

The simultaneous occurrence of surge and discharge extremes for the Rhine delta

Response to anonymous referees 1 and 2

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We wish to thank both reviewers for their comments which have highlighted parts of our manuscript requiring further clarification. In our response below, all page and line numbers refer to the NHESSD document. Paragraphs in italics are the words or summarised words of the reviewers.

Response to reviewer 1

1. *An interesting paper, demonstrating a potential engineering application of climate model data. However, the authors seem to have adopted a complex approach, based mainly on model data, when a more direct approach based on measured data could probably have done the same job.*

This reviewer is particularly focused on the engineering application of our study. We expect that the approach we take will be more familiar to the climate modelling community, but we hope that the paper will be relevant and understandable to both climate and hydrology groups. We thank him/her for their detailed review.

The applications are indeed a strong motivation but we do not intend that our results should be directly applied to the Maeslant barrier. Rather we hope to (1) provide insight into whether there are meteorological connections between the North Sea wind distribution and precipitation over the Rhine basin that favour a ‘joint event’ in both the recent and future climate and (2) show that, in response to van den Brink et al., a comparison of the results with outcomes based on the null hypothesis of independence is instructive if not essential for assessing significance, (3) motivate further research with finer-scale/hydrological models, depending on the significance of our results. We will emphasize point 1 by adding the following to our introduction of the idealised approach (Sec. 1.5):

‘Our objective is not simply to provide the best guess for the dependence between surge and discharge but, through investigating a range of conditions and examining the synoptic context, to better understand the reasons behind the strength/weakness of the connection between the North Sea wind distribution and precipitation over the Rhine basin, i.e. the large-scale factors that contribute to a ‘joint event’.’

Measured data could not have ‘done the same job’ for the future climate and there are only limited records available for the past (about 100 years). In keeping with other climatology studies, we use a 30-year period to represent the

‘stationary’ climatic conditions, but effectively lengthen the timeseries to 510 years using an ensemble of climate model runs. A single 30-year period of observations is too short to conclude anything significant about joint probabilities of extreme events. If a longer range of years are used, there is the risk that the ‘climate’ itself evolves. For example, Buishand et al. (2013) present homogenized precipitation observations from the past 100 years and show that mean winter precipitation in the Netherlands increased by about 35%. Such observational series should not be used directly — the background trend must first be removed, introducing further assumptions about the relative contribution of climate change at each data point.

A version of the above paragraph has been added to section 2.1 of the manuscript following P117 L2.

2. *The authors imply, both in the abstract and in the main text that the barrier’s fitness for purpose is usually considered based on the assumption of independence between sea surge and river discharge. It seems unlikely that any major project designed in the last 50 years would make such an assumption. I suggest, unless there is quotable evidence that the design was based on independence, it be made clearer that this is a hypothetical comparison. As it stands, the barrier designers may feel that the paper is questioning their professional competence.*

The reviewer refers to L16–17 in the abstract, and L17 on P113. Whilst we did not directly mention the assumptions made for the barrier’s construction, we can see how the text gives that impression. A new publication in Dutch (Geerse, 2012) by Deltares, a Dutch independent institute for applied research in water, states that ‘In determining the hydraulic boundary conditions for tidal rivers it has been assumed, until now, that storm surges at Hoek van Holland and Rhine discharges at Lobith are uncorrelated. The assumed lack of such a correlation is based on research from the sixties [Brief Minister De Quay, 1967; Van der Made, 1969] (our translation)’. The probabilities of surge and discharge were therefore assumed to be independent at the time of the barrier’s construction. However, a change to the assumed probabilities of simultaneous surge and discharge extremes, would not have affected the design for the Maeslant barrier, but it would have implications for the dikes that the barrier protects (F. Diermanse, Deltares, 2013, personal communication). In the case of a very high surge in combination with a very high discharge, the barrier will be opened, if the water level at the landside becomes higher than the water level at the sea-side, and the risk that water levels will not be maintained at a safe level in the tidal area is accepted (F. Diermanse, Deltares, 2013, personal communication).

Sections 1.1 and 1.3 have been modified to include the above discussion.

