Review 2

Main comments

This study concerns the dynamics of floating plastic waste in the Bay of Bengal based on particle tracking simulations. Using a uniform release of particles along coastlines in the Bay of Bengal, the authors find that most particles beach in their country of origin and that the seasonally reversing East India Coastal Current (EICC) is the main driver of particle transport in the region. The study is relevant, and the manuscript is well-written and has high-quality figures. I have two main comments, which I believe can be addressed with additions and clarifications to the manuscript text and figures, with perhaps some additional analyses.

My first main comment concerns the approach to beaching in the study and, as an extension of that, the release locations. Since the connectivity between countries in the Bay of Bengal is determined based on beached particles, I think the assumptions and limitations of the beaching method should be thoroughly discussed in the manuscript. The authors mention in the Discussion that their "model accounts for several processes … key mechanisms thought to drive the beaching of floating particles, such as windage or Stokes drift". They also discuss that the higher resolution ROMS model likely incorporates coastal dynamics more accurately than the global CMEMS model. While I agree with both these points, I think it is important to note that:

1. Although the ROMS model may have a higher resolution and may capture coastal dynamics more accurately than the global CMEMS, both the Stokes drift and wind fields are based on global models and have coarse resolution (I assume ~25 km, although this is not specified). Since these are the mechanisms that are responsible for 'beaching' in the simulations, I think it is important to clarify that these are unlikely to represent fine-scale coastal dynamics and therefore are probably not capturing accurate beaching dynamics of floating particles.

   Additional details about the resolution of the wind and Stokes datasets were missing from the manuscript – thank you for pointing this out – and have now been included in the Methods (lines 128-132):

   "Stokes drift velocities were available in 3-hourly timesteps at a resolution of 1/5°, which is roughly 21 km at the latitudes of the Bay of Bengal; wind velocities were hourly and with a resolution of 1/4°, which is approximately 26 km at these latitudes. Both datasets were interpolated onto the relevant grid for each of the CMEMS and ROMS runs using cubic interpolation and then averaged to daily timesteps."

   Further discussion about the use of these datasets in both simulations despite their coarser resolution compared with the ocean velocity datasets has now been added to the Model limitations subsection within the Methods description (lines 194-197):

   "While the ocean current velocity datasets, particularly the ROMS data, have relatively high spatial resolution for a regional model such as this, the Stokes drift and wind velocities used in both the CMEMS and ROMS simulations are coarser than the ocean velocities. Any differences in beaching between the two simulations is therefore expected to result from the differences in general circulation patterns as opposed to wind and wave effects."

2. The beaching of particles is not simulated explicitly in this study, instead particles simply become stuck on land if they end up on a coastline (approximated in the study by identifying near-zero velocities rather than using a land mask). Since there is currently no consensus on the correct way to simulate beaching, I do not necessarily have an issue with this method, but I think the authors should make this clear in the manuscript. There are several studies that use different (probabilistic) approaches to beaching (e.g. Onink et al. 2021; Irfan et al. 2024; van der Mheen et al. 2020b; all already referenced in the manuscript); it would be good to briefly mention the different approach in these studies and explicitly clarify the approach used in this study in the Methods.

   Different approaches taken by other researchers have now been mentioned in the Model limitations subsection and the method used by others which is most similar to the method we employed (which is stated explicitly earlier in the Methods section) is pointed out (lines 169-177):

   "The beaching process is a critical step in the journey of a piece of marine litter (Hinata et al., 2020a) yet there is no consensus on how best to implement this step in particle tracking models. Some researchers have used a similar method to this study whereby a particle was deemed to be beached when its position was on a land grid cell (e.g. Irfan et al., 2024), whereas others considered particles beached if they persisted in a coastal grid cell for a given amount of time (e.g. Isobe and Iwasaki, 2022). Several other studies have taken the approach of probabilistic determination. For example, van der Mheen et al. (2020a) used a random probability to determine if a particle would beach, so long as it was within a given distance of the coast and that distance was decreasing. Chenillat et al. (2021) and Onink et al. (2021) used similar methods to this. Nevertheless, each of these methods used to determine particle beaching are simplistic and neglect much of the nuance involved with beaching processes in reality. Therefore, this study acknowledges the limitations of using this approach to determine particle beaching."

