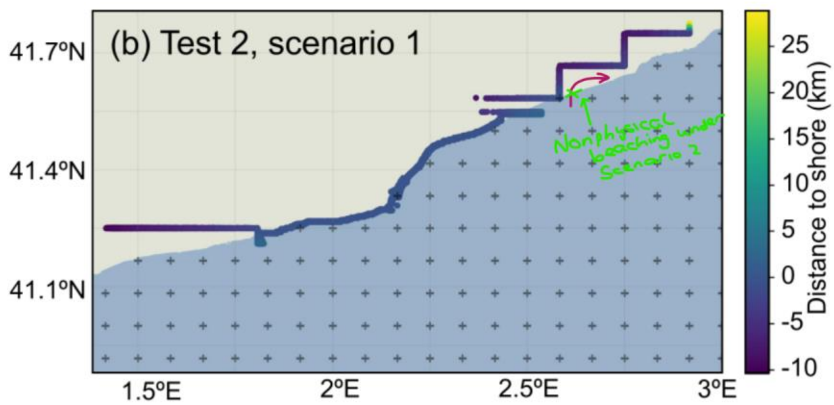
I thank the authors for their detailed response to my review. The purpose of the manuscript is now clearer to me, and I agree with the authors that a well-documented and transferable modelling system including beaching would be a useful contribution to the modelling community. This resolves one of my major concerns about the manuscript. I agree with most of the authors' responses to most other points raised in my review, and look forward to reading the revised manuscript.

However, I am writing this pre-emptive comment as a few points in my review may not have been made clearly enough, and I would like to clarify a couple of (significant) remaining concerns before the manuscript is revised.

Most importantly, I am still not convinced by the manuscript's claim that beaching scenario 2 is an improvement over beaching scenario 1. This is implied at several points in the manuscript, and is stated explicitly in lines 551-559. The only justification for this claim appears to be Figure 7, based on the beaching locations in scenario 2 conforming best with the real coastline (which is obvious, because scenario 2, by definition, only allows beaching at the real coastline – this outcome is predetermined). However, the purpose of this model is presumably not to produce something that *looks* realistic, but rather something that has skillful and useful predictive capacity (e.g. predicting accumulation hotspots). Predicting that beaching occurs at the real coastline is not useful, since we already know that. I do not think it is obvious that scenario 2 would have improved performance for predicting things stakeholders would be interested in (e.g. accumulation hotspots), because the 'real' coastline is not consistent with the hydrodynamic model grids.

For example, it is clear by comparing the top-right of Figs 7(b) and (d) that the CMEMS-IBI grid has ocean cells that intersect with the 'real' coastline. The hydrodynamics of CMEMS-IBI are blind to the 'real' coastline, however, so particles can travel into the 'real' coastline despite following nondivergent flow. Under scenario 2, where particles beach as soon as they reach the 'real' coastline (and setting aside the effects of Stokes drift), if there were, say, a NE-ward along-shore current, this would result in a convergence of particles beaching (as particles are being carried into the coast by the currents). This behaviour is <u>not physically meaningful</u>, as from the perspective of the hydrodynamic model (and therefore the underlying physics), the particles are not converging against the coast.




If I were going about evaluating these beaching scenarios, I would plot the density of beached particles per unit length of coastline, along the coast. It is obvious that scenario 2 will generate beaching locations that conform well with the coastline. It is not obvious that scenario 2 can predict which areas are high and low risk for beaching debris. It is of course entirely up to the authors how they wish to compare these beaching scenarios, but I do not see how the manuscript, in its current form, can make a justified recommendation about which scenario is 'best'.

The other clarification I wanted to make was on the diffusive parameterisation. I did not intend to question the use of $K$ =10 $m^2$ $s^{-1}$ for the IBI-CMEMS grid. My question is why the same value of $K$ was used for the finer (coastal and harbour) grids, which should have a much lower value of $K$, or none at all?