Coastal and Hydraulic Structures*, *4*.

Lorentz, H. A. (1926). *Verslag van de commissie Lorentz (gevolgen afsluiting Zuiderzee op het getij)*. Algemene Landsdrukkerij.

Ott, L., & Longnecker, M. (2016). *An introduction to statistical methods & data analysis* (Seventh edition). Cengage Learning.

Pugh, D., & Woodworth, P. L. (2014). *Sea-Level Science: Understanding Tides, Surges, Tsunamis and Mean Sea-Level Changes*. Cambridge University Press; Cambridge Core. https://doi.org/10.1017/CBO9781139235778

Rijkswaterstaat. (2022). *Waterinfo*. waterinfo.rws.nl

Stefan, A. M., & Schönbrodt, F. D. (2023). Big little lies: A compendium and simulation of p-hacking strategies. *Royal Society Open Science*, *10*(2), 220346. https://doi.org/10/gs8ntm

Steffelbauer, D. B., Riva, R. E. M., Timmermans, J. S., Kwakkel, J. H., & Bakker, M. (2022). Evidence of regional sea-level rise acceleration for the North Sea. *Environmental Research Letters*, *17*(7), 074002. https://doi.org/10.1088/1748-9326/ac753a

Stuur, L. (2022). *Verkenning morfologische ontwikkeling westelijke Waddenzee*. Deltares.

Visser, H., Dangendorf, S., & Petersen, A. C. (2015). A review of trend models applied to sea level data with reference to the "acceleration-deceleration debate". *Journal of Geophysical Research: Oceans*, *120*(6), 3873-3895. https://doi.org/10.1002/2015JC010716

Voortman, H. G. (2022). *Mail exchange with mr. Le Bars and ms. Keizer, may 2022* [Persoonlijke communicatie].

Voortman, H. G. (2023). Robust validation of trends and cycles in sea level and tidal amplitude in the Dutch North Sea. *Journal of Coastal and Hydraulic Structures*, *3*. https://doi.org/10.59490/jchs.2023.0032

Wang, Z. B., Elias, E. P. L., Van der Spek, A. J. F., & Lodder, Q. J. (2018). Sediment budget and morphological development of the Dutch Wadden Sea: Impact of accelerated sea-level rise and subsidence until 2100. *Netherlands Journal of Geosciences*, *97*(3), 183-214. https://doi.org/10.1017/njg.2018.8

Wang, Z. B., Louters, T., & de Vriend, H. J. (1995). Morphodynamic modelling for a tidal inlet in the Wadden Sea. *Marine geology*, *126*(1-4), 289-300. https://doi.org/10.1016/0025-3227(95)00083-B

Woodworth, P. L. (2012). A note on the nodal tide in sea level records. *Journal of Coastal Research*, *28*(2), 316-323. https://doi.org/10.2112/JCOASTRES-D-11A-00023.1