Dear reviewer,

We would like to thank the reviewers for their detailed reviews of our manuscript and for the relevant points that they have raised.

In the context of our response, we have conducted some further analysis and addressed all the points that were raised by the reviewers. In response to the suggestions, the main changes in the revised version of the manuscript are:

1) We have added explanations on the choice of scenarios and further elaborated on this, and dependence analysis in the discussion.
2) We have added a table summarising the events used for validation.
3) We have updated figure 3.
4) We have conducted additional scenarios.
5) We have updated the graphical representation of figure 6.

We believe that the manuscript has benefited substantially from the revisions and hope that the reviewers agree with the changes that we have implemented.

Please find below the answers to the individual comments and suggestions (in blue bold font). The answers to the comments are placed below the comments (in black font) and changes made in the manuscript are included in italic black font.

The selection is rather limited and conservative based on the combination of 100-year return values of Q and waves without any analysis of the dependence between Q and waves. While I appreciate that adding a statistical analysis would change the focus of the paper and modelling many more combinations with different return periods will dramatically increase computational effort, it would provide a more comprehensive analysis of the importance of waves for compound flood risk. At least some discussion on choice of scenarios on the results should be included.

Thank you for this remark. We have indeed performed a preliminary dependence analysis between Q and waves by estimating the Kendall's Tau coefficient (see Ward et al., 2018; Couasnon et al., 2019). However, we decided not to present these results, since we used data from two different sources, as wave observations were not available. To assess dependence however, data used should come from the same source (Marcos et al. 2019). We used observed Q and modelled offshore waves from the CSIRO hindcast, produced from NCEP-NCAR reanalysis data (Cox and Swail, 2001). Moreover, we did not include a dependence analysis in the manuscript, since independence or dependence does not affect our results, showing driver interaction. It would only provide an overview on the likelihood of occurrence of our chosen results, which is relevant for a risk assessment.We decided on the extreme scenarios following previous flood assessment studies for South Africa. Most have previously used 50-year return periods, which is now shifting towards using 100-year return periods (Theron and Rossouw 2008; Basson et al., 2017). For computational reasons we did not explore a wider range of scenarios, as lower wave return periods (20- and 50-year) do not significantly differ from the 100-year return level and higher return levels were not available. As we have shown that Breede Estuary is a Q-dominated estuary, even during compound flooding, we developed an additional scenario with extreme wave (100-year, all directions) and moderate (20-year) Q conditions. However, the results did not significantly differ from the 100-year RP compound flood scenario, which is why we decided not to include the additional scenario in the manuscript.

As our results clearly show that the estuary is Q-dominated, we did not consider higher return periods for river discharge. We explain our scenario choice in section 3.3, line 246:

"*For computational constraints and data limitations, we decided on the 100-year return period for waves and Q, as this was also recommended by previous flood assessment studies for South Africa (e.g. Theron and Rossouw, 2008; Basson et al., 2017).* "

We have added a paragraph to line 433, further elaborating on this:

"In this study we focus on the effects of driver interactions on flood characteristics but have not considered dependence and joint probabilities of waves and Q. This information becomes relevant when assessing risk from compound flooding, which is beyond the scope of this study and should be considered in future work. For such a risk assessment, a wider range of return periods should be explored."

It is also unclear what the relative timing between the Q, waves and tides peaks is and how this is selected and what the effect of the time lag on the results is. If no observed data is available for a time lag analysis a sensitivity analysis would strongly improve the robustness of the findings.

We agree with the reviewer that the timing between Q, waves and tidal peaks is not clearly explained in the manuscript. For all scenarios, peaks of tides and Q (when considered) occur at the same time, while waves are constantly at peak conditions (due to constant wave conditions). We agree that conducting simulations with different peak timings (sensitivity analysis) would clearly increase the relevance of our results, but would also require immense additional computational effort. Since we are interested in maximum flood heights, we decided to simulate co-occurring peak values only. To clarify the timing between Q, waves and tides, we added a sentence in section 3.3, line 226:

"All scenarios assume that the peaks of the drivers occur at the same time."

L185: A table for the validation events would be helpful to a reader.

Thank you for the suggestion. We included a table in section 3.2, summarizing all validation events and their names, used in figure 3. We adjusted the text in section 3.2, line 194 accordingly.

"To account for the full tidal range, these simulations include a spring-, average-, and a neap tide event (see Table 3 for the event names and dates of occurrence)."

We removed lines 196 and 197.

Table 3: Validation events and dates of occurrence.

Event Name	Average	Neap	Spring	Spring + high Q
Date	14-19/07/2007	18-23/09/2007	27/09-01/10/2007	22-25/11/2007

L226: It is unclear how the return value for Q is translated into an event hydrograph, which is required to force the model.

We agree with this comment. We added a sentence to line 230, in section 3.3, to elaborate on how we came up with the hydrograph:

"We developed the Q-hydrograph to force the upstream open boundary by normalising the hydrograph of the highest Q event, for which the full hydrograph was available. We then multiplied the normalised hydrograph with the 100-year peak value."