3. *It would be a more interesting paper if the authors were able to say what their conclusion would imply for the barrier. For example, if there were a slight dependence between sea surge and river discharge, how would this affect the probability of simultaneous occurrence at the barrier and would this make any difference to its standard of service. Related to this, it*

would be more interesting to focus on the n-day duration, and the relevant lag with surge or wind speed, that would tend to cause the "simultaneous" occurrence implied in the papers title, and which could potentially be used to comment on implications for the barrier.

We expect that the slight dependence will lead to an increase in water height of the order of cm (communication with Deltares, 2011). A new observation-based study by Deltares (Geerse, 2013), motivated by an internal report of our work on the 'current climate' period (Kew et al., 2011), concludes that the slight dependence leads to an additional increase of about 10 cm in sea level at Dordrecht. For dike design and safety assessment, this would be considered a substantial increase. For the barrier itself, this would not jeopardise its standard of service (F. Diermanse, Deltares, 2013, personal communication).

We have added a version of this paragraph to the conclusions.

- 4. Perhaps the ensemble modelling I have been involved with is different to that used in the paper, but doesnt it involve the same sequence of forcing events just with small changes to model parameters. If so, ensemble modelling would deliver a number of representations of the same periods of time and the same storms, and therefore break the assumption of independence between the years of data analysed in the paper. A little more explanation of why this is not so would be enough.*

In the ESSENCE ensemble, different members are generated by disturbing the initial state of the atmosphere. Gaussian noise with an amplitude of 0.1 K is added to the initial temperature field (Sterl et al. 2008). On the time scale of about 2 weeks, the memory of the initial synoptic configuration is lost. Considering the ESSENCE baseline simulation starts in January 1950 and the earliest data we use is for November 1950 (20 days before 1 December) and we select periods that are 30 years long, the storms in one ensemble member will be completely unrelated to the storms in another. We will add this discussion to section 2.1. The ensembles that the reviewer has used were possibly intended for a much shorter time period.

See http://www.knmi.nl/cms/content/84072/the_essence_project_the_power_of_a_large_model_ensemble, Fig.1b. The figure shows how the temperature time-series between ESSENCE members compares to observations for a fixed point. The spread of the ensemble values does not noticeably increase in time, indicating that already very early on, memory of the initial state is lost.

We will add to the manuscript after P117 L7: 'Note that, within a few weeks, the memory of the initialising synoptic conguration is lost. Considering the ESSENCE baseline simulation starts in January 1950 and the earliest data we use is for November 1950 (20 days before 1 December) and we select periods that are 30 years long, the storms in one ensemble member will be completely unrelated to the storms in another.'

- 5. Related to that, looking for slight dependence between one model parameter and another parameter derived from the same model seems of doubtful*

validity, as it would presumably just reflect the assumptions underlying the model. Again, a little more explanation of why this is not so may be enough to allay this concern.

Wind and precipitation extremes strongly relate to the large scale atmospheric flow in the model. The large scale flow is governed by a set of well know equations, and flow characteristics in this type of atmospheric model are well evaluated not only in a climate integrations but also in a weather prediction mode. Also, the sequence of events we obtain in the model (the building up of a ridge over the Atlantic after the passing of number of cyclones as shown in Fig.8 and Fig. S5.) is frequently observed. Therefore, there is no a-priori reason to suspect that the results are just reflecting the underlying assumption of the model. But, we admit that approximation in the model and the coarse resolution could affect our statistics, and that the robustness of the results needs to be confirmed with other model runs (now stated in the conclusions).

6. *Combining this and the previous point, could it be that the same storm is picked up repeatedly, one per ensemble run, as this would approximately correspond numerically with the level of dependence detected.*

No, the runs are independent. The explanation for this is covered by point 4 above.