3. I think it is also important for the authors to clarify whether it is possible for particles to become stuck on land because of ocean currents (which is sometimes the case when ocean currents are interpolated incorrectly along coastlines but is not physically valid) or because of the random motion of particles, or if they implemented some method to only allow particles to end up on land as a result of Stokes drift and windage (which, as the authors also mention in the Discussion, would be the only physical mechanism to result in beaching). If no method was implemented to ensure this in the simulations, it would be useful if the authors could provide an indication of how many particles may become stuck on land (in areas on non-zero velocity) due to model artifacts (e.g. ocean current interpolation and random motion) versus due to Stokes drift and windage, though I appreciate this may be difficult to determine. In addition, the authors use the condition that a particle's velocity is close to zero to determine if a particle has beached. I assume that wind velocities have been set to zero above land? Since the land boundary is effectively identified by zero velocities, does this mean that "land" (or the area of zero velocities) is formed by a combination of the different velocity fields at different resolutions (rather than, for example, taking the land mask from just the ocean current models)? It would be useful to briefly explain this in the methods, and if "land" is indeed identified as a combination of all velocity fields, I think it would be useful to show maps (perhaps in an Appendix) of what this looks like for both the CMEMS and the ROMS simulations.

The inclusion of horizontal diffusivity representing subscale turbulence will result in beaching even if the currents are perfectly interpolated. In the model results it is not possible to discern when beaching resulted for this or from Stokes drift or windage aimed at the shore, but in reality it is not possible to relate this with observations.

Wind over land has not been set to zero as wind does not stop at land boundaries. So, it is true that windage may still contribute to particle motion over land. However, "land" is determined in the model only by near-zero ocean velocities. This was already stated in the manuscript. However, based on the reviewer's comments, we have added further clarification as follows (lines 109-112):

"At the end of each timestep, after advancing each particle's position, ocean velocities were checked at this new position. If the ocean velocity was less than 10^{-14} m/s, the particle was considered to be beached (after Delandmeter and van Sebille (2019)) and was no longer tracked. Stokes drift and wind velocities were not included in the calculations to determine if a particle was beached."

4. Regarding the release locations: I think using a uniform release along coastlines is an excellent method to gain insights into the dynamics of floating plastic in the region, especially (as the authors also highlight) given the large uncertainties surrounding estimates of riverine and coastal plastic sources. However, I do think that the choice in release location (distance from the coast) may have an effect on the simulation results, especially since the emphasis is on the beaching of particles. I think the motivation of releasing particles away from the coast (to not release any on land) but still in the continental shelf region (to hopefully capture some coastal dynamics, rather than only open ocean dynamics) is the correct one. However, in the Methods it is mentioned that "this distance was chosen to complement different coastlines from the two hydrodynamic models". Considering that the coastlines from the hydrodynamic models are not actually used as "land" to determine beaching, I am not sure if this is the correct motivation. I do not necessarily think that this needs changing in the simulations, but I think the wording here gives the impression that the ocean current models determine where beaching will occur, which (if I understood correctly) is not the case since the Stokes drift and wind field velocities also contribute to this. I would recommend showing maps with both the release locations and the zero-velocity "land" that is determined with the combined velocity fields (assuming that the land boundary shown in Figure 1 is a general boundary and not based on the zero-velocity region). Releasing close to the coast and considering the high beaching percentages on countries of origin also raises the question how many particles end up on land shortly after their release (see also my second point for this).

We have altered all figures to show the zero-ocean velocity boundary in each case, rather than the general boundary/coastline (as specified by Cartopy using Python) that was in the previous iteration of the maps. Figure 1 now shows the release locations near to the zero-velocity "land" for both the CMEMS and ROMS simulations.

Only zero-velocities in the CMEMS or ROMS ocean velocity datasets determined whether a particle was considered beached or not. As noted in response to the previous comment, we have altered the text regarding the determination of whether particles were considered beached, in case it was unclear.

