Author's reply

Firstly, we wish to thank Referee 1 for the extensive explanations of their arguments, which are very valid and convincing, it is much appreciated. While the authors feel that using the distance grid in the beaching parameterisation for scenario 2 provides clearer results, the claims of increased precision and improvements have been removed, because as Referee 1 points out, more data would be required to objectively substantiate the claims, which is beyond the scope of this article. Also as Referee 1 mentioned, the inconsistencies between the hydrodynamic and the real coastlines are now highlighted in the manuscript. We do state, however, that in the absence of such high-resolution hydrodynamic data that can resolve individual piers, groynes and other small-scale structures the distance-to-shore parameterisation can provide a suitable compromise when looking at beaching patterns and comparing to using hydrodynamic data alone where such structures are not visible, as well as providing uniformity of the coastline when using varying resolutions with nested grids. We also acknowledge and mention the possibility that using the distance-to-shore parameterisation can introduce artificial convergence artefacts and include a figure in the Appendix as suggested by Referee 1 to this effect. We believe we have now addressed and accommodated all of the concerns that Referee 1 has highlighted and wish to thank them for their thoroughness and detailed approach, and for providing such well-rounded arguments that have enriched this manuscript.

Replies to the individual comments are in blue. The concerns in the referee's major comments are worked into the replies below.

Suggestions

In summary, I think the comparison between scenarios 1 and 2 is interesting and publishable. My concern is that the manuscript argues that scenario 2 is an *improvement*, and I cannot see any evidence supporting this claim, beyond the elimination of certain artefacts. I would recommend that the authors remove claims that scenario 2 improves the 'precision' (and certainly 'accuracy') of predictions or, at the very least, acknowledge that the scenario 2 parameterisation is inconsistent with the hydrodynamic coastline, and therefore runs the risk of introducing new artefacts (a detailed investigation of which is beyond the scope of the present study). The paragraph starting on line 510 and, in particular, the sentence starting on line 612, may be an appropriate place to mention this inconsistency, and that the fine-scale flow around groynes, piers etc. will also not be resolved.

From the concerns and recommendations, the changes proposed by Referee 1 are included in the discussion and conclusion. The paragraph starting on line 510 deals with the resolutions in the hydrodynamic data, and a more suitable place for the inclusion of these points is further on, in the last two paragraphs of the discussion.

Lines of concern that I would strongly recommend removing or revising are as follows:

- L17: "…represent deposition patterns with greater precision of particle beaching locations using high-resolution shoreline data"

Lines 15 to 18 in the abstract are changed to:

LOCATE can effectively integrate high-resolution hydrodynamic data within nested grids to model the dispersion and deposition patterns of particles at coastal scales using high-resolution shoreline data for shoreline detection uniformity.

- L600-605: "…but to provide a more reasonable prediction of where beaching can occur with greater certainty and precision, especially at coastal scales. In practical terms, employing a distance-to-shore parameterisation and high-resolution hydrodynamic data could be more effective at identifying which beaches around the Barcelona metropolitan area could be more impacted by a discharge event after heavy rainfall, where small-scale structures were resolved as seen in Fig.8h. Other scenarios do not resolve structures at small scales, making the quantification of beaching at specific locations more difficult."

The last two paragraphs of the discussion with lines 598 to 614 in the previous manuscript have been changed to:

