



EGUsphere, community comment CC1
<https://doi.org/10.5194/egusphere-2022-935-CC1>, 2022
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Comment on egusphere-2022-935 (#1)

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Community comment on "The acceleration of sea-level rise along the coast of the Netherlands started in the 1960s" by Iris Keizer et al., EGU sphere,
<https://doi.org/10.5194/egusphere-2022-935-CC1>, 2022

Summary

I have read the manuscript with great interest.

With reference to the **review criteria of OS**, the work proposed well-circumscribed and substantial conclusions on debated topics (sea level rise acceleration in a convoluted stretch of a shallow sea) obtained with powerful statistical methods (separation of a time-varying signal into a time-varying influences of predictive variables and a smooth time-varying background residual). However, while the line of reasoning is clear in its broad terms, the assumptions/methods are often outlined unclearly, whereby assessing their validity and their linkage to the results proves awkward.

In terms of **personal reading experience**, I found it difficult to get engrossed and isolate with agility what merits a focussed discussion on content and correctness. A certain draft-like, think-aloud quality still hampers speed reading and in-depth consideration. I am nonetheless convinced that a few of rounds of streamlining and paraphrasing will drastically improve the manuscript. Linguistic points of coherence (organization in sections and paragraphs) and clarity (at the level of sentences) should be addressed thoroughly to let the content shine its own light.

I give below suggestions for samples of passages where my reading flow froze. I surely invite the authors to apply those suggestions anywhere they deem it beneficial. (Equally surely, the authors do well to cross-check my suggestions one with another and with other peers.) This document also address some point of content, whether or not entangled with reading difficulties.

I have read the manuscript in full a couple of times, but will restrict my contribution to the text within Section 3.1 included. I have not looked into the supplementary material (the code), but I would expect from the authors that the documentation follows standard of clarity adequate for reproducibility.

[1] Abstract 2-4

This is an extended remark on clarity (speed-reading). My ensuing suggestions will be more succinct.

The kick-off sentence:

While a global acceleration of sea-level rise (SLR) during the 20th century is now established, locally acceleration is more difficult to detect because additional processes play a role which sometimes mask the acceleration. Here we study the rate of SLR along the coast of the Netherlands from six tide gauge records, covering the period 1890-2000. (55 words, 349 characters)

reads more easily in the form:

While the global acceleration of the sea-level rise (SLR) during the 20th century has already been established, additional processes play a masking role that makes it difficult to detect local acceleration. Here we study the rate of SLR along the coast of the Netherlands using the records of six tide gauges in the period 1890-2000.

which exposes the question of what that 'additional' feature is supposed to be added to; there is nothing before the first sentence.

Then, may I suggest:

While the global acceleration of the sea-level rise (SLR) during the 20th century has already been ascertained, the masking by several processes makes detecting local acceleration difficult. Here, we investigate the rate of SLR along the coast of the Netherlands using the records of six tide gauges in the period 1890-2000.

which exposes the question of which processes the writer has in mind.

Then, may I suggest:

While the global acceleration of the sea-level rise (SLR) during the 20th century has already been ascertained, the masking caused by several drivers hampers detecting local accelerations of SLR. Here, the records of six tide gauges in the period 1890-2000 are used to analyse the rate of SLR along the coast of the Netherlands.

or

The global acceleration of sea-level rise (SLR) during the 20th century has already been ascertained. Nonetheless, several drivers of SLR mask the detection of its local acceleration. Here, the records of six tide gauges in the period 1890-2000 are analysed to investigate the rate of SLR along the coast of the Netherlands. (51 words)

You may deviate from any suggestions, of course, and devise better ones. The benefit in sight is that these last few formulations create an anticipation as to which data and factors are considered in the subsequent analysis.

[2] Abstract 12-15

I found this awkward to read and hence to assess:

If, apart from tidal, wind effects and fluctuations, sea level would have increased at a constant rate, then the probability (the p-value) of finding a rate difference between 1940-1959 and 2000-2019 of at least our estimate is smaller than 10⁻⁵. Our findings can be interpreted as an unequivocal sign of the acceleration of current SLR along the Dutch coast since the 1960s. This aligns with global SLR observations and expectations based on a physical understanding of SLR related to global warming.

Do you mean something like this?

If we discount for the effects of tide and of wind fluctuations and then assume a constant SLR rate, the probability that the SLR rates in the periods 1940-1959 and 2000-2019 differ as much as our estimate or more is smaller than 1 (p-value). This result indicates unequivocally that the SLR along the Dutch coast has been accelerating since the 1960s. Global observations and the expectations based on the physics of global warming are consistent with this finding.