We have also included new analysis and figures regarding how quickly particles beach following their release. Details are provided in answer to your second main comment below.

In addition to clarifying the points above in the manuscript, I think at least a paragraph in the Discussion should be dedicated to discussing the limitations of the simulation methods, with a particular focus on the beaching method. I think a discussion of the dependence on beaching on fine-scale local dynamics (which I think are unlikely to be captured in these simulations, despite the higher resolution ROMS model) should also be included. Some potential references for consideration for this are, for example: Pawlowicz et al. (2019), https://doi.org/10.1016/j.ecss.2019.106246; Zhang et al. (2020), https://doi.org/10.1016/j.scitotenv.2020.136634; Hinata et al. (2020a), https://doi.org/10.1016/j.marpolbul.2020.110910; and Hinata et al. (2020b), https://doi.org/10.1016/j.marpolbul.2020.111548.

A "Model limitations" subsection has been added to the Methods (following a similar comment from another reviewer), so that readers can keep these in mind when assessing the results. The limitations of the beaching process in the model were central to this discussion and all of these references have contributed. Thank you for the suggestions.

My second main comment concerns the discussion of the seasonal variations results (section 3.3). Figures 3c-h show connectivity matrices for particles released during the monsoon, post-monsoon, and pre-monsoon but beaching at any time during the simulation. Figure B1 shows the same connectivity matrices but for beaching only occurring during the specified monsoon season. I am not sure that Figure B1 should be in an appendix. I think showing both the connectivity matrices for beaching throughout the simulation and for beaching during the relevant monsoon season is important. For example, in the results for the monsoon season the authors identify that "the second highest beaching rate was always on a country in the anticlockwise direction" in Figure 3c, which does not make sense to me during the monsoon season, since the currents are in a clockwise direction. In Figure B1a this pattern doesn't seem to be as pronounced (e.g. second-highest beaching of particles from India is in Bangladesh rather than in Sri Lanka, which seems to make more sense with the direction of the ocean currents during the monsoon season). The connectivity matrices during the post-monsoon season in Figure 3 and Figure B1 also seem quite different. I think it is important to discuss these results along-side each other, as they can also provide information about how many particles tend to beach within the same season versus during a following season. Similarly, the mention of the different boundaries through which particles exit the region during the different seasons is interesting, but it does raise the question during which season these particles exited the region (not just in which season they were released).

In addition to showing the connectivity matrices, I think it would also be very relevant to show timeseries of the percentage of particles beaching and exiting the region (e.g. different panels per season release and different colours per country release). This would provide insights into how many particles beach very quickly after their release, within the same season of release, and in a different season.

Figure B1 and the related text has been moved into the results section of the main paper. Two additional figures have been added to the paper showing what percentage of particles released during each season beached or left the domain very quickly (within a week), later in the season of release, or later (in a different season). A series of tables have also been added to Appendix D breaking down the numbers of particles exiting the domain via each domain boundary. These figures and tables have been referred to in several locations within the Results and Discussion and additional analysis has been included particularly in Sections 4.2 & 4.3.

Minor comments

I appreciate the validation of the ocean models with the undrogued drifter trajectories. However, I think it is important to make it clear from the start that these drifters provide validation in the open ocean only and not in the coastal ocean (where you may expect the higher resolution ROMS model to perform better than CMEMS). The authors make this clear in the Discussion, but I would recommend also briefly pointing this out in the Methods and Results sections about the validation. I would also be careful about including this in the Abstract, I personally think the sentence "Both simulations were validated using the pathways of undrogued surface drifters, with better agreement found for particles advected by data-assimilated ocean velocity" misses some nuance and may be misinterpreted.

The Methods have been updated to include information about the drifters mostly verifying open ocean dynamics to make the reader aware of this when they consider the results (lines 212-215):

"Most of these drifters began and continued their journeys in the open ocean. Consequently, minimal proportions of these drifter trajectories can be considered to verify coastal dynamics. This is an important consideration given that the CMEMS simulations include data assimilation (for sea level, temperature and salinity) and would therefore be expected to provide more accurate offshore currents than the ROMS velocities."

