General comments

The manuscript needs comprehensive language editing. There are a lot of spelling mistakes, and many sentences are unclear to me. A thorough language editing for the manuscript is necessary to publish this study in Ocean Science.

Authors' response: Thank you for pointing this out. We agree with the reviewer's assessment. Accordingly, we have revised the spelling and grammatical errors pointed out by Reviewer#2 but also by the Reviewer#1 throughout the manuscript. Besides, we have rephrased and splitted many sentences to make then shortener and consequently provide more understandable information to the reader.

1.Introduction

The Introduction should be shortened. There are reiterative sentences and sections which are disconnected. Furthermore, technical details of the radar data should be moved to the methods section. References to webpages should be deleted as they just load the text.

Authors' response: Agree. We have dealt with your suggestions by reducing this section in order to present an introduction as clear as possible. We have deleted when not necessary and rephrasing many sentences, including the references to webpages. We have also moved the description on the technical aspects of the HF radar to the section 3 Methods and data.

2.Windage

The method used to calculate the wind slip of the particles is questionable. The referenced numerical studies do not simply add different windage values and estimate the distance of the trajectories. Please go more in-depth here and use an appropriate method to compare your numerical trajectories with those of the drifters.

Authors' response: Thank you for the suggestion. We agree with the reviewer that the approximation to the particle's behavior due to windage can be very complex since it depends on different parameters, from the shape and buoyancy of the objects to small scale processes in the air-sea interface. The fine tune of the Lagrangian model for the wind slip is out of the scope of this paper, where we focus on the submesoscale to mesoscale transport of particles in the coastal areas and how considering a simple windage approximation can be key for more accurate simulations.

Furthermore, as I understand it correctly, the particles were re-initialized every 4 hours on the drifter trajectories. This may neglect submesoscale processes that significantly affect the dispersion and distribution of floating objects in the ocean. The effects of tides may be underestimated, which of course, also play an essential role in the propagation and dispersion of particles in the Bay of Biscay. Please strengthen the study in this regard.

Authors' response: Thank you for your comment. New particles are released along the observed trajectories every 4-h but run during 24 hours in the simulations for wind drag estimation. However, for the study of seasonal scenarios of transport the particles are advected for 1 week, which provides integration of submesoscale and high frequency processes observed by the HF radar (mainly tides and eventually inertial oscillations)

3. HF radar current observations and wind data

The methodology of how the HF data is extracted and assimilated with the wind observations is, in my view, unclearly described. How are these data products incorporated on a uniform grid for further analysis? In addition, lines 178-180 indicate that the data extraction is questionable. Please clarify precisely how you extracted the data and what criteria were used for the quality check.

Authors' response: Thank you for your comment and for arising this question. The methodology for the processing and ingestion of the HF radar data is now improved. The new paragraph reads as follows:

“Surface velocity current fields were obtained from the EuskOOS HF radar station composed by two antennas located at Matxitxako and Higer Capes and covering the SE Bay of Biscay covering since 2009 a range up to 150 km from the coast. The EuskOOS HF radar is part of JERICO-RI (https://www.jerico-ri.eu/) and it is operated following JERICO-S3 project best practices, standards, and recommendations (see (Solabarrieta et al., 2016; Rubio et al., 2018) for details). Data consist of hourly current fields with a 5 km spatial resolution obtained from using the gap-filling OMA methodology (Kaplan and Lekien, 2007; Solabarrieta et al., 2021). “85 OMA modes, built setting a minimum spatial scale of 20 km and applied to periods with data from the two antennas, were used to provide the maximum spatiotemporal continuity in the HFR current fields, which is a prerequisite to performing accurate Lagrangian simulations. The application of OMA methodology has been validated for the Lagrangian assessment of coastal ocean dynamics in the study area by Hernandez-Carrasco et al. (2018). HF radar velocities were quality controlled using procedures based on velocity and variance thresholds, signal-to-noise ratios, and radial and total coverage, following standard recommendations (Mantovani et al., 2020). Data subsets were built for the Lagrangian simulations avoiding periods with temporal gaps (still present in case of failure of one or the two antennas) of more than a few hours.”

