Referee Comments #3

Comment	Authors response and changes in manuscript
1. As a general comment, the approaches taken for estimating the wave run-up are rather bold and general. There are definitely a number of not necessarily better but similarly justified choices and I wonder how big the uncertainty from making such choices might be relative to issues discussed in this text. My assumption would be that it is probably a major source for uncertainty. I suggest that this should at least be discussed and conclusions should be put into perspective.	You are correct that we have not justified our choices properly. Taking into account also the feedback from reviewers #2 and #1, there are three main points we need to address: 1) Using the maximum wave as the run-up instead of a value exceeded e.g. 2% of the time. 2) The Hmax=2*Hs approximation. 3) The assumption of full reflection. We will repeat our response to Reviewer #1 below: 1) The choice of Hmax instead of e.g. 2% exceedance value is not a matter of being conservative. It is a choice done to get the results to correspond to "one event". It would be possible to choose a lower value that is exceeded e.g. 25 times. However, when combined with the sea level data the values would not be events, but "25 events", and the probability of 0.4% would not correspond to one event in 250 years, but to 25 events in 250 years and would inevitably lead to some inference challenges. 2) The relation Hmax=2*Hs is not really a conservative assumption. It has its bases in the measurements and theory (Rayleigh distribution). This will be clarified in the manuscript also. 3) The assumption of full reflection is the main conservative assumption. However, we feel it has a valid base, since we have observed fully reflected waves even when wave damping chambers are present. Since the damping chambers are not present everywhere, it is reasonable to assume that the short waves – that were damped by the chambers in the measurements – will be reflected in the same way as the longer waves. This might not be true, but since we have no evidence of the contrary, we feel that this is a valid assumption, albeit a conservative one. We will modify the manuscript to better explain the reasons for 1), better justify the validity of 2) and discuss the assumption taken to conclude 3).

2. I would appreciate if the authors could better motivate the sensitivity experiments described in section 5. I understand technically what was done but cannot see the added value. For the discussion of results and significance of differences, confidence intervals should be provided otherwise statements regarding the significance of the results such as on page 15, line 8 are difficult to assess.	See our response to comment [16] from #2 Reviewer. Our purpose is not to refer to statistical significance in the discussion of results and significance of differences, and we will modify our statements according to this in the "Results" section by e.g. replacing the term "significant" with more appropriate expression (e.g. "The contribution of the waves is now *larger compared to* the situation with the first pair.")
3. Page 5, Lines 6-7: Contribution from rivers to the water balance in particular the seasonal or longer variability should be mentioned.	We will add a mentioning of the contribution of rivers to the water balance in Chapter 2 (in the section describing the long-term mean sea level).
4. Page 5, Line 19: There are higher waves reported for the North Sea in chapter 7 of "State and Evolution of the Baltic Sea, 1952-2005: A Detailed 50-Year Survey of Meteorology and Climate, Physics, Chemistry, Biology, and Marine Environment" (doi: 10.1002/9780470283134)	It is true that there are higher waves outside the Baltic Sea, which is a semi-enclosed basin and rather shallow compared to other seas around the globe. At this point we did not have an access to the referred book but as this paper focuses on the Baltic Sea we therefore in the text have referred to the highest wave that has been measured inside the Baltic Sea (see Björkqvist et al., 2017b).
5. Page 7, Lines 4, 5: Please use projections instead of predictions here and at several other places in the manuscript.	We agree that projections is better term for the mean sea level scenarios and this will be corrected throughout the manuscript as suggested.
6. Page 7, Figure 3 and Lines 1-3: Please explain a bit more detailed. I cannot immediately infer the numbers given in the text from the Figure. Please also mention the baseline; that is, the year relative to which changes were computed.	We agree that our explanation could be more detailed and clear. The purpose of Figure 3 is to show the different shapes of the mean sea level probability density functions for the selected years i.e. the spreading of the distribution towards the future. Figure 3 is drawn from the results of Pellikka et al. (2018) that is published now: Pellikka, H., Leijala, U., Johansson, M. M., Leinonen, K., Kahma, K. K., 2018. Future probabilities of coastal floods in Finland. Continental Shelf Research, 157, 32-42. DOI: 10.1016/j.csr.2018.02.006. To clarify our message, we have redrawn Figure 3 and rephrased its caption (see Figure RC_F) so that the numbers mentioned in the text can be read from the figure. The manuscript text will be changed accordingly, and the baseline will be also mentioned.
7. Figure 4: I would appreciate a comment on the extent to which the extrapolation is justified. The	We don't know of any physical upper limit for the short-term sea level variations. The value that seems to be the upper limit in Figure 4 (around 150 cm) is