As this information is now stated in the Methods and Discussion, we have not repeated it in the Results which we wish to keep to the statistics; these results are discussed later on with this information in mind.

The sentence in the abstract has been updated to add some nuance (lines 22-24):

"Both simulations were validated using the pathways of undrogued surface drifters, which moved primarily within the open ocean, with better agreement found here for particles advected by data-assimilated ocean velocities."

The terminology around the monsoon seasons is a bit inconsistent throughout the manuscript (e.g. use of spring, summer, winter in the Introduction; pre-monsoon, monsoon, and post-monsoon in Methods, Results, and Discussion; Northeast Monsoon, Southwest Monsoon, Winter Monsoon in Discussion). I would recommend defining the pre-monsoon, monsoon, and post-monsoon seasons in the Introduction (the monthly definitions are now only given in the Methods). Since there is some different terminology used around the monsoon seasons in the region, it would also be beneficial to include the specific months you are referring to in all Figures and the Table.

The choice to refer to the same season by different names was a consequence of choosing to use the language utilised by other researchers in their respective papers, but we understand this may be confusing to readers, especially those unfamiliar with the monsoon and its seasonal changes. Therefore, we have removed almost all references to either the Summer/Winter Monsoon or the Southwest/Northeast Monsoon and instead refer to the relevant months to be clear about the time of year that is being discussed (e.g. Line 401: "This is in line with van der Mheen et al. (2020a) who found particles in their own simulations of the wider northern Indian Ocean were transported from the Bay of Bengal into the Arabian Sea during December – February.”). The only exception is line 58 where we refer to a season by another researcher’s definition of it and this has been stated explicitly (lines 58-59):

“They concluded that beaching in the Bay of Bengal peaked on the north-northeast coastlines during the Southwest Monsoon (which they defined as June - October) but did not quantify beaching rates for each country.”

I would appreciate some discussion about the choice of using ocean surface currents + Stokes drift + 1% windage to represent the transport of floating plastic waste. It is mentioned in the Methods that 1% windage best represents the effect of wind on drifters (undrogued?) but is there evidence that these drifters behave as floating plastic would? I do not have an issue with the choice of forcing fields, but I do think the choice should be discussed more. For example, while this may represent the transport of many very buoyant plastics (all but the largest, as mentioned in Methods), this may not correctly represent less buoyant or smaller plastics drifting at, or close to the ocean surface. I was hoping to see a sensitivity analysis on different forcing mechanisms, at least with and without windage added. It would be very interesting to see how this affects beaching in the simulations, but I appreciate this may be out of scope for the current study.

The type of sensitivity analysis you mentioned has already been done elsewhere (Irfan et al., 2024), so we do not wish to repeat that here. The methods have been updated to give more details of how we came to the decision to use a combination of ocean velocities + windage + Stokes drift. This is an approach used in other studies (e.g. Chassignet et al., 2021; Isobe and Iwasaki, 2022) and is based on the physical processes that need to be considered for the movement of floating litter (Haza et al., 2019) (lines 92-95):

“The model includes several processes which are believed to be the main physical processes responsible for influencing the movement of floating particles around the domain to simulate the dispersal of buoyant marine debris (Haza et al., 2019). This approach follows similar methods of others to simulate marine plastics distribution (e.g. Chassignet et al., 2021; Isobe and Iwasaki, 2022).”

Plastics of different buoyancies have been found to affect drift behaviour (Pereiro et al. 2018) but undrogued drifters are the closest analogy to floating litter that we can track to validate the model. A new study has used actual plastic bottles fitted with tracking technology (Duncan et al., 2020) but this is an emergent technology, and the bottles weren’t released during our time frame in the Bay of Bengal so we cannot use these as a validation dataset in this study. The drifters referenced in the methods were undrogued and the sentence in question has been updated to state this. An explanation that we do not expect the drifters to behave as all plastics would, because plastic behaviour is affected by shape and density, has been added to the drifter validation subsection of the methods to clarify this (lines 204-210):

“Undrogued drifters would float at the surface of the ocean and are therefore analogous to some types of floating marine litter. The movement of floating litter at the surface of the ocean differs due to factors such as shape and density, particularly with respect to the effect of wind (Pereiro et al., 2018). Therefore, drifters are not expected to represent all items of floating litter, but they are one of the closest analogies that can be tracked to validate the particle tracks in the model.”