Then the validity of the statement is hopefully more accessible.

[3] Introduction 28-31

This is a point of clarity (speed-reading). I found this cumbersome to read:

More recently, Walker et al. (2022) estimated the time when the rate of global SLR emerged from the background variability of the Common Era (0-2000CE) to the middle of the 19th century. For the North-East Atlantic, they found this emergence to occur around the middle of the 20th century. This is in line with Dangendorf et al. (2019) who found a global acceleration of SLR from the 1960s.

May I suggest:

Dangendorf et al. (2019) had found the global rate of SLR to accelerate from the 1960s. More recently, Walker et al. (2022) estimated that the rates of SLR emerged from the background variability of the Common Era (0-2000 CE) in the middle of the 19th century for the globe and in the middle of the 20th century for the North-East Atlantic.

One benefit for speed reading is that new information appears in the same order of publication of the articles. So one appreciates the progress in the field without backtracking through the text.

[4] Introduction 32-33

This is a point of clarity (speed-reading). Re:

Along the coast of the Netherlands, there has been an ongoing debate about whether an acceleration of SLR takes place or not (Baart et al., 2011; Wahl et al., 2013; Steffelbauer et al., 2022).

While I appreciate the natural word order in Dutch, I would suggest streamlining the sentence into:

The existence of SLR acceleration along the coast of the Netherlands is still debated (Baart et al., 2011; Wahl et al., 2013; Steffelbauer et al., 2022).

Something similar occurs in lines 58-60.

[5] Introduction 47-50

This is a point of clarity (speed-reading). Re:

Understanding and removing the interannual to multidecadal sources of variability from tide gauge records was found to be essential for detecting an acceleration of SLR (Haigh et al., 2014). To this end, multilinear regression models between sea levels and atmospheric variables like sea-level pressure gradients, zonal and meridional wind velocity and

sometimes precipitation as predictive variables have been used by various authors.

may I suggest something like:

Detecting the SLR acceleration requires understanding the sources of interannual-to-multidecadal variability and removing them from tide gauge records (Haigh et al., 2014). To this end, various authors have used multilinear regression models between sea levels and atmospheric variables. The pressure gradients at the sea surface, the zonal and meridional wind velocities and, at times, precipitation are usual predictive variables.

[6] Introduction 56-57

The verb tenses in the sentences

In general, the variability due to atmospheric forcing was first estimated by linearly detrending the time series. After that, the variability is removed from the sea level data before estimating the trend and acceleration.

confuse the extent to which old/new practices may be better/worse. (Place this remark in the context of the paragraph contrasting known methods.)

[7] Introduction 61-62

This is a point of clarity (making key figures stand out). Re:

The calculated rate of mean SLR of the stations increases from 1.7 ± 0.3 mm/yr before the breakpoint to 2.7 ± 0.4 mm/yr after the breakpoint implying an acceleration.

I suggest:

The average SLR rate of the stations increases at the breakpoint from 1.7 ± 0.3 to 2.7 ± 0.4 mm/yr, which implies an acceleration of the SLR.

[8] 2.1 Tide Gauge Observations 80

Regarding

Annual-mean sea-level measurements are used from the six reference tide gauges along the coast of the Netherlands: Delfzijl, Den Helder, Harlingen, IJmuiden, Hoek van Holland and Vlissingen.

is the signal that you study the average of these six stations? I inferred this from the caption of Figures 1, 2, 3 and I couldn't seem to find this (important) piece of information in the body of the manuscript. This is the place to (re-)state this, and also an appropriate place to refer to Figure 1a.

[9a-c] 2.1 Tide Gauge Observations 81-82

This is a remark on content. In the sentence

These stations are used for operational sea level monitoring because of their extended temporal coverage and uniform distribution along the Dutch coast (Baart et al., 2019).

the term 'uniform distribution' sounds like a phrase borrowed from statistics. Regardless, the attribution of uniformity does not sound applicable in several respects:

a. The attribution of uniformity is geographically uninformative. To gain a feel of the geographical scope of the study, mentioning the approximate span length of the Dutch coast covered by these stations might be more informative.

