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Interactive Comment

Interactive comment on "Mapping wave set-up near a complex geometric urban coastline" *by* T. Soomere et al.

T. Soomere et al.

soomere@cs.ioc.ee

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We thank the referee for the professional comments that highlighted some shortcomings in the paper which have now served as a valuable aid to improve the manuscript.

The major concerns of Referee 1 were (i) the lack of the wave hindcast model validation at nearshore locations and (ii) a need for more rigorous analysis of the simulated results (i.e., from 30-yr setup calculations to obtain return periods and analyze the trends).

We agree that the discussion paper does not provide any detailed information about the validation of the wave hindcast model. Although this issue is already extensively discussed in this paper based on earlier research, we reworded the relevant parts of text and added several comments following the review. Several implementations of the

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WAM model with various resolutions have been thoroughly validated for the Baltic Sea conditions. The common understanding of the wave modelling community, expressed on lines 4–7 of p. 1658 the discussion paper, is that this model gives good results in the Baltic Sea basin provided that the model resolution is appropriate and the wind information is correct. This conjecture is supported by several references to papers in which such a validation has been performed (Soomere, 2005; Soomere et al., 2008a; Tuomi et al., 2011, 2012). Therefore, to our understanding, there is no need to once more validate this model. This said, we have changed the wording of this paragraph with the aim of making it explicitly clear that model validation results can be found in the listed publications.

The key question in using any wave model is the quality of wind information. We discuss this issue extensively on page 1658 of the discussion paper and explain why we use the (very best available) wind information from Kalbådagrund (p. 1659). The output of the particular three-level nested implementation of the WAM model, using the described method for the reconstruction of wave fields, has previously been compared against measurements in the Tallinn Bay in (Soomere, 2005). We have added a relevant comment to the end of par. 3, page 1659.

A large part of the additional analysis suggested by Referee 1 (return periods) is basically equivalent with, or easily obtainable from, results that we present. A return period is essentially an inverse of the relevant higher quantile. We prefer the language of higher quantiles as the conclusions about (shorter) return periods can be made this way without employing assumptions about analytical expressions for the distributions of set-up heights. Very little information is available about these distributions and their detailed analysis is, albeit interesting, beyond the scope of this paper. However, we consider adding to Fig. 11 the quantiles that correspond to set-up values with return periods of 2 and 10 years.

The reasons why we have not checked for the trends in set-up heights are discussed in Section 4. First of all, reaching the adequate climatology of high set-up events in areas with complex geometry is very complicated because the return period of unfavourable combinations of wave properties is substantially larger than that of just high waves alone. In particular, the prediction of high set-up requires a proper replication of wave periods (which is a challenge even for the very best contemporary wave models) and wave propagation directions.

Another reason is that a high set-up is only an issue if it occurs simultaneously with a high storm surge; otherwise it is a sort of ‘non-event.’ Therefore, it only makes sense to evaluate the return period of joint impact of storm surge and high set-up. This problem is out of the scope of current paper and we plan to perform such an analysis in the future

Thank you for highlighting the issue of the possible variable values of the breaking index and the need for a suitable reference (Lentz and Raubenheimer, 1999). We discuss extensively the validity of the particular choice of the breaking index (0.8) on lines 5–19 of page 1662. We are well aware, and explain this also in the discussion paper, that this parameter may easily vary in the range of 0.5–1.5 at the breaker line. We, however, admit that we only briefly mention the possibility for this parameter exhibiting considerable changes within the surf zone (lines 5–8, page 1662). This is an important detail for the reader to know, and we have added a relevant explanation into the 2nd paragraph of page 1662.

The comment of Referee 1 concerning the water level decrease (p. 1653, lines 13–15) probably results from a misunderstanding. We talk here about wave-induced set-down. The common opinion is that it only occurs in areas of finite depth and reaches a maximum value (minimum water level) at the breaker line. To avoid any misunderstanding, we now use the prefix “finite” instead of “reduced” in the revised version.