Please mention the temporal resolutions of the ocean models in the 2nd paragraph of the Methods as well as the horizontal resolution. This is briefly mentioned later in the Methods, along with a vague reference to sensitivity tests in Appendix A. I think the temporal resolution is important, and the choice to use daily-mean (or is it daily intervals?) velocity fields rather than hourly velocity fields is non-trivial. This should be clarified and discussed up front. Regarding the sensitivity tests in Appendix A, while I do not think that these are critical to the manuscript and its results, I am afraid that I do not find the tests themselves very convincing. Looking at the particle positions in CMEMS, these are quite different between the daily and hourly resolutions (though the ROMS positions are remarkably similar, and interestingly the CMEMS daily positions seem to more closely resemble those from ROMS). Since the sensitivity simulations were only run for a month, I don't think using the beaching connectivity matrices is a valid justification for using the daily resolution. Perhaps a comparison between hourly and daily particle trajectories (as was done with the validation against drifter trajectories) makes more sense here? It is also not clear to me why the authors prefer to use daily velocity fields when hourly ones are available? I think there should be some justification for this, and it should also be mentioned in the Discussion as a possible further limitation to capturing coastal dynamics relevant to beaching.

The sentence specifying that daily-mean ocean, Stokes drift, and wind velocities forcing was used for the simulations has been moved to the second paragraph as you suggested. The drifters that were used in our study don't match the time period used for this sensitivity analysis, therefore, to include a new comparison would be a significant amount of extra effort that we are unable to do at this time. Additionally, the drifter comparison with particle movements that is detailed in the main text shows there is a considerable spread of particles released at the drifter locations due to diffusion. Given that the focus of the analysis for the manuscript was on country-to-country connectivity and beaching, as quantified by connectivity matrices, this sensitivity analysis was undertaken to ensure that daily forcing would give results consistent with hourly forcing. That is why the sensitivity study focussed on whether there was a large difference between connectivity when using daily versus hourly forcing. We appreciate that reviewer does not think this analysis is critical to results but we want to leave it in because the daily versus hourly approach has been raised in other studies. We have chosen to put this analysis in the Appendix for the reason that it is not crucial to the results we present in the main paper.

Please also mention the horizontal and temporal resolutions of the Stokes drift and wind fields in the 2nd paragraph in the Methods.

Thank you for pointing out this omission. The details have been added to the end of this paragraph (lines 128-132):

"Stokes drift velocities and wind fields at a height of 10 m above land, respectively. Stokes drift velocities were available in 3-hourly timesteps at a resolution of 1/5°, which is roughly 21 km at the latitudes of the Bay of Bengal; wind velocities were hourly and with a resolution of 1/4°, which is approximately 26 km at these latitudes. Both datasets were interpolated onto the relevant grid for each of the CMEMS and ROMS runs using cubic interpolation and then averaged to daily timesteps."

Please note that, in addition to Chassignet et al. (2021), van der Mheen et al. (2020b) also identified country-to-country connectivity. It may be worth adding a comparison to those results as well (currently only a comparison with particles exiting the region is done, not country connectivity). Alternatively, I would briefly mention that van der Mheen et al. (2020b) also includes this country-to-country connectivity and explain why no comparison with these results is done.

van der Mheen et al. (2020a) did publish connectivity matrices detailing country-country connections but they populated their matrices as a percentages of the total sink particles on a given country, whereas our analysis used percentages relating to the total released from a given source country, so the results cannot be compared. Given our chosen method of sourcing particles uniformly, the total number of particles released from a country is relative to the length of its coastline rather than based on any estimates of pollution. Therefore, calculating our connectivity matrices from the point of view of total sinked particles per country would not be informative.

We have added this detail to the manuscript and clarified why we cannot compare our results with theirs (lines 317-319):

"van der Mheen et al., (2020a) calculated connections between countries but chose to publish their results as the percentage of total particles that beached on a given country rather than total particles released from a given country. Therefore, our results are not comparable with their findings."