The main purpose of this beaching parameterisation is not to predict that beaching occurs on the real shoreline, but to provide a consistent coastline independently of the hydrodynamic resolution used when nesting grids. Additionally, the interpolation and grid nesting capabilities of Parcels allowed distance calculations not to be limited by a decrease in spatial resolution throughout the domain. Although small-scale structures are seemingly resolved using this parameterisation allowing for quantification of beaching at specific locations with much less difficulty than other scenarios, it is not consistent with the hydrodynamic coastline. Therefore, the flow around the sub-grid scale features resolved may not be based on physical processes and the localised effects these structures could have on the hydrodynamic data are not considered. Additionally, the potential for the introduction of artefacts from artificial convergence cannot be ruled out in areas where the hydrodynamic coastline and the real coastline based on high-resolution shoreline data converge. Whether these inconsistencies have material effects on the prediction of beaching patterns remains an area for future work. Other limitations of this scenario include the dependency on the availability of high-resolution spatial data and the requirement of preprocessing steps. In the absence of hydrodynamic data of such a fine resolution that may counter these shortfalls, this beaching parameterisation can provide a suitable compromise for small-scale studies and could lead to the development of further parameterisations at beach level in future research. It is crucial to underscore that the considerations for using a distance-to-shore beaching parameterisation are especially relevant for small-scale or localised studies where stakeholders may prioritise identifying specific at-risk areas. In contrast, concerns at a larger scale may differ significantly and the parameterisations used in scenario 2 may not be as useful or meaningful then.

- Line 638: "…and particle accumulation zones"

Lines 636-639 in the previous manuscript have been changed from:

Despite these constraints, the LOCATE model effectively integrated high-resolution hydrodynamic data using nested grids around areas of high interest and used high-resolution shoreline data, providing greater confidence and precision in the detection of the land-water boundary and particle accumulation zones, which becomes more salient the smaller the scale of the study

Changed to:

Despite these constraints, the LOCATE model effectively integrated high-resolution hydrodynamic data using nested grids around areas of high interest and used high-resolution shoreline data to provide land-water boundary detection uniformity throughout the domain when using varying hydrodynamic resolutions.

Minor comments

- I am still not sure what the point of equations 3 is, given that this study assumes K is a constant. I would recommend just giving equation 4, whilst acknowledging that this is a simplification.

The authors insist on keeping the general equation 3 for context as is. Although the K value is constant in the study due to no other data being available as discussed in the previous round of comments, should this change then it would be relevant to see how equation 3 is simplified to equation 4. Given that so many other points suggested have been included, the authors wish to keep this one as is.

- From the authors' response, it does not appear that an understanding of the temporal variability in debris input is relevant to the interpretation of results in the manuscript. I would suggest moving Figure 4 to the supplementary materials for brevity.

As per this suggestion, the figure has been moved to the Appendix

- Concerning Figure 8, the authors wrote in their response that they would use a divergent colourmap. However, the figure still uses a sequential colourmap (e.g. see the cmocean or cmasher packages).

Apologies for this oversight in stating that, this was tried but did not work well, and I forgot to change the comment. For a divergent map, the lightest shade should be at 0, provided the scale would diverge similarly from that point. However, the scale goes from -10 to 2, with the midpoint at around -4 which doesn't make much sense, and having a light shade at 0 offers very little contrast with the background. The authors feel the figure is fine as is using the viridis colourscale which shows the distance range appropriately.

- Concerning Figure 9, the authors state in their response: "and while there may be particles showing blue dots on the left side plots, the concentrations may be low and not be enough to show on the heatmaps". However, the colour bar for the concentration maps start at 0, indicating that all cells with at least one particle (> 0 km^{-2}) should be coloured.

This figure now shows the concentrations starting at 1 per km^2. Also, in the previous round of revision, Referee 1 commented on the simulation snapshot for 31 May 2017. Upon double checking (again), the referee was correct in that the maps did not align, and has now been replaced with the correct day that now corresponds to the concentration heatmap. This was due to an oversight in zero indexing of days from the beginning of the month when producing the snapshots.

- I would recommend adding a line at x = 1 to the rightmost panel of Figure 10 (since this is equivalent to both grids being the same. Alternatively, consider changing the x axis to a logarithmic scale.

The x-axis of Fig 9c (formerly Fig 10) is now on a logarithmic scale and is easier to interpret. With thanks to Referee 1 for the suggestion.

Technical comments

- Line 408: "Additionally, small-scale structures, such as piers and groynes do not seem to be

considered” – I would suggest changing ‘do not seem to be’ to ‘are not’.

This has been changed.