b. From the point of view of physical oceanography, the station **Vlissingen** is distinctive insofar as is placed at the mouth of the Western Scheldt Estuary. It is conceivable that morphological developments in the estuary since 1890, of both natural and human origin, have affected the gauge signals in that area. Likewise, station **Delfzijl** is placed inside the Ems-Dollard estuary, whose tidal regime has been heavily affected by capital dredging in recent decades. (I do not have literature at hand to substantiate these claims, but both estuaries have been investigated extensively. I might collect some literature at a later point.) This consideration also implies that a specific factor is at play for these two stations and not for the others. Given your interest for shallow seas, you ought to include your view as to whether using the average of all stations as a study variable will make good any objective diversity between the stations; in other words, whether what you determine for "the Netherlands" applies to each of the selected stations. A precise answer might be found in previous studies or require further study. Nonetheless, stating proactively to which extent the objective geographical non-uniformity might affect the scope of the conclusions will strengthen the manuscript.

c. Likewise, the station **Den Helder** is placed at a tidal inlet of the Wadden Sea and **Harlingen** is inside. Similar morphological influences from the back basin may be conjectured after the closure of the Afsluitdijk in 1932. (I do not have literature at hand to substantiate this claim, but the topic has been investigated. I might collect some literature at a later point.) The same notes of caution as in point b apply, though.

[10] 2.1 Tide Gauge Observations 84-88

This is a point of linguistic clarity. Hoping to have brought forward a sufficient number of ex/samples, I will refrain from commenting on further passages that prompt similar linguistic editing.

The passage:

While the time series for the different stations start between 1862 and 1872, only 1890 to 2020 are used for the analysis. As was done for other studies, tide gauge data is limited to the period after 1890 to avoid the inclusion of a sea-level jump around 1885 (Frederikse and Gerkema, 2018; Baart et al., 2019). From that year, the monthly mean sea level is based on mean sea-level readings rather than mean tide level readings, which could result in a jump in the monthly data (Woodworth, 2017). (88 words)

could become:

The time series at these stations start between 1862 and 1872. However, the monthly mean sea level before 1885 is gauged with respect to readings of the mean tide rather than of the mean sea level. This could result in a jump in the data (Woodworth, 2017). Therefore, only the tide gauge data after 1890 are used as done in other studies (Frederikse and Gerkema, 2018; Baart et al., 2019).

If correct, this paraphrase exposes the fact that the 'monthly mean sea level' had not been based on the 'mean sea level' in a certain past. So why is the former called 'mean sea level' in the first place? The bare point seems to be that the mean sea level could only have been determined since 1885. If this too is a correct reading, the passage could become:

The readings at these stations start between 1862 and 1872 and are gauged with respect

to the mean sea level, rather than to the mean tide, since 1885. Therefore, we only use the tide gauge data after 1890 as did Frederikse and Gerkema, 2018 and Baart et al., 2019. Failure to do so introduces an artificial jump in the data (Woodworth, 2017). (61 words)

[11] 2.1 Tide Gauge Observations 84-88

The fact that the global database did not account for the change of datum of 1855 raises a slight concern that other biases of local origin may have crept in. For example, the instruments at the tidal stations have obviously been changed since 1890. There, I would expect that all instruments have been thoroughly recalibrated at source, but their accuracy will have varied.

Therefore, it would be very fine to read in the manuscript whether you expect that the dataset of choice contains other biases linked to the history of the tide recordings in the Netherlands. Any potential bias in the model input in the the periods 1940-1959 and 2000-2019 seems particularly relevant for the conclusions of this study. These potential biases should be named. The Discussion should also assert whether those biases, however speculative and/or undetermined, tend to strengthen or weaken the interpretation and hence the conclusions.

[12a-b] 2.2 Atmospheric Reanalysis (and Appendix A)

a. This section does not explain for which purpose and in which way you use the reanalyses. For example, the ERA5 has a "backward extension to 1950". How do you go about this? Do you consider it fit for your purposes? Waiting does not pay off either, for the subsequent section Methods does not dwell on how you pre-processed the model input either. Therefore, an early explanation of all pre-processing choices would be beneficial before diving into the Statistical Methods – either in an expanded § 2.2 or in a new § 3.1.

b. I feel that **Appendix A** should be either blended in the body of the manuscript or published separately as a technical note. In the former scenario, which I would endorse, the section on Data should then describe the other datasets introduced in the present Appendix.

[13] 3.1 Statistical Models 98-99

The sentence

Four statistical models were developed and used to unravel the influence of different factors on SLR and to extract the background sea-level trend.

sounds like a downplayed announcement of the novelty of the study, which had suitably been stated in the Introduction (65-). The operation of unravel-and-extract is vague without recalling the explaining power of the factors that are made explicit and of those left in the background. Also, from the logical viewpoint, the background trend is dual to the selection of influencing. (I find comfort in a later sentence at 105-106: "its exact meaning depends on the choice of the predictive variables".)