In the Discussion (L380-383) and Conclusion (L402-405) the possibility of weighting particle releases based on future improved source estimates is suggested. I personally think it unlikely that this would be done, it seems more likely that simulations would be rerun with more particles released from relevant sources, especially since running particle tracking simulations is relatively accessible and not very computationally costly. I suspect the authors mention this possibility to justify the uniform release (there is quite a lot of emphasis on justifying this throughout the manuscript), but I think this release is already well-justified because it allows a focus on the dynamics of floating plastic waste in the Bay of Bengal without having to deal with uncertainty surrounding source-estimates.

We are glad you see the benefits of the study as is, even without the need to further apply it to future research questions. Another reviewer had similar reservations about using our results in future when combined with updated estimates of plastics making their way into the ocean. Therefore, we removed the text you referred to and the Discussion and Conclusions no longer mention using the model as a tool that could be reused with newer, more accurate weightings applied to our results.

In the Abstract (L23-26) and Conclusion (L404-406), I personally think that the results of this study are generalised a bit too strongly. I would say that the main results of this study are that most simulated floating plastic beaches in its country of origin and that the EICC seems the main driver of plastic transport in the Bay of Bengal. Perhaps some more general conclusions can be drawn from this, for example that countries preventing plastic waste entering the ocean will benefit from reduced plastic waste washing up on their own shores, but I would not say that this study can be used directly to target beach clean-ups and aid policy decisions. I would suggest a bit more nuance in both the Conclusion and the final sentences of the Abstract.

Following this and comments from another reviewer, this paragraph has been removed from the conclusions section. The statement in the abstract about aiding policy decisions has also been altered to place less emphasis on how the results might be helpful to policymakers (lines 24-25):

“This study will therefore crucially inform future research and policy in this region, providing advice on the accuracy of different modelling approaches independent of assumptions of the source locations or volumes.”

Figure 1: Can you add the months to the “Pre-monsoon & monsoon” and “Post-monsoon” labels in the figure? Is it possible to also add a general direction for the wind/Stokes drift to the schematics here?

The months for each season have been included in the season labels and arrows indicating the average direction of the wind and Stokes velocities for the periods depicted have been added to each sub figure.

Figure 2: Panel a shows “a snapshot of ocean speeds from the ROMS dataset”. Please add the date used for the snapshot.

This information has been added to the figure caption.

Can you make the coloured dots in the legend of panel a larger? Please also check if the colours chosen are suitable for different colour vision deficiencies.

The colours of all figures have been altered to be more colour vision deficient-friendly. Thank you for the reminder! We have added extra dots to the legend so they are still the same size as those in the drifter tracks but are easier to identify.

Perhaps consider adding a “start” and “end” marker to each of the drifter trajectories (of the overall trajectories only, not the weekly portions for validation).

A large star marker has been added to indicate the starting position for each of the drifters. We feel this is sufficient to indicate the direction of the trajectories.

I appreciate the drifter trajectory with the particles in panel b. Are the particles for the CMEMS simulation? Would it be possible to show the same for the other drifters as well (perhaps in an Appendix/as supplemental figure)? D4 may be very chaotic to show, but I would be interested to at least see D1 (lowest MCSD for the ROMS run) as well as D5 (lowest MCSD for the CMEMS run).

The particles shown in Figure 2b are from the ROMS simulation, The caption has been updated to specify this. We have added a figure to the appendix which shows the validation particles for all drifters in both runs and added a note into the caption for Figure 2 to direct readers to this new figure.

Table 2: Please add the months to the monsoon seasons as well. Also, perhaps consider “Full simulation” instead of “Year”?

We have taken your suggestions and altered the figure as such.

Should this be Table 1?

Yes, thank you for pointing this error out. The caption has been altered.

Figure 3: Please also add the months to the monsoon seasons above the connectivity matrices here as well. Consider “Full simulation” instead of “Year” for the top panels.

These changes have been made.