L227: It is unclear why the maximum Q peak value was corrected. Also, provide more details about how the 100-year event was derived from the time series.

Referring to the correction of the maximum Q peak value, we added a sentence to line 228 in the manuscript:

"The value was corrected, as for this event the flow gauging station stopped measuring before the peak was reached."

Basson et al. (2017) explain in detail how they came up with the 100-year return level of the Q time series. We refer to their report in line 230 and hope that this point is now clearer.

L229: Can you justify the choice of a 3 m³/s Q event for the S_{TW} scenario?

Thank you for raising this point. First, we must correct the value, as the selected maximum Q of the boundary conditions for the S_{TW} scenario was not 3 m³/s but 1.2 m³/s. We apologize for this mistake. Second, we agree that adding an explanation on this choice helps to understand the scenarios. We had to keep the upstream open boundary open, as a closed boundary would have led to water accumulation inside the estuary. We therefore chose the lowest measured Q from the time series, to keep the scenarios realistic. We do not expect such low Q to have an effect on the scenario results. We added a sentence in line 231 to explain the choice of Q for the S_{TW} scenario:

"For the S_{TW}, so the no-Q scenario, we kept the upstream boundary open, so that incoming flood water does not accumulate there. Thus, we chose the lowest measured Q event from the time series to force the upstream open boundary, where Q does not exceed 1.2 m³/s."

L244: It is not clear how the two columns in Table 3 relate to the four events shown in Figure

Thank you for this suggestion. We adjusted the event names in Figure 3 accordingly to relate to the newly created Table 3. We additionally added goodness-of-fit estimates to the figure and moved Table 3 to Appendix C. Please see our changes in the figure below:

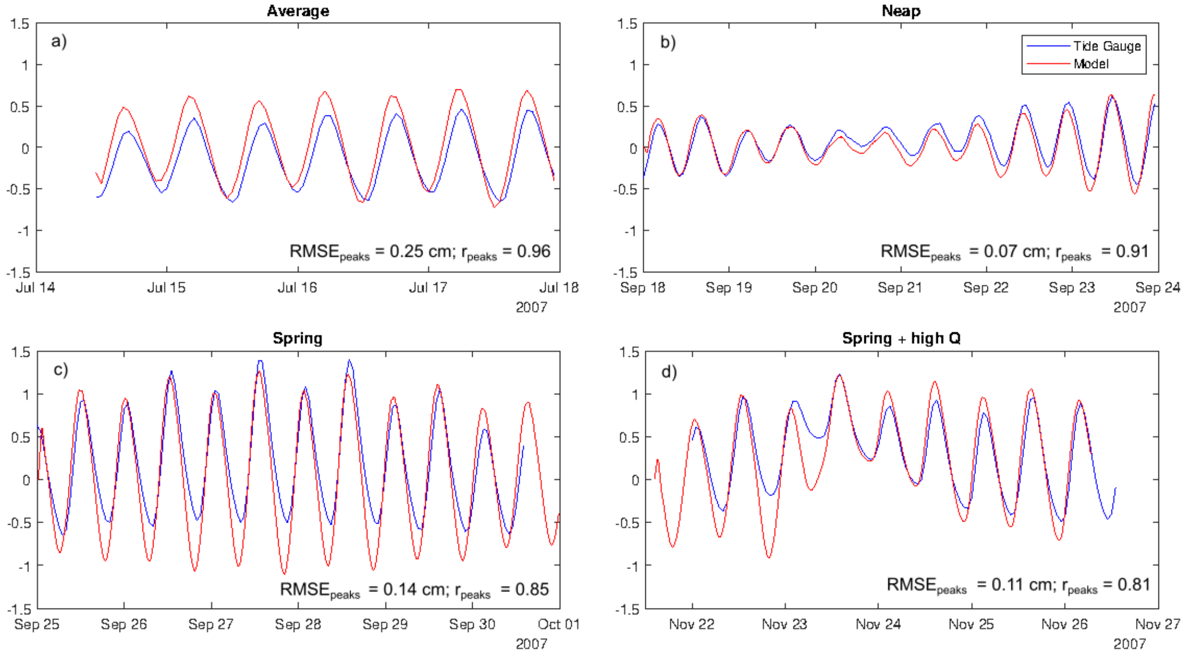

Figure 3. WLs of the model validation runs (red curve) at the tide gauge station, compared to observed WLs from the tide gauge (blue curve). (a) shows WLs of the average tide event, (b) the neap tide event, (c) the spring tide event, and (d) the high

river discharge event, coinciding with the spring high tide. All panels include goodness-of-fit estimates for peak values of each event.

L296: It is not clear what you mean with “similarly higher”. In Figure 4 it seems to me that the difference between STW and STWQ decreases outside the estuary.