I would probably rephrase the sentence above as

Four novel statistical models have been developed to separate the SLR signal into the time-varying influence of chosen predictive factors and the time-varying resulting background signal.

assuming I have understood correctly merit and novelty of the study. But if you developed

a single model in four setups, the wording should be adjusted (and the appropriate terminology used consistently all over).

[14] 3.1 Statistical Models 100

The 'penalised maximum likelihood' is referred to as 'parametric bootstrap method' in the abstract (6) and in the introduction (73). If the relationship between 'penalised maximum likelihood' and 'parametric bootstrap method' is topical, please comment on it explicitly. In general, please make sure to use a uniform terminology across the manuscript and avoid switching between near-synonyms. A consistent terminology will help the readers to skip forwards and backwards through the sections comfortably.

[15] 3.1 Statistical Models 106-107

I find it difficult to interpret the sentence

Its smoothness is controlled by a penalty term subtracted from the log-likelihood, which is proportional to the time integral of the squared curvature of the smooth term Wood (2020).

because the "smoothness" of the smooth curve is presented as the effect of a "smooth term" not defined earlier. I would suggest to write the parametric equations, if only in a concise symbolic form, in the body of the manuscript and add a lay résumé of the attending mathematics in the Appendix (with reference to Wood 2020 for the more statistically-minded reader of OS).

[16] 3.1 Statistical Models 107-108

Re

The penalty term was assigned a weight tuned to match the variance of the smooth curve to the variance of a 30-year average.

leads to the question of how sensitive to this statistical parameter the conclusions are. Take note: this is not a general question of model sensitivity. The point that ought to be addressed is: should one revise the conclusion about the SLR in the Netherlands if the smooth curve had been tuned to the variance of a shorter/longer time window? Is 30 years a robust one-fits-all value or does it affect the model's capability to assess multidecadal physical processes?

[17] 3.1 Statistical Models 110

I prefer this

This setup makes no assumptions about the drivers of SLR.

to the more colloquial

This setup is equivalent to assuming we do not know anything about the drivers of SLR.

I would suggest to make the terminology for drivers, factors, and predictive variables more uniform or more consistent across the manuscript.

[18] 3.1 Statistical Models 118-121

My understanding of the passage

Therefore, we decide to use a linear regression model with an undetermined phase and amplitude but a fixed period as in Baart et al. (2011) even though it might remove some additional variability around the period of nodal tides. Using this second model, the influence of the nodal cycle on the trend and variability of sea level can be studied.

is that

Therefore, we study the influence of the nodal tide constituent on the trend and variability of sea level using a linear regression model with undetermined phase and amplitude as in Baart et al. (2011). As a downside, the resulting sea level variability may not account properly for other multidecadal influences having periodic components close to the nodal cycle.

If correct, I hope that the second version is useful to review the first.

[19] 3.1 Statistical Models 122-123

In

For the third model, wind effects are included by adding $u|u|$ (TrNcZw), where u is the zonal wind from reanalysis averaged over the closest grid cell of each tide gauge (Fig. 1a).

I resent the lack of clarity about the average of the tide gauge readings. In the case of wind, I then wonder if you meant "averaged over the cells closest to the tide gauges". In sum, do they process only one wind signal for the zonal wind or as many signals as there are eligible wind cells?

[20] 3.1 Statistical Models 127

Why do you use Pd to label the model considering the **pressure gradient**? Would Pg not be a valid name considering that gradient starts with g ?

[21] 3.1 Statistical Models 127

I would appreciate reading which data have been used for the "linearly detrended sea level along the Dutch coast". This connects to the remark that the manuscript lacks a section detailing the use of model inputs.

[22] 3.1 Statistical Models 132-134

In

Then, instead of using the pressure in both boxes as predictive variables as in Dangendorf et al. (2014b), we take the difference between the southern and northern boxes. This adds only one variable to the model and is physically motivated by the fact that the meridional pressure gradient is related to the zonal wind by geostrophy.

I was puzzled by the expectation that geostrophy applies to the upper atmosphere, while the winds used as predictive variables here are taken at 10 m height. If this expectation is correct, the correlation between winds well within the atmospheric boundary layer and those well above would be another relationship not mentioned in the manuscript. Please clarify.

This concludes this comment. Thank you for your reading.