We have also included a more detailed explanation on the interpolation of the HF radar and wind data in the model:

“Simulations were forced by HF radar surface current velocity and wind data and interpolated at the particle position for integrating the trajectories. Beaching along the coast was implemented by a simple approach: if the particle reaches the shoreline it is identified as beached and it is removed from the computational process.”

4. Particle transport model

This paragraph does not describe the particle tracking module. The information given here is repetitive and only explains what the intent is for the particle simulations. Please describe exactly which way particle tracking was used. Are concepts for horizontal diffusion included and what scheme is used to move the particles forward in the module? It is not sufficient to cite studies that have used the same particle tracking module.

Authors’ response: Agree. Reviewer#1 has also recommended further improvements to the manuscript in order to provide a more extended and detailed description of the particle transport model. Accordingly, we have rewritten the sub section 3.5 Particle transport model and now reads as follows:

“The application of the transport module of the TESEO particle-tracking model (Abascal et al., 2007, 2017a, b; Chiri et al., 2020) was twofold: (1) simulate the transport and fate of floating litter items once they arrived to the open waters of the SE Bay of Biscay and (2) estimate a windage coefficient by calibrating the model according to the ‘low-cost buoys’ trajectories. This module allows for simulating passive particles driven by surface currents, wind and turbulent diffusion. Particle trajectories were calculated using the following equation:

$$\frac{d\vec{xi}}{dt} = \vec{u_a}(\vec{x_i},t) + \vec{u_d}(\vec{x_i},t) \qquad (1)$$

where $\vec{u_a}$ and $\vec{u_d}$ are the advective velocity and diffusive velocity, respectively, for the $\vec{x_i}$ point and t time. The advective velocity is calculated as the lineal combination of the wind and currents according to:

$$\vec{u_a} = \vec{u_c} + C_d\vec{u_w} \qquad (2)$$

where $\vec{u_c}$ is the surface current velocity, $\vec{u_w}$ is the wind velocity at 10m over the sea surface and Cd is the wind drag coefficient. The turbulent diffusive velocity is obtained using Monte Carlo sampling in the range of velocities $[-\vec{u_{d,}}\ \vec{u_d}\ ]$ which are assumed to be proportional to the diffusion coefficients (Hunter et al., 1993; Maier-Reimer and Sündermann, 1982). For each timestep Δt, the velocity fluctuation is defined as:

$$|\vec{u_d}| = \sqrt{\frac{6D}{\Delta t}} \qquad (3)$$

where D is the diffusion coefficient, whose value is 1 m2/s in accordance to previously modelling work for floating marine litter (Pereiro et al., 2019; Ruiz et al., 2022). Simulations were forced by HF radar surface current velocity and wind data and interpolated at the particle position for integrating the trajectories. Beaching along the coast was implemented by a simple approach: if the particle reaches the shoreline, it is identified as beached and it is removed from the computational process. TESEO has been calibrated and validated by comparing virtual particle trajectories to observed surface drifter trajectories at regional and local scale (Abascal et al., 2009, 2017a, b; Chiri et al., 2019). Although the TESEO is a 3D numerical model conceived to simulate the transport and degradation of hydrocarbons, it has also been successfully applied to other applications such as the study of transport and accumulation of marine litter in estuaries (Mazarrasa et al., 2019; Núñez et al., 2019) and in open waters (Ruiz et al., 2022)."

5. Discussion

The various sections of the discussion seem very disconnected to me. I encourage the authors to streamline the discussion and bring together the multiple aspects of the study. Please try to connect the different aspects of the study (litter distribution, particle tracking and windage) in a better way in the discussion. Regarding the limitations of the model, there are some other problems besides the points raised by the authors. For me, some points remain very unclear. How are the data sets for currents and wind assimilated? What effect does diffusivity have on the pathways of particles in the model or on litter or drifters in the ocean? Does a 4-hour reinitialization of particles suppress tidal effects? All of these questions should be carefully discussed and considered. This is especially important for coastal areas where complex submesoscale processes, fronts, and strong tidal currents become important for particle transport. In addition, Stokes drift is significant for transporting floating objects in the ocean. This should also be discussed in this section.