7. *I found it difficult to see the intended information content in some of the figures, particularly Figure 4. Presumably in the finished paper, the figures will sit within the text, but I would suggest some example interpretations are added so that readers can follow through the detail of the calculations and conclusions drawn. I suggest also that all figures of one type are plotted on the same scale. At present, most of the figures are on different scales.*

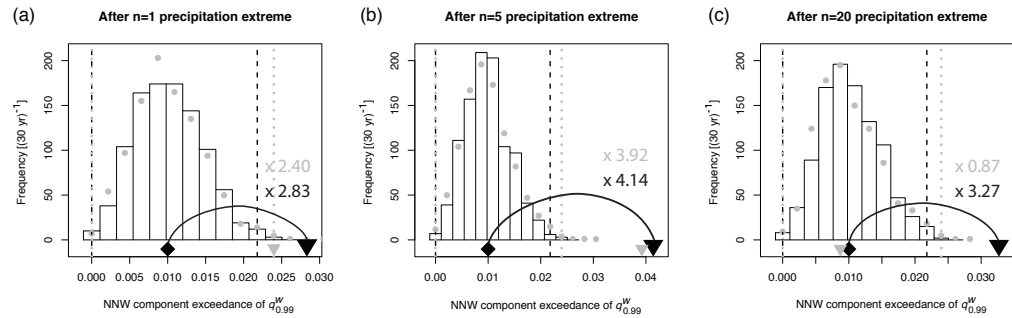


Figure 1: The new Fig. 4.

We made the following changes to the Fig. 4 (See Fig. 1 above) to improve its clarity:

- As only the horizontal axis is relevant for the triangle markers, we have shifted them to a position below the zero-frequency line, in an attempt to separate the two types of data (histogram and single values) on this plot. We also inverted the triangles so that one of their tips points to the value they indicate. They are intended to look like markers on a scale.
- We added a diamond marker to indicate the climatological exceedance of 0.01 (equivalent to the expected exceedance of a random sample of smaller size than the climatology) .
- We added an arrow between the diamond and the black triangle to illustrate the multiplication factor between them and noted the multiplication factors for the current and future climate periods.
- The new caption will be: ‘Exceedance of the climatological $q_{0.99}^w$ for days immediately following extreme n -day precipitation sums (marked on the horizontal axis by a black triangle for 1950–1980, and grey triangle for 2070–2100) and, for comparison, for the 1000 random samples (see Section 2.3 for details on construction of the samples) presented as a histogram (bars for 1950–1980, dots for 2070–2100). The vertical lines enclose 99% of the 1000 samples (black dashed for 1950–1980, grey dotted for 2070–2100). The climatological exceedance, 0.01, which is the expected exceedance if assuming $P(w_1^*)$ and $P(r_n^*)$ are independent, is marked by a black diamond. The multiplication factors between the expected exceedance and the black and grey triangles are written in the panels in black and grey, respectively.’

The vertical scales across panels of Fig. 4 and other figures where appropriate are now uniform.

8. *I suggest that the conclusions be prefaced with a comment to the effect that these are tentative conclusions drawn from numerical model data. Anyone reading only the conclusions might think that they are based on more solid information.*

Agreed. We preface the conclusion with ‘In this study, we explored the simultaneous occurrence of extreme North-Northwesterly winds over the North Sea and extreme n -day precipitation over the Rhine basin (proxies for North Sea storm surges and extreme Rhine river discharge respectively) for the current and future climate in a large 17-member global climate model ensemble. The conclusions are based on results from the ensemble.’

Minor comments from scanned mark-up

Title *The normal definition of simultaneous is stretched too far here. And the relevance to combined occurrence at the barrier is tenuous at best.*

See the comment on P118 L18 below with regard to the time delay of the discharge peak between Lobith and the barrier.

P112 L2–14 *Threats is wrong word; correct spelling occurrence; ‘water management’ usually refers to desirable water and seems wrong here; ‘inspected’ wrong word: maybe ‘assessed based on historical data’.*

We will remove ‘threats’ and correct the spelling. We will leave ‘water management’ as it is, as the meaning of the term includes controlling the water levels. The word ‘inspected’ is appropriate here, in the sense of ‘close examination’ of the synoptic development by viewing a sequence of synoptic charts. These are the model fields, not historical synoptic charts. If one needs to statistically extrapolate the data (using e.g. extreme value analysis) to explore the extremes, model fields will not exist for those extremes, and one cannot investigate the physical processes that produce the extremes. We will adjust the text to ‘and thereby permit the *model’s* synoptic development of *the extreme* events to be inspected.’

