Hernandez *et al.* Review

General comments

Hernandez *et al.* introduce a modelling framework (LOCATE) for simulating marine dispersal in coastal regions using nested hydrodynamic grids. The authors apply LOCATE to a nested model of the Barcelona coastline (forced by a regional CMEMS model), and compare trajectory forecasting skill and beaching behaviour between the (higher resolution) nested model and (lower resolution) regional model, as well as the sensitivity of beaching to three beaching parameterisations.

Although LOCATE has been used in previous studies, this manuscript represents a considerable effort to document, describe, and illustrate a potential application for the framework. This is a great example of 'open science', and the quality of figures is also very high. Unfortunately, in its current form, the manuscript suffers from a lack of purpose and clarity, and feels more like a detailed supplement to Castro-Rosero *et al.* (2023). If the main purpose of the manuscript is to facilitate the wider adoption of LOCATE, the manuscript should clearly explain the advantages of LOCATE over just using Parcels directly, since the nested grid capability of Parcels is already quite accessible (i.e. if I were to investigate dispersal across nested grids, it is not clear to me why I would use LOCATE rather than 'pure' Parcels). If the main purpose is to instead explore the utility of nested grids for particle tracking in coastal waters, more thorough validation is needed than comparison with a single drifter profile. I realise that there is limited observational data available given the small domain size (using drogued drifters may help), but many of the manuscript's claims about "model accuracy" are weakly supported by evidence, and the manuscript therefore provides limited insights into the (dis)advantages of using nested grids beyond "there are differences". I also have concerns about some aspects of the methods, and other aspects were very difficult for me to understand.

If the authors can address the general comments above, then I would recommend major revisions to the manuscript (please see the attached document for specific and technical comments).

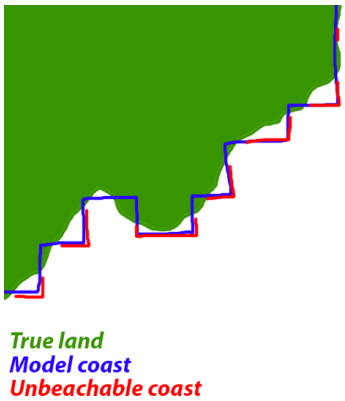
Specific comments

- The abstract is long compared to the number of key messages from the paper. I would recommend condensing the abstract to make it easier for readers to identify the main points.
- Section 1.2 covers the dynamics of beaching debris in great detail, but most is of little relevance to the paper (since most of these dynamics are not investigated). I would recommend condensing.
- Section 1.4 begins by stating that the "goal of the present work is to simulate particles taking into account coastal processes using nested hydrodynamic grids at varying resolutions". I would argue that this is the *method*, not the *goal*. As stated in the general comments, I would recommend clarifying what the actual goal of the paper is.
- Section 2.2: What is the frequency of the Eulerian model output used in LOCATE? Line 159 states that "data are calculated daily" – daily strikes me as very low compared to the timescales over which currents resolved by a 2.5 km model (and certainly 350/70 m) change.
- I cannot see any reference to tides in the manuscript outside of the introduction. Were tides not included in these simulations and, if not, why?
- Section 2.3: Why was model output interpolated to a 'regular' grid (presumably A-grid)? As described in the OceanParcels documentation[1], A-grids introduce interpolation problems whereby particles get artificially stuck at the model coastline, because both velocity

[1] https://docs.oceanparcels.org/en/latest/examples/documentation_stuck_particles.html

components approach zero at the coastline (i.e. particles will always stagnate at the coastline of an A-grid, regardless of the strength of any along-shore currents). Particularly given the focus of this manuscript on beaching, I am concerned about the potential for beaching to occur due to interpolation artefacts, rather than any physical reason. The authors could consider using the original C-grids instead, or the free-slip boundary condition that was recently implemented in Parcels[2].

- Section 2.5: Given that LOCATE uses a constant value for K, most of the detail in lines 233-243 is unnecessary and should be removed. I am also confused by the choice of a constant value for K (10 $m^2 s^{-1}$). Should this not be scale-dependent? Okubo (1971) would suggest that a value of 10 $m^2 s^{-1}$ would be appropriate to reproduce the effects of unresolved scales of motion at a resolution of around 10 km, much coarser than the resolution used in this study.
- Section 2.7: I am surprised by the decision to use undrogued drifters. Drifters lacking a drogue experience a significant direct push from the wind due to the exposed part of the drifter (Poulain et al., 2009), which is not included in LOCATE. Although the authors' concern about velocity shear within the upper water column is valid, there is some evidence that surface currents reasonably represent the forces driving drogued drifter movement (Imzilen et al., 2023). Using drogued drifters may also increase the number of drifters available to the authors for use in validation.
- Section 2.7: I am uncomfortable with the use of the word "validation" for the comparison with a single drifter. Is this really enough data to count as validation?
- Section 2.7: How were drifter trajectories temporally interpolated if there are no timestamps associated with locations?
- Section 2.8: I cannot find a precise definition for the residence time in this manuscript – please clarify how this was calculated.
- Section 2.8: Since the manuscript does not investigate or validate the temporal variability of debris accumulation, I do not see the point of varying the input rate of debris.
- Section 2.9: If I understand correctly that scenario 2 defines the distance-to-shore relative to the true (rather than model) coastline, would this not make it impossible for debris to beach in many places? There are presumably parts of the model grid where ocean cells do not quite reach the true coastline (see below), so it is never possible for the distance-to-shore parameter to reach zero here, and therefore also impossible for particles to beach? In any case, I do not understand the point of using the real coastline in a beaching parameterisation – particles within the simulation do not 'know' anything about the real coastline, they only 'know' about the model coastline.



