Ocean Science
Discussions

Interactive comment on "Recurrence intervals for the closure of the Dutch Maeslant surge barrier" *by* Henk W. van den Brink and Sacha de Goederen

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Below we reply to the comments of anonymous referee #2. The original comments are given in *italics*.

0.1 Major comments

- *The methodology description and its applications are not always straightforward. In particular, the part concerning Poisson process (section 3.2 and further on) is misleading because, as far as I understood, it is used only to show the falseness of the independency assumption. Presumably, the trial of this method was part of the research process and search for the proper estimate of the reoccurring*

C1

events. In this case the description of the method seems to be superfluous for this paper.

The same point is mentioned by referee # 1. We agree with the referees that the description was not clear in all points. We changed the manuscript as follows:

- We added the following sentence to the Abstract: "We show that the Poisson process model leads to wrong results, as it neglects the temporal correlations that are present on daily, weekly and monthly scales. By counting the number of double events over a threshold of 2.5 m, and using that the number of events is exponentially related to the threshold, ... "
- We added an introductory paragraph to the Methodology section, in which we explain that the Poisson process model fails.
- We reshuffled the Conclusions, by first mentioning that the assumption of independence is violated, and afterwards that we used the empirical distribution to count the number of double closures.

- *The usage of forecast members is a valid and effective approach to expand the dataset in the lack of instrumental measurements. Just for understanding, did the authors have 2 model simulations - with and without meteo-forcing, or astronomical tides were analytically estimated (p3. lines 32-33)?*

The WAQUA-DCSMv5 has two options considering the astronomical tides. The first one is to use the tide that the model calculates, given the astronomical input from the boundaries. This result is indeed obtained by running the model without meteorogical forcing. The second option is to use tidal constituents calculated from observations for specific locations.

We used the first option, i.e. the tide as calculated by WAQUA-DCSMv5.

- *Are surge heights used somewhere in this study? It looks like all the data analysis is done based on water level timeseries, in this case, please remove 'surge' from the text (e.g. p3. line 18).*

C2

We agree with the referee that the use of the word 'surge' is misleading. We replaced the word with 'water level'.

- *There are several sources of uncertainty emerging throughout the analysis; it would be helpful to see the estimate of total uncertainty range. This is partly done in Figure 8 and section 5.1, but what about the uncertainties from GEV estimate and correction? In the conclusion (lines 40-42) very precise numbers are given without any potential error intervals, an additional sentence or two and a rough estimate in percentage of the total results would suffice here.*
  We agree with the referee(s) that the discussion about the uncertainties was insufficient. We added a discussion about the different uncertainties as follows:

    - We added an extra paragraph about uncertainty analysis (Section 3.4) in which we discuss two sources of uncertainty that the referee mentions, i.e. due to bias correction and due to the choice of the threshold.

    - We added the estimate of the 95% uncertainty range due to the bias correction in Figure 9 (old Figure 8). The figure shows that the uncertainty in the bias correction is considerably larger than the uncertainty due to the choice of the threshold. We thank the referee for pointing this out.

    - We added the uncertainty ranges also to the Abstract, Results and Conclusions.

0.2 Minor comments

- *in abstract, p8 line 9 and maybe somewhere else in the text: reduplicates is presumably used in the meaning of "a factor of two" but cannot be used in this sense and is misleading in the context. Please use other synonyms (doubles, redoubles, duplicates,. . .)*
  We changed the word "reduplicates" with "doubles"

- *Eq.5 and others use $\log$ and p.3 line80 use $\ln$. If $\log$ is natural logarithm, please use the same notations everywhere.*
  We changed $\log$ to $\ln$.

- *p5. lines 13: L_{adj} is actually adjusted surge or rather adjusted water level?*
  This should be (adjusted) water level. We changed it accordingly.

- *p3. line 77: please coordinate singular/plural forms The distribution(s) . . . are (is) . . .*
  We changed it to plural.

- *fig.1 and 4.1: is the correction made by adding these values to the observed data for each year? Where the numbers are coming from?*
  Indeed the correction is made by adding the corrections to every year (rounded to cm). The correction is presented in the 'Basispeilen'-report (1993), which is added to the references.

- *p5. line 33: how does the value 0.57 m came from the eq. (1) using 12280 m³/s discharge I come out with 0.43 m*
  The referee is (of course) right, the number should be 0.43 m. Fortunately, the calculations are done with the correct formula. We adjusted the number to 0.43.

- *p5. line 38-39: it is not that the water levels at Rotterdam are 3cm higher than at Hoek van Holland, it is that for Rotterdam you consider additionally the river discharge and for HvH only the sea level. Please reformulate the sentence, it is misleading.*
  We agree that the WAQUA-DCSMv5 model does not take the river discharge into account in calculating the water level at Hoek van Holland, whereas the observations at Hoek van Holland are influenced by the river discharge. However, from De Goederen (2013, page 16) it can be derived that the effect of the river discharge on Hoek van Holland for the maximally observed discharge (12280 m³/s)

is only 4 cm, i.e. $\approx$10% of the effect in Rotterdam. The effect on the Gumbel location parameter is only 8 mm. It is therefore allowed to neglect this effect - especially if we take the uncertainty in the Gumbel parameters of the observed record into account.

We added the following sentence to the manuscript: "Based on calculations by Rijkswaterstaat (de Goederen, 2013, page 23), and neglecting the effect of the river discharge on Hoek van Holland, the water level at Rotterdam can be approximated by:..."

- *p5. line 27-28: 'River run-off is not considered in this paper'. Do the authors mean it is not considered in the hydrodynamic model? Because later in the paper there is a talk about run-off again.*
  We meant that River run-off is not considered in the hydrodynamic model. We agree that it is misleading, and removed this sentence.

- *fig.6: upper panel – 2 times "observation" in the legend.*
  corrected