We agree that the difference between STW and STWQ decreases outside the estuary. Since we are not focussing on the outside area, we modified the sentence as follows:

“Further towards the estuary mouth, differences reached a minimum of 15 cm, which decreased towards the outer area.”

L335: A simple time series plot of WLs at one or more locations would be easier and more appropriate and informative instead of the right panel of Fig. 6.

Thank you for this remark. We agree that a time series of the area highlighted at the right panel in Fig. 6 is more intuitive and informative. We therefore replaced all figures of the right panel with time series of all three scenarios, at three different locations. These points are included in each map of the left panel, as shown below. We hope that this modification considerably improves the figure and provides a better insight on the flood timing.

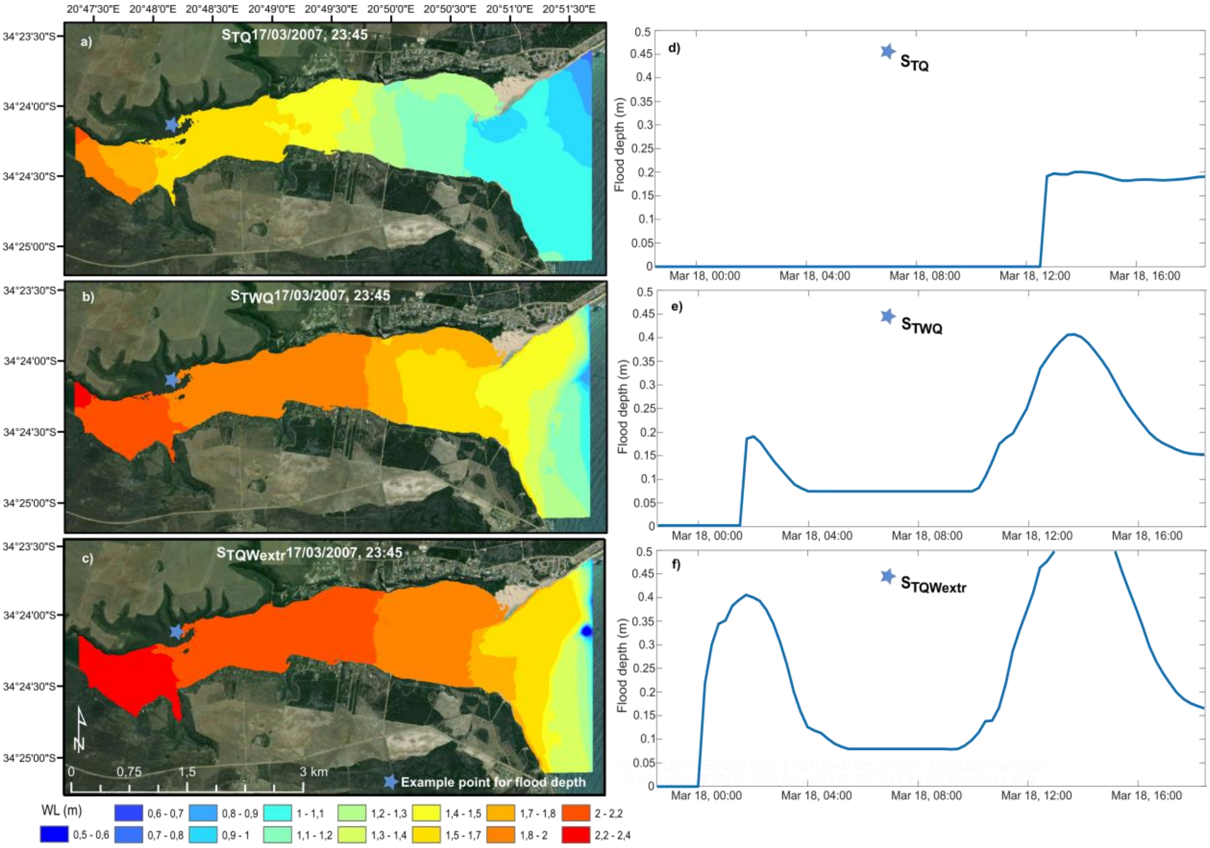

Fig. 6: WL maps of the scenarios S_{TQ}, S_{TWQ} and $S_{TQWextr}$ at the same time step (left panel) and time series of all three scenarios, including the timing of the peak, extracted from the point, shown in the maps of the left panel.

L377: Should “flooding” not be “flood drivers” in this sentence?

We agree with the reviewer, this was a mistake. We corrected it accordingly.

Additional references:

Cox, A. T., & Swail, V. R. (2001). A global wave hindcast over the period 1958–1997: validation and climate assessment. *Journal of Geophysical Research*, **106**(C2), 2313–2329.

Marcos, M., Rohmer, J., Vousoukas, M.I., Mentaschi, L., Le Cozannet, G. and Amores A.: Increased Extreme Coastal Walter Levels Due to the Combined Action of Storm Surges and Wind Waves. Geophys Research Letters 46 (8), https://doi.org/10.1029/2019GL082599, 2019