data seem to suggest an upper (physically based?) limit of about 150 cm.	due to the fact that the highest observed points are not independent but originate from the same sea level event which lasted for several hours. We have addressed our decision to use the exponential fit by referring to studies of Särkkä et al., 2017 in the text. See also our response to the comment [1] from #1 Reviewer.
8. Page 12, Line 23: The authors introduce “SL-distribution” to refer to sea level variations but mainly use “still water levels” hereafter. This should be made consistent.	This is a good comment. We agree that “SL-distribution” is unnecessary definition and using it complicates the text. Thus we will rephrase the sentences that involve SL-distribution and used F_{SL} instead. The same procedure will be done for sentences including “SL,W-distribution” (i.e. SL,W-distribution will be replaced by $F_{SL,W}$).
9. Page 13, Table 1: Prediction should be replaced by projection. Confidence intervals would be helpful.	We will replace “prediction” with “projection” as suggested (see also our response to your comment [5]). We agree that confidence intervals would be helpful. However, calculating them would require more in depth analysis of the uncertainties of the sea level distributions (short and long-term), which we decided to leave outside this study where the main focus is to present the method for combining the sea level distributions with the wave distributions.
10. Section 8 “conclusions” is rather a summary of results.	We agree that the “Conclusions” section was mainly summarizing our results. Thus we will rewrite it to better address conclusions that arise from our results.
11. Page 23, Line 14: It could also be that none of them is eventually realized.	This is a good point and true. The sentence will be reformulated better.

