Comments on revised manuscript

I thank the authors for their comprehensive responses to my comments on their manuscript. The revised manuscript has addressed most of my concerns, and the purpose of the manuscript is now clear (and convincing). I continue to disagree with the interpretation of the perceived benefits of scenario 2 over scenario 1, but consider the results to be robust and would therefore recommend this manuscript for publication, pending minor revisions. In particular, I ask the authors to acknowledge the potential for scenario 2 to introduce artefacts due to the inconsistency between the hydrodynamic coastline, and the coastline used in the beaching parameterisation.

Major comments

In their response, the authors state that the main purpose of the beaching parameterisation is "to predict where beaching occurs along the coastline with greater certainty and precision". I agree. This being the case, the authors make the following arguments in support of their favourable interpretation of scenario 2:

1. The hydrodynamic grids do not resolve key geometric features of the coastline such as piers and groynes, which may significantly affect beaching patterns. A beaching parameterisation that accounts for these unresolved features using high-resolution coastline data therefore improves the accuracy of beaching distribution predictions, particularly for small (beach-scale) studies.
2. Using the hydrodynamic grid to infer beaching generates artefacts, such as at domain boundaries and grid cell corners.

Point 1 – unresolved features

On pages 20-21 of their response, the authors demonstrate how scenario 1 is unable to resolve beaching on many of the fine-scale features of the Barcelona city area, such as groynes and piers. In contrast, the pattern of beaching in scenario 2 (pages 22-23) resolves all these features.

Superficially, the figures on pages 22-23 look like a significant improvement. The problem is that figures 22-23 are based on a coastline and hydrodynamics that are <u>physically inconsistent</u>, and I therefore do not think it is obvious that this improvement is 'real'.

From the figure below (page 21), we can clearly see that these piers are not resolved by the hydrodynamic model. The hydrodynamic model therefore (obviously) assumes these piers do not exist, and simulates currents that can straight through them. If the hydrodynamic model resolved these piers, this would not be the case – currents would be <u>deflected</u> around the piers. Of course, beaching could still occur on these piers if the currents were deflected (e.g. by Stokes drift, wind, getting stuck in rocks, etc) but these are different from the process driving beaching in scenario 2 simulations, which is artificial convergence due to mismatch between the coastlines assumed by the hydrodynamics, and by the beaching parameterisation. Scenario 2 would predict a trajectory like the red arrow in the figure below. A particle moves parallel to the model coast, attempts to pass through a pier (because the hydrodynamic model isn't aware of it), and then 'beaches' as it reaches the pier. Instead, it is entirely possible that the 'true' current is deflected around the pier, and that the particle doesn't beach. Without resolving the pier in the hydrodynamic model and/or having data for validation, we do not know which one is true.

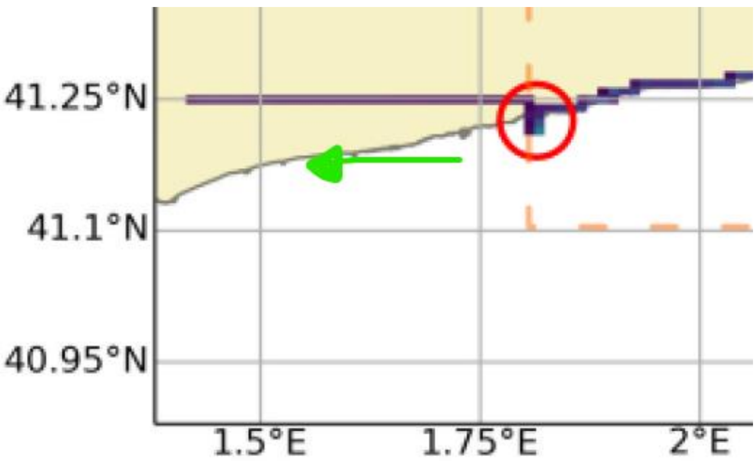
Fundamentally, we have no useful data beyond the resolution of the hydrodynamic grid. Figures 22-23 look nice and neat, but they're not based on physics. Attempting to resolve sub-grid scale features might make the results look more realistic, but that doesn't mean they necessarily are more realistic.

The authors state that "having a model that predicts that debris becomes beached several kilometres inland renders it useless in knowing which beaches in or around the city would be more affected by debris". This is correct, but imposing a high-resolution coastline isn't necessarily going to change this, since we are not resolving the relevant physics. This is why many studies pass particle distributions through a Gaussian filter with a standard deviation several times greater than the model resolution, in recognition of the fact that a model cannot meaningfully resolve structures at or below grid resolution (e.g. Mitarai et al., 2009).

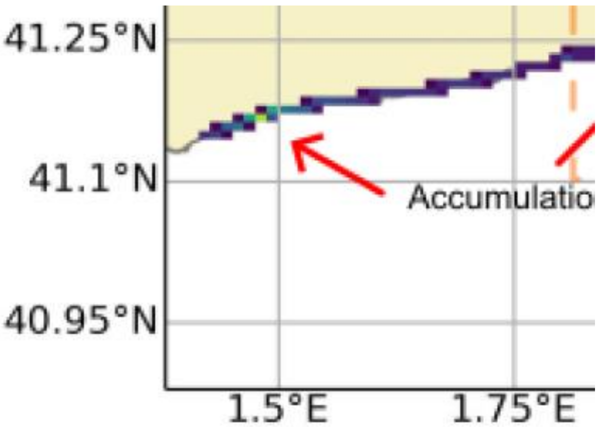
Maybe scenario 2 does improve the accuracy of predictions – perhaps the inconsistency between the hydrodynamic and parameterised coastline isn't a big problem. However, unless the authors have evidence supporting this (e.g. in the form of fine-scale debris observations), we have no way of knowing this.

Point 2 – artefacts

This second point is interesting, and the particle density plots on page 26 of the authors' response is particularly useful – I would recommend adding this to the supplementary materials. Although I agree that scenario 2 appears to reduce some artefacts (particularly the cell-corner and domain boundary 'hotspots'), for the same reasons as explained in point 1, how do the authors know that it hasn't introduced new artefacts? As an example, from this figure, we can see that the model coastline is inland of the true coastline, and based on the particle distribution, we can guess that there is a westward coastal current. Ignoring Stokes drift, the flow should be broadly parallel to the coast (arrow below). And indeed, in scenario 1, we therefore see very low particle density along the coast here.



However, with scenario 2, particles will beach as they intercept the 'coastline' as they travel westwards. This convergence with the coastline is not real. If the hydrodynamic model was aware of the real coastline, the currents would be different. Indeed, the accumulation hotspot indicated by the authors in the figure below is entirely consistent with this hypothesis of artificial convergence with the 'true' coastline. The higher beaching amount under scenario 2 (Table 4) is also consistent with this, due to this artificial convergence (and therefore beaching). Without in-situ data for comparison, there is no way of knowing whether the prediction of accumulation around 1.5E, 41.17N is an improvement over the Scenario 1, or entirely artificial.



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.

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"

- 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."
- Line 638: "…and particle accumulation zones"

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.
- 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.
- 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).
- 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.
- 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.

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'.

References

Mitarai, S., Siegel, D. A., Watson, J. R., Dong, C., & McWilliams, J. C. (2009). Quantifying connectivity in the coastal ocean with application to the Southern California Bight. *Journal of Geophysical Research: Oceans*, *114*(10), 1–21. https://doi.org/10.1029/2008JC005166