P114 L24–25 *Isn’t this [Van den Brink et al.’s 1570 yr of data] 92 small variations of the same 18-year period? [The assumed independence of their data] seems unlikely as they are from multiple runs of the same model.*

See Van den Brink et al. (2005) for a good explanation of the validity of their assumption in their paper. In our paper, we comment that they adequately verify the independence of their ensemble runs and that they also admit that the model climate variability might not be as large as in reality.

P115 L23 *Winter: Delete or explain ‘winter’ [in ‘Multiday precipitation extremes are likely to increase in intensity (winter) (Kew et al., 2011)]’*

We will rephrase this to ‘Multiday winter (DJF) precipitation extremes are likely to increase in intensity (Kew et al., 2011)’

P116 L1 *This is an idealised approach in which a model parameter is compared with another parameter from the same model!*

See point 5 above.

P116 L5 *Not obvious what is gained over the direct approach of plotting a scatter diagram of surge against flow or rainfall . . .*

If the reviewer means what is gained by using models rather than direct observations here, then see our comment about ‘measured data’ in point 1 above. But perhaps the reviewer means ‘why take an idealised approach with a sensitivity analysis when we could simply plot the relevant scatter diagram directly’. The gain is that we can start to understand the reasons why the scatter diagram is as it is, which can provide more confidence in the results. For example, by inspecting the distribution of wind directions at 1, 5 and 20 days after heavy rain, we obtain an idea of the time scale over which the rain-bearing systems have an impact on surge favourable winds. In observing that there is continuity between the results for different n-day precipitation sums, we can be more confident that

there is a physical mechanism behind the statistics. Or, stated otherwise, the weak dependence at the relevant n can be explained from the physically-based and initially strong dependence that decays with time.

P118 L18 *There is no need for n (days) to be selected arbitrarily. Why not focus on the n and the lag relevant to both surge and flow peaking together at the barrier; otherwise the detected dependence may be irrelevant for the barrier.*

We state that $n=10$ and $n=20$ are most relevant. The lag between the peak discharge at Lobith and the peak discharge at the barrier is on the order of 6–18 hours (Geerse, 2013). Reasons for looking at n outside the range 10–20 are given in the discussion about sensitivity above. Our objective is to increase understanding about the meteorological connection between the surge and discharge events in order to make sense of the statistics, i.e. not solely to provide the ‘best guess’ relevant for the barrier.

P118 L11–12 *Are these spells [of precipitation and stream flow] overlapping or discrete?*

The stream flow spells are discrete — we take the daily stream flow. The spells of n -day precipitation prior to the stream flow day are thus overlapping, except when $n = 1$. We realised a description of the data used to calculate the correlation between precipitation and stream flow is lacking. We will add one in the Supplement together with Fig. 2 below. We did not correlate the model data with the stream flow measurements because the model ensemble was used to generate new sequences of rain-bearing systems and thus does not contain the historical sequence that gave rise to the observed stream flow.

P116 L22–23 *Coarse grid model data only, seems an odd choice for very subtle dependence*

Yes, this coarse grid is a caveat of this study and we would have used higher resolution ensembles if they had been available. However, we expect the multi-day, large area, winter precipitation sums and large area wind fields, to originate from large-scale disturbances, which ESSENCE produces well. We also use percentiles rather than absolute values in our definitions of the extremes. We already acknowledge in the concluding discussion that there are other processes not captured by our model study that would be important to consider in a more thorough investigation. We hope to motivate more thorough investigations by having indicated that there is a small but significant dependence already notable at large scales.

P118 L22 *0.99 means or order 99.9% of the data are not used [in joint extremes]; why not 0.97 or 0.98 to provide a more robust sample?*

The 99% threshold is a subjective choice and in future work, results for a range of thresholds could be investigated. Here we use 99% as a compromise between having a reasonable sample size and taking a hard extreme, close to the return periods of interest (e.g. 1 in 10 years for the closing of the Maeslant barrier or 1 in 1250 years for dike heights.)