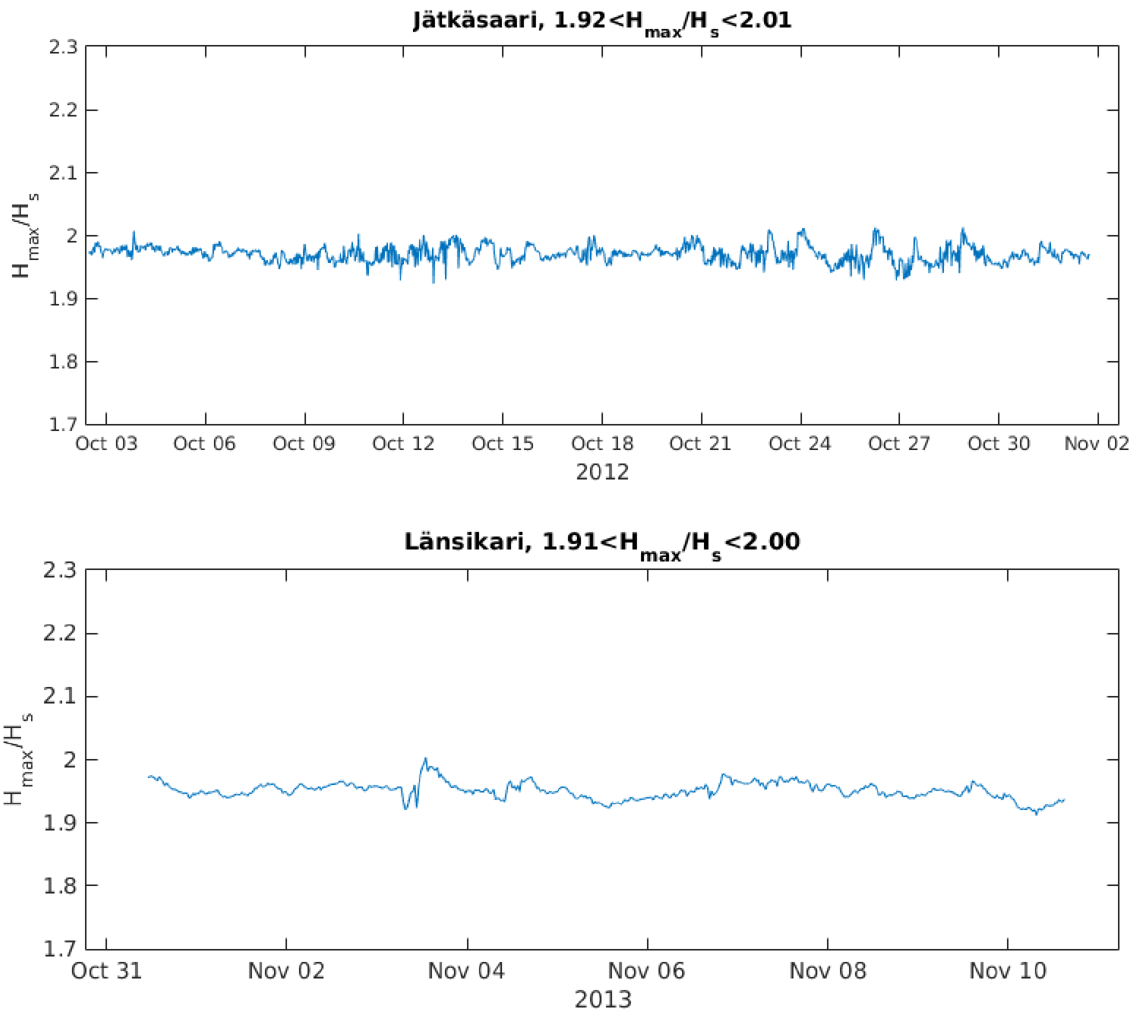

Figure RC_A. The ratio between the highest single wave and the significant wave height estimated from the Rayleigh distribution at Jätkäsaari and Länsikari.

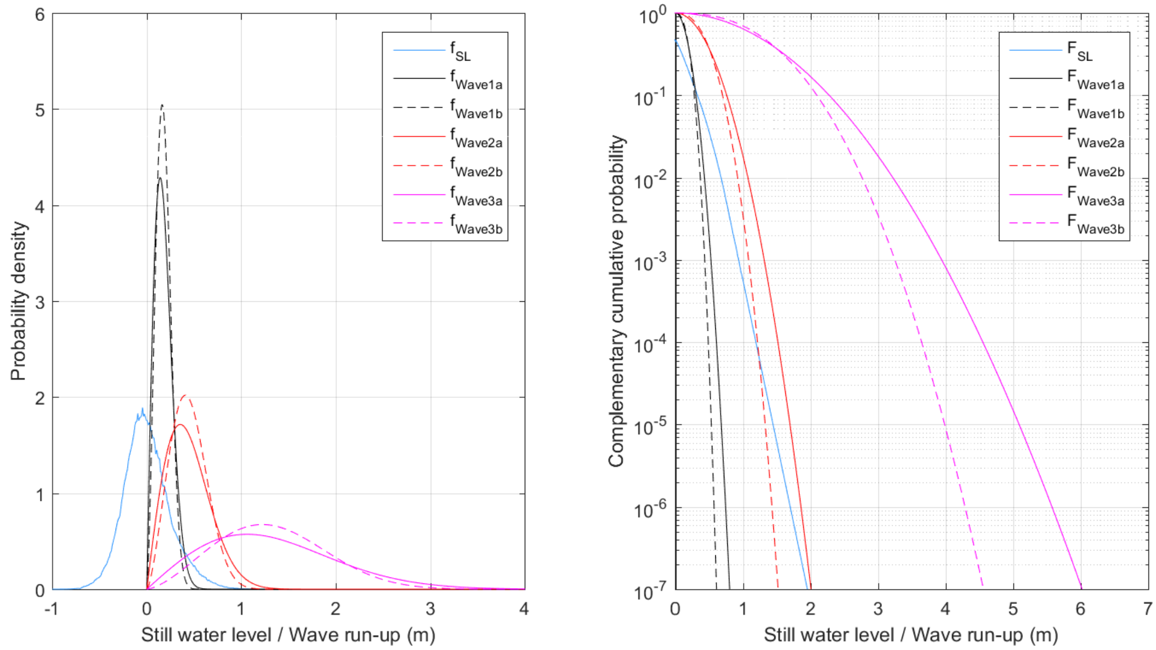

Figure RC_B. Pdfs (on the left) and ccdfs (on the right) for the still water level and the six theoretical wave run-up distributions.