References

Borrelle, S. B., Ringma, J., Law, K. L., Monnahan, C. C., Lebreton, L., McGivern, A., Murphy, E., Jambeck, J., Leonard, G. H., Hilleary, M. A., Eriksen, M., Possingham, H. P., De Frond, H., Gerber, L. R., Polidoro, B., Tahir, A., Bernard, M., Mallos, N., Barnes, M., and Rochman, C. M.: Predicted growth in plastic waste exceeds efforts to mitigate plastic pollution, Science, 369, 1515–1518, https://doi.org/10.1126/science.aba3656, 2020.

Castro-Rosero, L. M., Hernandez, I., Alsina, J. M., and Espino, M.: Transport and accumulation of floating marine litter in the Black Sea: insights from numerical modeling, Frontiers in Marine Science, 10, 1213333, https://doi.org/10.3389/fmars.2023.1213333, 2023.

Chassignet, E. P., Xu, X., and Zavala-Romero, O.: Tracking Marine Litter With a Global Ocean Model: Where Does It Go? Where Does It Come From?, Frontiers in Marine Science, 8, 667591, https://doi.org/10.3389/fmars.2021.667591, 2021.

Chenillat, F., Huck, T., Maes, C., Grima, N., and Blanke, B.: Fate of floating plastic debris released along the coasts in a global ocean model, Marine Pollution Bulletin, 165, 112116, https://doi.org/10.1016/j.marpolbul.2021.112116, 2021.

Cózar, A., Echevarría, F., González-Gordillo, J. I., Irigoien, X., Úbeda, B., Hernández-León, S., Palma, Á. T., Navarro, S., García-de-Lomas, J., Ruiz, A., Fernández-de-Puelles, M. L., and Duarte, C. M.: Plastic debris in the open ocean, Proceedings of the National Academy of Sciences, 111, 10239–10244, https://doi.org/10.1073/pnas.1314705111, 2014.

Delandmeter, P. and van Sebille, E.: The Parcels v2.0 Lagrangian framework: new field interpolation schemes, Geoscientific Model Development, 12, 3571–3584, https://doi.org/10.5194/gmd-12-3571-2019, 2019.

Duncan, E. M., Davies, A., Brooks, A., Chowdhury, G. W., Godley, B. J., Jambeck, J., Maddalene, T., Napper, I., Nelms, S. E., Rackstraw, C., and Koldewey, H.: Message in a bottle: Open source technology to track the movement of plastic pollution, PLOS ONE, 15, e0242459, https://doi.org/10.1371/journal.pone.0242459, 2020.

Eriksen, M., Lebreton, L. C. M., Carson, H. S., Thiel, M., Moore, C. J., Borerro, J. C., Galgani, F., Ryan, P. G., and Reisser, J.: Plastic Pollution in the World's Oceans: More than 5 Trillion Plastic Pieces Weighing over 250,000 Tons Afloat at Sea, PLoS ONE, 9, e111913, https://doi.org/10.1371/journal.pone.0111913, 2014.

Haza, A. C., Paldor, N., Özgökmen, T. M., Curcic, M., Chen, S. S., and Jacobs, G.: Wind-Based Estimations of Ocean Surface Currents from Massive Clusters of Drifters in the Gulf of Mexico, Journal of Geophysical Research: Oceans, 124, 5844–5869, https://doi.org/10.1029/2018JC014813, 2019.

Hinata, H., Ohno, K., Sagawa, N., Kataoka, T., and Takeoka, H.: Numerical modeling of the beach process of marine plastics: 2. A diagnostic approach with onshore-offshore advection-diffusion equations for buoyant plastics, Marine Pollution Bulletin, 160, 111548, https://doi.org/10.1016/j.marpolbul.2020.111548, 2020a.

Irfan, T., Isobe, A., and Matsuura, H.: A particle tracking model approach to determine the dispersal of riverine plastic debris released into the Indian Ocean, Marine Pollution Bulletin, 199, 115985, https://doi.org/10.1016/j.marpolbul.2023.115985, 2024.

Isobe, A. and Iwasaki, S.: The fate of missing ocean plastics: Are they just a marine environmental problem?, Science of The Total Environment, 825, 153935, https://doi.org/10.1016/j.scitotenv.2022.153935, 2022.