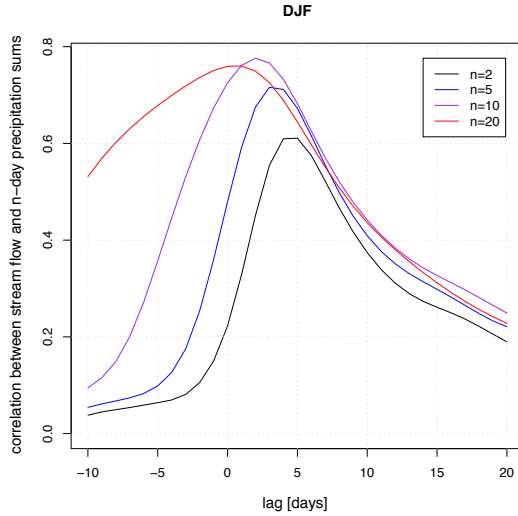


Figure 2: Correlation between DJF Rhine basin n -day precipitation sums from the E-OBS dataset and stream flow measurements at Lobith (1950–2000) as a function of the lag between them in days.

P118 L19 *where is it [the asterisk] marked?*

To eliminate confusion, we will reconstruct the sentence as follows: ‘In subsequent equations and figures, the notation r_n^* , i.e. r_n with an asterisk, is used to represent the set of n -day precipitation sums that satisfy the condition $r_n > q_{0.99}^r$.’

P119 L4–5 *This needs explaining, with the number of days and records per season ...*

The relevant information appeared earlier in the manuscript in an early version but was mistakenly eliminated during a reorganisation of the text.

We will insert the following after P117 L10: ‘A total of 90 n -day (n in range from 1 to 20) precipitation sums are created, each ending on a subsequent day of the DJF season. The first 20-day sum thus runs from 12th November – 1st December and the 90th 20-day sum runs from 9th – 28th February (also in leap years).’

We also add to P119 L15: ‘For a 30-year period, a 90-day DJF season and 17 ensemble members, ESSENCE provides $30 \times 90 \times 17 = 45900$ n -day precipitation sum and wind event pairs.’

P121 L22 *Explain [‘shifted clockwise’], e.g. SW moves to NW.*

We have changed P121 L22–23 to ‘For 1-day precipitation events (left hand column), the peak of the distribution is shifted clockwise (from SW to W) with

respect to climatology, favouring westerlies, whilst the NNW direction is not favoured more than in the climatology.’

P123 L6–9 *The information content of Fig. 4 is unclear. Add an example interpretation bringing in the bars and/or triangles of Fig. 4. How is ‘3–4 times’ seen in Fig. 4? Same comments for Figs S1 and S2.*

See point 7 above.

P124 L9 *Define SLP*

SLP was already defined on P114 L21–22, but we will write it in full for the section title for clarity.

P126 L24–26 *Throughout, needs to be clear and qualified as being based on a coarsely gridded climate model.*

We have now emphasized that we use climate model data in the abstract and conclusions.

P133–134 *Frequency seems the wrong word for the y-axis, especially without a unit ... Use the same frequency scale for each panel.*

We will add $[(30 \text{ yr})^{-1}]$ to the y-axis.

Reviewer 2

General comments

1. *The authors present an analysis they designed to investigate whether the occurrence of high river discharges from the Rhine and North sea storm surges are independent. They use average areal precipitation within the Rhine aggregated over a number of days as a proxy variable for river discharge and average daily windspeed in a specific direction over the North sea as surrogate for sea surge. There are a number of interesting results their investigation has yielded. However, I feel that the way the authors have discussed their results and summed them up make drawing unambiguous conclusions relevant to the objective of the article difficult. Based on their results, I am not sure whether the conclusion they drew in the abstract is justified.*

The reviewer later questions (point 3 below) whether our results contradict lines 16–18 of the abstract, which concern the 3-fold increase in joint probability. This is probably what he/she is referring to by ‘[difficulty of drawing] unambiguous conclusions’. We explain why this is not the case in response to point 3. Considering here also the views of reviewer 1, we will emphasize in the abstract that our results are based on climate model data.