[2] https://docs.oceanparcels.org/en/latest/examples/documentation_unstuck_Agrid.html#3.-Slip-boundary-conditions

- Section 2.9: I do not understand scenario 3. Lines 533-535 makes it sound like particles beached if they travelled less than 1.694 km in 6 hours, but I am not sure if this is correct.
- Section 3.1: I did not understand this section (particularly what was meant by 'horizons') until I read Révelard et al., (2021). Please clarify (probably in the methods) the methods, i.e. comparing the skill of the model to *forecast* the trajectory of a drifter 6/24/72 h ahead, as a function of when the forecast started.
- Figure 6: How is the 'mean trajectory' defined? Is this just the arithmetic mean of all latitudes and longitudes at a point in time?
- Figure 7: Please use a divergent colourmap for these figures, centred at 0. The use of a sequential colourmap makes it very difficult to tell which particles are close to the coastline, versus which have over/undershot.
- Section 3.2: Please specify which beaching scenario was used for Figure 9 and lines 403-418.
- Figure 8: The left and right panels do not seem to correspond in this figure. For example, lots of blue particles appear in the far SW corner on May 31 (left), but not in the panel on the right.
- Section 4.1: Is it not surprising that the regional grid did not perform considerably worse than the nested grid? This is one of the most interesting observations in the manuscript for me (that the regional grid often had similar performance to the nested grid), and seems to somewhat contradict the manuscript's conclusion that using nested grid improves the accuracy of predictions (although there is insufficient validation in the manuscript to be sure).
- Section 4.2: Line 540 states that, under scenario 1, particles can travel "several kilometres inland before being considered beached". Along similar lines to my comment on Section 2.9, does this really matter? Is it really a problem that beaching locations are 'wrong' by a few kilometres in a model with a resolution of a few kilometres?
- Section 5: The manuscript does not validate any beaching predictions, so there is no data to support the claim that "using real-time particle distance to the shoreline… can accurately model particle arriving time and beaching locations". Similarly, no data has been presented to support the claim that "LOCATE… provides accurate depictions of accumulation zones and debris hot-spots…", at least not in this manuscript.
- Table A1: This table may not set out to be exhaustive but, in case of interest (as this is quite a nice compilation of beaching parameterisations that could be useful for others), some other studies using 'deterministic' parameterisations include Bosi et al., 2021; Cardoso & Caldeira, 2021; Critchell & Lambrechts, 2016; Dobler et al., 2019; Seo & Park, 2020; Zhang et al., 2020. Some other studies using 'probabilistic' parameterisations include Kaandorp et al., 2023; van der Mheen et al., 2020; Vogt-Vincent et al., 2023.
- Did the authors observe any artefacts in particle concentrations along the nested domain boundaries? There are quite striking discontinuities in surface velocity along domain boundaries in Figure 2, and it would be interesting to know whether this causes any artificial particle convergence/divergence at these boundaries.

Technical comments/corrections

- Lines 29-31: The number of references for the physics of marine dispersal is excessive in my view. I would recommend condensing, e.g. just using van Sebille et al., (2020).
- Lines 31-33: It is not clear to me why this sentence is relevant. Lines 31-36 could be removed for brevity.
- Line 40: This is a very specific range (50-600 m). Where is this range from?
- Line 42: Are density gradients really a *driver* of coastal currents (is the driver not the process that generated the density gradients in the first place, e.g. upwelling/downwelling)?
- Line 45: The referenced paper by Stokes was published in 1880, not 2009!

- Line 70: Would recommend changing “high spatial discretisation” to “fine spatial discretisation”.
- Line 82: I do not follow how this relationship is exponential. Computational costs should scale broadly linearly with the area modelled, and a polynomial (not exponential) relationship with the model resolution.
- Lines 160-161: Please separate the references for ROMS, IBI-CMEMS, and SAMOA.
- Line 172: Typo (‘downlaod’)
- Line 174-178: I would recommend giving the resolution of the wave model here (I assume 1/20 degree, based on line 494).
- Line 198: It is not clear what is meant by “typical stochasticity”. I assume the authors meant subgrid scale diffusion.
- Line 242-243: Citing OPeNDAP is unnecessary here (the text is describing the data itself, not OPeNDAP).
- Line 246: Please state or cite where the value (1/3) for the variance of the random process comes from.
- Table 3: It is a bit confusing using the same numbering for sensitivity test and beaching scenario. It may be clearer to remove the sensitivity test numbering, and change sensitivity test 1 to test 1R (beaching scenario 1, river release), test 2 to 2R, test 4 to 1H, etc.
- Figure 6 caption, last line: I assume “(f) to (j) should be “(h) to (l)”.
- Lines 365-402: This paragraph is unrelated to beaching, so should not be in section 3.2.
- Line 404-405: This line implies that no particles were retained (at sea, unbeached). Is this correct?
- Line 406-408: Although it is possible to work this out from context, please specify that “The Prat de Llobregat area *had* 12.7% more particles…” means that 12.7% more particles *beached*. This sentence also makes it sound like these are relative percentages, whereas based on Figure 9 it looks like these are absolute percentages.
- Line 431: Typo (“harbourss”)
- Lines 439-441: Please state for which configuration the SS value was higher (presumably the nested grid).
- Line 483: I would recommend replacing “wave-induced currents” with “wave-induced Eulerian (mean) currents”. The Stokes drift is not a current, but some readers might be confused.
- Line 520: Please specify that “both simulations” refers to the regional vs nested grids.
- Lines 570-571: The manuscript does not discuss the difference in computational cost between the regional and nested configurations, so I would recommend removing this sentence (or quantifying the difference in computational cost, and moving this to the discussion).
- The labels “IBI-CMEMS” and “regional grid” are used interchangeably in this manuscript, which can get confusing. I would stick to one of them.

References

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