Authors' response: Thank you for your comments. As the reviewer suggested, we have restructured the manuscript by deleting the sub section 5.6 to achieve a more straightforward and connected discussion. However, we believe that keeping separate sub sections would be more appropriate than bringing all together in order to gain a better understanding of the key aspects of the study. Reviewer#1 has also stated that a more detailed discussion on the limitation of the model would improve the manuscript. Accordingly, we have rewritten this sub section and now reads as follows:

"The interaction between floating litter and the shoreline is highly complex and relies in many processes including waves and tides. Indeed, waves and tides can constrain coastal accumulation since they can resuspend and transport litter back into the ocean (Brennan et al., 2018; Compa et al., 2022). The geomorphology can also affect the retention of litter washing ashore. Sandy beaches tend to be more efficient at trapping and accumulating litter than rocky areas, which favor litter fragmentation (Robbe et al., 2021; Weideman et al., 2020). How these processes contribute to the actual beaching is unknown and they cannot be resolved yet at a suitable resolution (Melvin et al., 2021). In this study, particles were released in open waters and once they reached the shoreline, they were classified as beached. The tidal effect and the wave-induced Stokes drift were not accounted for to avoid introducing more uncertainties. However, further field and laboratory experiments to better understand on how these processes influence floating litter behaviour in the coastline is recommend. It is also important to consider for future research exploring the effect of the type of shoreline on coastal accumulation. In this study, a constant diffusion coefficient of 1 m^2/s was considered as a pragmatic choice based on previously modelling work for floating marine litter. However, more field measurements are necessary to accurately assess the influence of the diffusion process on the transport of floating marine litter."

Specific comments

I do not want to make remarks about linguistic and spelling mistakes. There are some significant spelling errors such as "week" instead of "weak" or "self-currents," which probably means "shelf-currents". I encourage the authors to carefully revise the manuscript for language and spelling if they decide to resubmit it.

Authors' response: Thank you for your comment. As previously mentioned, we have conducted a deep revision to rephrased long sentences difficult to understand and to amend spelling and grammatical errors pointed out by the reviewers. We hope that this new version is now more suitable for publication.

Figure 5: The authors mention in the caption "trapezoidal integration" I can't find this in the methods chapter. Please explain this in-depth in the methods section as well.

Authors' response: We agree with this comment. We have revised the text consequently to include a short detailed description of the method. The new sentence now reads as follows:

"The area $\widetilde{D}$ was calculated as a numerical integration over the forecast period via the trapezoidal method following Eq. (5). This method approximates the integration over an interval by breaking the area down into trapezoids with more easily computable areas."

Please use consistent upper- and lower case in subsection headings.

Authors' response: Agree. Thank you for your suggestion. We have therefore amended the headings to be consistent with the style and format of the manuscript.

In line 127, a figure from another publication is cited. This should be avoided.

Authors' response: Thank you for the suggestion. Reviewer#1 has also stated that citing a figure form other paper is very strange, so we have deleted to provide more understandable information to the reader.

Lines 115 and 311 are contradictory.

Authors' response: Thank you for your comment. We have revised both lines and we did not find contradictions between the description of the tidal currents in the study area and the hydrological (geo)morphological characteristics of the rivers. We would appreciate a more detailed explanation from the reviewer to accurately address the comment.

Section 5.6 contains a lot of information about visual observations of litter with camera systems. For me, this is not related to the results of this study. If I understand it correctly, the study was conducted as part of the LIFE-LEMA project. This is also mentioned for the first time in this section and it is confusing to mention it here. Why is the camera system data not included in this study if the project also collected this data? I would recommend including the data or not mentioning it in this section.

Authors' response: Thank you for the suggestion. This specific point was also raised by Reviewer#1. Since this sub section may seem disconnected, we have accordingly removed it to make a clearer and much straightforward discussion.