Detailed comments

2. *The analysis makes an assumption that the extremes of n-day (for n: 1-20) precipitation sum over the basin lead to high discharge and similarly the extremes of the daily average wind speed in a specific direction lead to surge. I would be cautious in making such an assumption, especially in relation to discharge. The consequences of aggregated basin average precipitation over different numbers of days depend very much on the size of the catchment, which make it difficult to use them all as proxies for extreme discharge. Instead of using all aggregated n-day extremes, why not identify the number of aggregation days whose extremes are strongly related to the extremes of the discharge (at least statistically) and base the investigation on those aggregation days only? Furthermore, how strongly are the extremes of the aggregated precipitation related to extreme discharge? This dependence can be influenced by the spatial and temporal patterns of the precipitation field over the days of aggregation.*

We did statistically identify the number of aggregation days whose extremes are strongly related to the extremes of the discharge (see P118 L11–13 of the manuscript). We will include a description of how we did this in the Supplement, along with Fig. 2 above. The reason for nevertheless presenting results from a range of aggregation periods is given in the responses to reviewer 1. In short, our objective is not simply to provide the best guess for the dependence between surge and discharge, but to understand the reasons behind the strength/weakness of the large-scale physical connection. The question of how strongly *extremes* of aggregated precipitation are related to *extremes* of discharge and the dependence on spatial and temporal patterns of the precipitation field over the days of aggregation is a very interesting and relevant point, but beyond the scope of this idealistic study. We will add a short discussion about this in the concluding discussion.

3. *To draw conclusion on whether extreme discharge and sea surge are dependent, the authors seem to focus on the 20-day precipitation sum as a proxy for discharge. Although their analysis shows an increase in the probability of directional wind speed they say is relevant to surge after an extreme 20-day precipitation, the authors mention that only 3% of all surges have occurred after a 20-day extreme precipitation. Does not this put in question the conclusion the authors make in the abstract that the probability of extreme surge following a 20-day precipitation extreme (a proxy for extreme discharge) is higher than the probability one would obtain if independence was assumed? Also, does not the result discussed on page 127, lines 18–20 enhance this contradiction? Under what discharge conditions did the other 97% of the surges occur? Do they have any relationship with the extremes of precipitation on other aggregation days? This might have to do with identifying an appropriate proxy for extreme discharge (see the first comment above).*

Aggregation periods between $n = 10$ and $n = 20$ correlate best with discharge. We focus on $n = 20$ because it has the best correlation for zero lag, it is the period used by Van den Brink et al. (2005), to which this paper may be seen as a response, and also because $n = 20$ is the least likely aggregation period from a range of 1–20 days to show a dependence. If a dependence exists for $n = 20$, we might expect it also to exist for $n = 10$. We refer the reviewer/reader to Fig. S2e–f of the supplement reproduced in Fig. 3 below. Our analysis shows

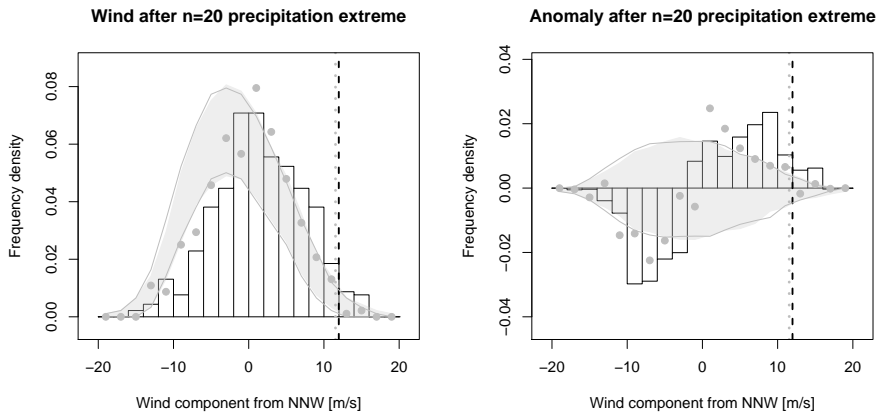


Figure 3: (Fig. S2e–f of supplement) NNW wind component following extreme (bars for current climate 1950–1980, dots for future 2070–2100) and random samples (95% density range, shaded for current climate, outlined for future) of n -day precipitation sums (left) and their anomaly with reference to climatology (right). The $q_{0.99}^w$ threshold is marked with a dashed line (black for current climate, grey for future).