The comment addressing linear and nonlinear features of surface waves (lines 16–17 on page 1653) probably reflects a slightly different usage of the word “nonlinear” in different communities. We of course agree that wave breaking is strongly nonlinear process. We, however, are of opinion that the related phenomena, including wave set-up, cannot be classified as linear, even if it is possible to parameterise some of their properties based partially on linear wave theory. This is specifically our point: to clarify to the reader that set-down and set-up have a radically different nature, although they can be both quantified using the concept of radiation stress. In order to make this clearer, we say now "It is well known that *even almost linear* ocean waves produce a mass transport . . ."

Thank you for indicating the unused literature resources in the comment addressing the material on lines 1–3 on page 1654. We do discuss the potential dependence of the set-up magnitude on the nature of the coastal environment, albeit in slightly different (and quite radical) context of Dean and Bender (2006), in the discussion paper (lines 1–3, page 1654). Of course we are happy to expand this discussion towards the general features of bottom roughness (Apotsos et al., 2007) and towards the observation that the largest role in the formation of set-up (as we assume in our analysis) has the cross-shore component of the radiation stress (Apotsos et al., 2008). The relevant comments are also added into Section 3.2.

The potential overestimation of the cross-shore component of the radiation stress in our approximation (Feddersen, 2004) is discussed in the revised version in the first paragraph of page 1663. This overestimation (by about 10–12%) is balanced by the evident underprediction of the breaking wave height by a simplified version of Eq. (4) (Dalrymple et al., 1977; Dean and Dalrymple, 1991).

On line 14 of page 1653 we replaced the words "This process... " (that may be interpreted ambiguously indeed) by "The propagation of such [even almost linear] waves results in a decrease in the average water level..."

To the end of the sentence on lines 16–17 of page 1654 we have added reference (Dean and Bender, 2006) that was originally placed after the next sequence in the discussion paper.

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Regarding the comment about the nomenclature on pages 1660 and 1662, we use commonly accepted practice of denoting depth by lowercase “d” and wave height by lowercase “h”, with possibly associated subscripts. In order to be consistent, we reformulated the expression on line 5, p. 1660. To remove any possible misunderstanding, we have now used the subscript “S10” on line 16 of p. 1662 in the revised version.

As discussed above, we only partially agree here with the comment of Referee 1 about our choice of the breaking index. The breaking index for waves at the breaker line usually varies in the range of 0.6–1.3 according to references in (Lentz and Raubemheimer, 1999), and in a much larger range according to some other authors, while smaller values are often observed within the surf zone (e.g., Sallenger and Holman, 1985). We discuss this issue in detail on lines 5–19 of page 1662 and hope that it is clear to the reader that we rely on a particular assumption. We also say explicitly that “This change [= the particular value of the breaking index] may, to some extent, affect the numerical values of the wave set-up at specific, individual locations but evidently does not change the location of areas of high and low values of the set-up” (lines 8–10 on page 1662). There is no need to specifically discuss the sensitivity of the results on the particular choice of the breaking index as in the frame of the commonly used parameterisation employed here (see also Lentz and Raubenheimer, 1999), all the results linearly depend on the chosen value of the breaking index.

We do not agree with the comment about assumption (1) on line 1, page 1662. As Referee 1 has pointed out above, the estimates of set-up height may substantially depend on the (spectral) bandwidth, distribution of wave heights and directional spread of the approaching wave field. As we do not have adequate information about these properties, a reasonable way forward is to use the approximation of monochromatic waves. Furthermore, the method for evaluation of the parameters of breaking waves is invariant with respect to the interpretation of the wave height. The issue of the particular choice of the breaking index has been extensively discussed above.

As recommended by Referee 1, the entire text has been corrected to remove any ambiguities of language. In particular, the expressions on lines 5–6 and 17 of page 1654 have been adjusted and the above-mentioned references have been added.

In addition to the above changes, we have corrected the phrase “constant term” into “leading term” on line 19 of p. 1664 and also removed an error in Eq. (5) and obvious errors in expressions on lines 20 and 21 on page 1664.

The changes to the manuscript (except for some minor adjustments; see the supplement) are indicated using blue font.

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Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/1/C513/2013/nhessd-1-C513-2013-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 1, 1651, 2013.