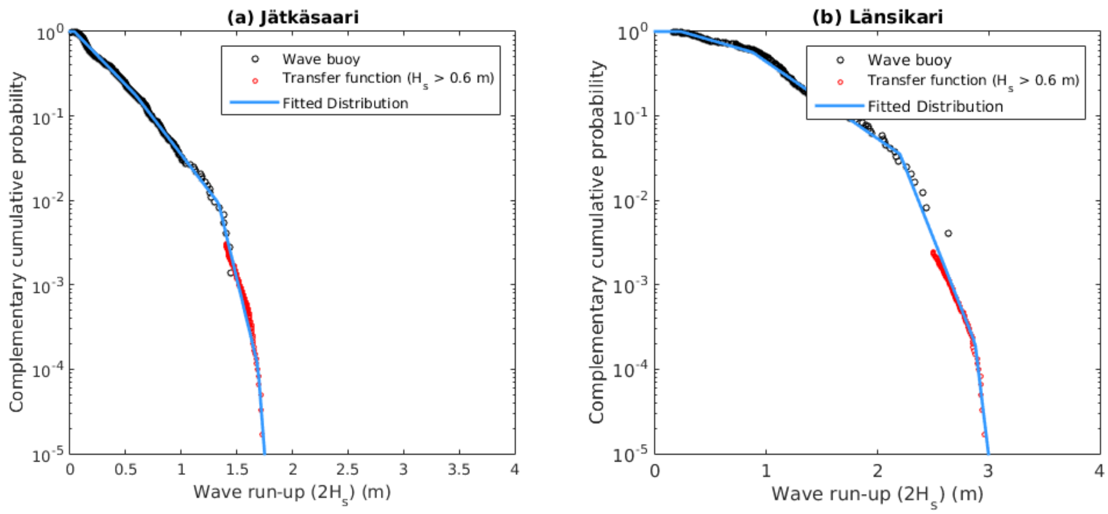

Figure RC_C. Wave run-up distributions for the two locations in the Helsinki archipelago: Jätkäsaari and Länsikari.