Jambeck, J. R., Geyer, R., Wilcox, C., Siegler, T. R., Perryman, M., Andrady, A., Narayan, R., and Law, K. L.: Plastic waste inputs from land into the ocean, Science, 347, 768–771, https://doi.org/10.1126/science.1260352, 2015.

Lange, M. and van Sebille, E.: Parcels v0.9: prototyping a Lagrangian ocean analysis framework for the petascale age, Geoscientific Model Development, 10, 4175–4186, https://doi.org/10.5194/gmd-10-4175-2017, 2017.

Lebreton, L. and Andrady, A.: Future scenarios of global plastic waste generation and disposal, Palgrave Commun, 5, 1–11, https://doi.org/10.1057/s41599-018-0212-7, 2019.

Lebreton, L., Egger, M., and Slat, B.: A global mass budget for positively buoyant macroplastic debris in the ocean, Sci Rep, 9, 12922, https://doi.org/10.1038/s41598-019-49413-5, 2019.

Lebreton, L. C. M., van der Zwet, J., Damsteeg, J.-W., Slat, B., Andrady, A., and Reisser, J.: River plastic emissions to the world's oceans, Nat Commun, 8, 15611, https://doi.org/10.1038/ncomms15611, 2017.

Lebreton, L. C.-M., Greer, S. D., and Borrero, J. C.: Numerical modelling of floating debris in the world's oceans, Marine Pollution Bulletin, 64, 653–661, https://doi.org/10.1016/j.marpolbul.2011.10.027, 2012.

Meijer, L. J. J., van Emmerik, T., van der Ent, R., Schmidt, C., and Lebreton, L.: More than 1000 rivers account for 80% of global riverine plastic emissions into the ocean, Science Advances, 7, eaaz5803, https://doi.org/10.1126/sciadv.aaz5803, 2021.

van der Mheen, M., van Sebille, E., and Pattiaratchi, C.: Beaching patterns of plastic debris along the Indian Ocean rim, Ocean Science, 16, 1317–1336, https://doi.org/10.5194/os-16-1317-2020, 2020.

Onink, V., Jongedijk, C. E., Hoffman, M. J., Van Sebille, E., and Laufkötter, C.: Global simulations of marine plastic transport show plastic trapping in coastal zones, Environ. Res. Lett., 16, 064053, https://doi.org/10.1088/1748-9326/abecbd, 2021.

Pawlowicz, R., Hannah, C., and Rosenberger, A.: Lagrangian observations of estuarine residence times, dispersion, and trapping in the Salish Sea, Estuarine, Coastal and Shelf Science, 225, 106246, https://doi.org/10.1016/j.ecss.2019.106246, 2019.

Peliz, A., Marchesiello, P., Dubert, J., Marta-Almeida, M., Roy, C., and Queiroga, H.: A study of crab larvae dispersal on the Western Iberian Shelf: Physical processes, Journal of Marine Systems, 68, 215–236, https://doi.org/10.1016/j.jmarsys.2006.11.007, 2007.

Pereiro, D., Souto, C., and Gago, J.: Calibration of a marine floating litter transport model, Journal of Operational Oceanography, 11, 125–133, https://doi.org/10.1080/1755876X.2018.1470892, 2018.

Schmidt, C., Krauth, T., and Wagner, S.: Correction to Export of Plastic Debris by Rivers into the Sea, Environ. Sci. Technol., 52, 927–927, https://doi.org/10.1021/acs.est.7b06377, 2018.

Shankar, V. S., Purti, N., Ramakrishnan, S., Kaviarasan, T., Satyakeerthy, T. R., and Jacob, S.: A new hotspot of macro-litter in the Rutland Island, South Andaman, India: menace from IORC, Environ Sci Pollut Res, 30, 82107–82123, https://doi.org/10.1007/s11356-023-28024-8, 2023.

Zhang, Z., Wu, H., Peng, G., Xu, P., and Li, D.: Coastal ocean dynamics reduce the export of microplastics to the open ocean, Science of The Total Environment, 713, 136634, https://doi.org/10.1016/j.scitotenv.2020.136634, 2020.