Comment on egusphere-2022-935 (#3 Methodological reserve)

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This community comment #3 has been prompted by additional readings that, in turn, had been prompted by remarks on the manuscript R8-R11, R28, R35, R39 and R42. Their common denominator was a desire for clarity as to the peculiarities of Dutch coastal waters. In this one community comment, I digress from topical issues and set forth a case for a deeper and wider consideration of the existing literature.

A most relevant resource is the Zeespiegelmonitor (Sea Level Monitor), which is referred to as Baart et al. 2019 in the manuscript. This is a technical report of national interest, commissioned by Rijkswaterstaat, the governmental agency responsible for water management. The document is publicly available from the link provided in the manuscript but also from a permalink in the reports database of Rijkswaterstaat: https://puc.overheid.nl/doc/PUC_635781_31. The report is in Dutch and, for the benefit of the general reader, I will translate into English some passages.

In the following, “B2019, 45,56” is short for “Baart et al 2019, pages 45 and 56”. “Rn” is short for Remark n, where n is a progressive number in my previous community comments (up to 22 included in https://doi.org/10.5194/egusphere-2022-935-CC1, and from 23 up to 56 included in https://doi.org/10.5194/egusphere-2022-935-CC2).

[57a-b] Existing citations of B2019

The manuscript cites B2019 sparingly in LL 82,86 of §2.1, regarding the tide gauge observations along the Dutch coast.

a. As for the location of the tide gauges in LL 81-82, the phrase These stations are used for operational sea level monitoring because of their extended temporal coverage and uniform distribution along the Dutch coast clearly renders the phrase Dit zijn zes stations, redelijk uniform verdeeld langs de Nederlandse kust, met een lange historie aan metingen in B2019,39. I have critiqued the infelicity of the phrase ‘uniformly distributed’ stations in R9. Regardless, this confirms me in my mind that the authors have consulted B2019 closely.

b. As for the adjustment of the chart datum of 1885 in LL 85-86, B2019 also mentions successive jumps in the Dutch recordings. The manuscript reference to the adjustment of 1885 only is incomplete information; consider also R10. More important for the readers is
to offer a guarantee that the datasets deposited at the PSMSL have been gauged with respect to a Revised Local Reference (see R2019,42-44 and https://psmsl.org/data/obtaining/rlr.php), which evens out all operations on the chart datum. This reassurance would also resolve a concern raised in R11.

[58a-b] Omissions in the citations of B2019

a. However, B2019 does present a methodology and an expert opinion to account for the SLR acceleration. Strangely, the manuscript does not mention this contribution, not even in the Introduction paragraph addressing the debate on acceleration along the Dutch coast (LL 32-41). In contrast, B2019, § 5 (Methoden voor de bepaling van de huidige zeespiegel = Methods for the determination of the present sea level) presents much relevant information in an orderly fashion. In particular, B2019,47-48 refers to a Generalized Linear Model being used to carry out an analysis remarkably similar to those of the manuscript. Referring to B2019 seems all the more compelling if it is correct to state that Generalized Additive Models are a type of Generalized Linear Models (according to https://en.wikipedia.org/wiki/Generalized_additive_model). Finally, also the results of B2019 display a striking similarity of those presented in the manuscript. For example, Fig. 6.1 in B2019,53 is

which should be contrasted with Figure 2 in the manuscript (below). The key for the axes labels above is hoogte: height; lineair model zonder wind: linear model without wind; lineair model met wind: linear model with wind; jaargemiddelden: yearly averages.
[59] **Common-sense considerations**

Not referring to this piece of previous research seems a conspicuous omission to me, at least one that justifies a textual analysis of such an extent as this. These incidents strongly suggest that the authors should peruse and credit B2019 more extensively, at a minimum. Regrettably, they leave an unpleasant aftertaste that substantial weaknesses may also lurk in the rest of the literature review.

For sure, the authors should exercise much more caution in not sailing inadvertently towards the rocks of plagiarism by implicitly portraying steps ahead as strides ahead. For the sake of prudence, claims of novelty such as LL 246-248 -- *To our best knowledge, the GAM has not been applied to estimate trends and acceleration in sea-level data before*, and we believe it could help solve similar acceleration detection problems in regions other than the coast of the Netherlands -- may well apply and be well deserved (as I am still inclined to believe), but should attract more scrutiny from the community.

IMHO, as a Dutch saying goes, *er is werk aan de wikel*, there's work (to do) in the shop. I hope this commentary will help the manuscript develop a strong backbone and gain it the unconditioned appreciation of many readers.