an increase in (NNW) directional wind speed by a shift towards more positive values in Fig. S2e, when we analyse the wind distribution on the day after an $n=20$ precipitation extreme. We define the threshold of an extreme surge by the 99th percentile of the total population NNW wind distribution, indicated by the dashed vertical line. The shift of the pdf results in more than 3% of data being found to the right of the dashed line instead of 1%, as expected if independence holds. This can be seen by noting that the bars of the histogram rise above the shaded (samples of the total population) distribution.

Regarding the comments about P127, L18–20, we adjust the text to make the meaning clearer, as follows: ‘Just as for Fig. 6b of Van den Brink et al. (2005), there is no correlation to be seen between the two variables (black contours, Fig. 10) and, at a first glance, the assumption that surge and discharge are independent appears valid. However, the significance of our results become clearer when they are compared to the distribution that would result if the assumption of independence were true. Therefore, in addition, we show a simu-

lated distribution for which we know that the wind and precipitation variables are independent (grey contours)' ... and later ... 'It is also noteworthy that the actual distribution is aligned slightly more towards the diagonal axis, indicating that the actual correlation between the two variables is slightly more positive than for the independent probability distribution.'

The probability of obtaining an extreme surge following an $n = 20$ precipitation extreme is indeed higher than one would obtain for independence. By independence, we mean that the joint event probability can be calculated by the multiplication of the two individual event probabilities, such as the probability of throwing e.g. a 3 and a 6 with a pair of dice is $\frac{1}{6} \times \frac{1}{6} = \frac{1}{36}$. If we examine all the times that we have thrown a 3 with the first dice, then the probability, out of these events, of throwing a 6 with the second is $\frac{1}{6}$. This is equivalent to selecting the set of days after $n = 20$ precipitation extremes and expecting to find that approximately 1% of those days contain a NNW wind extreme. We find that the surge condition is met in more than 3% of those days, so that is at least 3 fold increase. What about the other 97% of surges? Returning to the dice: in all the times that we throw a 6 with the second dice, approximately 5 times out of 6, we will not have thrown a 3 with the first dice. This is what we expect. If we assume that surges and discharge are independent, and extremes are defined by 99th percentiles, we expect that in 99 times out of 100 when we have an extreme surge, it will not have been preceded by an extreme discharge. However, we find that this is the case less than 97 times out of 100. The dependence is small but we find it to be significant.

In the 97 % of the data when there is a storm surge but no high river discharge, the barrier will be closed. It would be very interesting to inspect the synoptic situation for the some of the remaining 97% of surges, for e.g. when precipitation is low, medium and high but not extreme, but this is something we have not yet done.

4. *What was the sampling strategy employed in drawing the 1000 samples to estimate the sampling error?*

We select 459 unique days, out of the full 45900 available, using a random number generator. We record the exceedance of $q_{0.99}^w$ for that sample. The 459 days are then replaced. This procedure is repeated 1000 times. We will add more detail to our description on P120 L1–2.

Editorial comments

On pages 123 and 124, Figure 5a is mentioned, but there is just Figure 5. Page 128, line 14: ... was found to be ...

Changed, thank you.

Additional changes proposed by authors

P113 L5–7 The explanation that ‘the closed barrier would be at risk of failure due to a reversal of the pressure gradient at the barrier’s base’ should be referenced, however we do not have a record of the personal communication from which it came. We will instead write ‘In the case of an extreme joint event, the barrier will be re-opened if the water level at the landside becomes higher than the water level at the seaside, and the risk that water levels will not be maintained at a safe level in the tidal area is accepted (F. Diermanse and C.P.M Geerse, Deltares, 2013, personal communication)’

P113 L21 Define NAO.

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