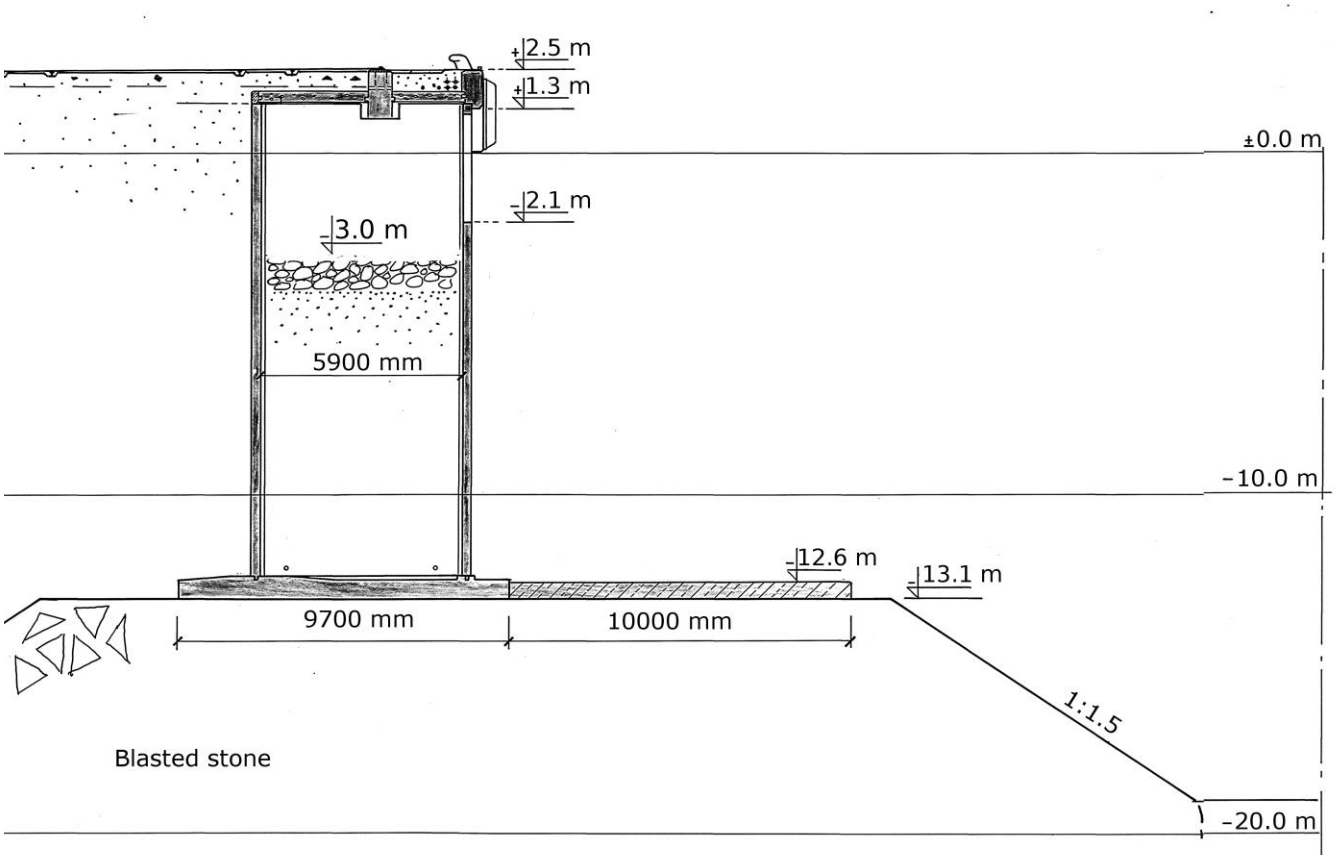

Figure RC_D. The shoreline at Jätkäsaari (from Björkqvist et al., 2017). Other parts of the shoreline are of similar shape (vertical walls), but are not equipped with wave damping chambers.

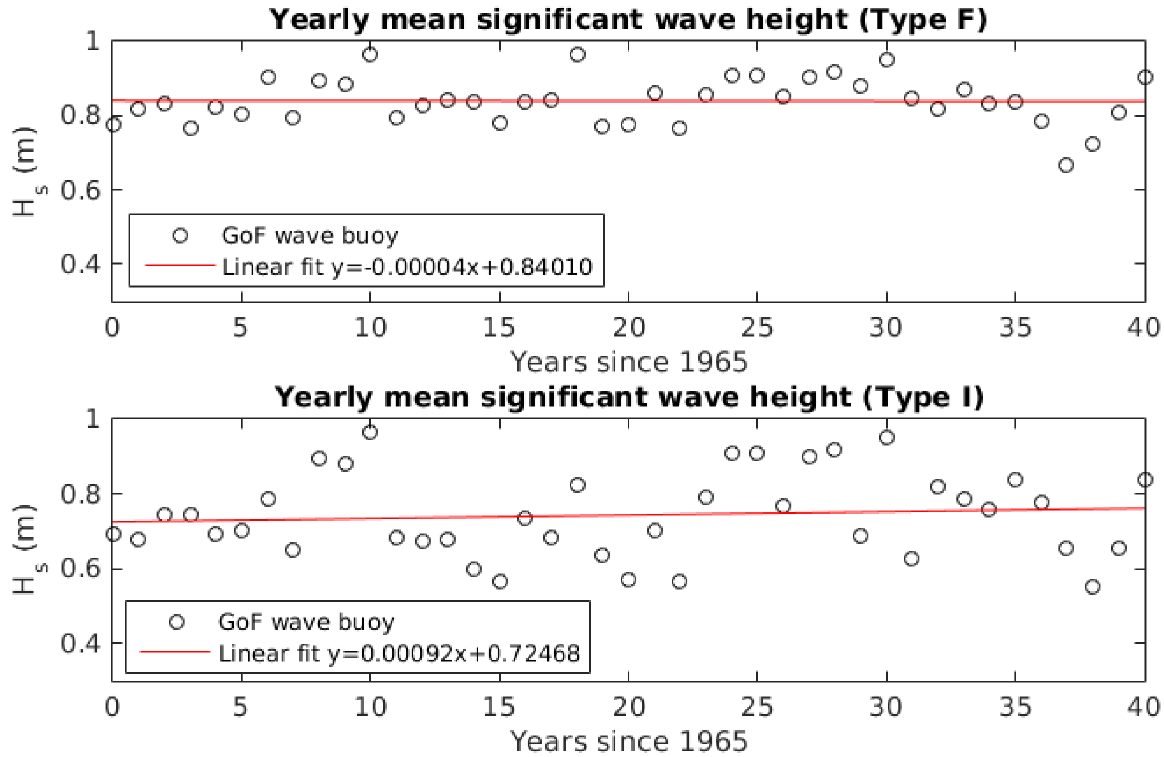

Figure RC_E. The yearly significant wave height at the Gulf of Finland wave buoy taken from the wave hindcast of Björkqvist et al. (2018). Trends were calculated for both the ice-free statistics and the ice-included statistics. Neither was statistically significant.

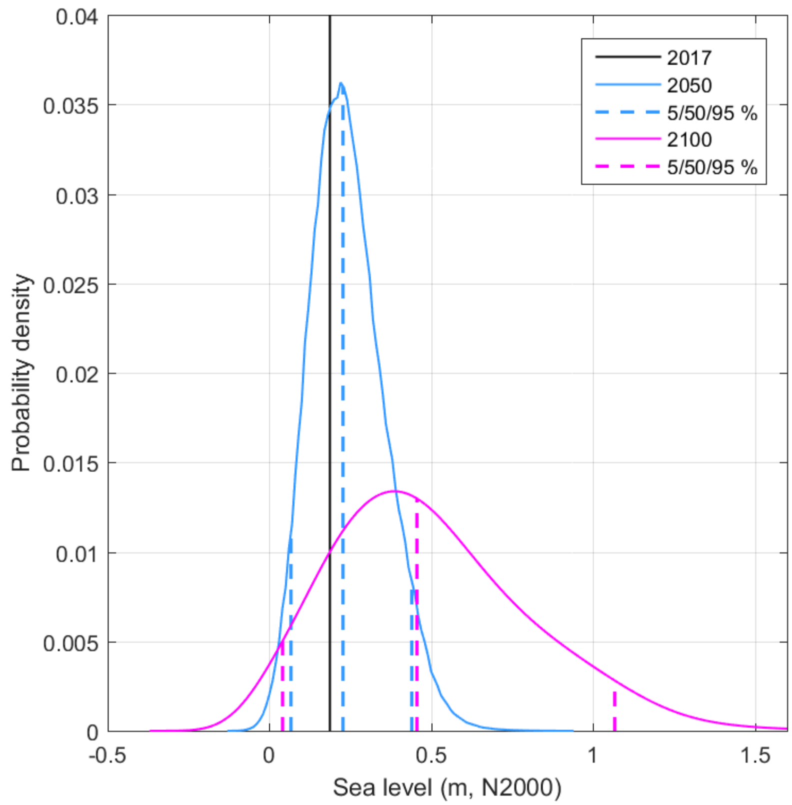

Figure RC_F. Probability density functions of future mean sea level at the Helsinki tide gauge for years 2050 and 2100 and the long-term mean sea level estimate of 0.19 m for year 2017. The 5th, 50th and 95th percentiles are shown for 2050 and 2100. The data in the Figure is from the results of Pellikka et al. (2018).