



EGUsphere, referee comment RC1
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Comment on egusphere-2022-316

Anonymous Referee #1

Referee comment on "Modelling floating riverine litter in the south-eastern Bay of Biscay: a regional distribution from a seasonal perspective" by Irene Ruiz et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-316-RC1>, 2022

Review of the manuscript "Modelling floating riverine litter in the south-eastern Bay of Biscay: a regional distribution from a seasonal perspective" by Irene Ruiz et al.

In this work the authors analyze the results of three different experiments aiming at characterizing the riverine input of marine litter (ML) in the SE Bay of Biscay. The first experiment is the characterization of the ML in the Deba river by using a barrier (net) that collected ML dragged by the river flow. Weekly samples were collected along a three-month period and the ML was classified and described. The second experiment is the deployment of four low cost lagrangian buoys in the mouth of four different rivers in order to analyze their trajectories using the TESEO lagrangian model. The model uses HF radar currents and reanalysis (ERA5) wind data to compute lagrangian trajectories. This analysis is used to calibrate the wind drag coefficient (C_d) in the model. Finally, the third is a numerical experiment aiming at simulate the fate of neutral lagrangian particles deployed in the mouth of eight different rivers along the Basque and French coasts. Weekly simulations (40, 10 per season) were carried out, starting with similar initial concentration of ML particles in the mouth of the rivers, for two different types of particles: highly buoyant, with a $C_d = 4\%$ hence affected by the wind drag, and low buoyant, with $C_d=0\%$ and not affected by wind. The results of the simulations are used to describe the distribution of the ML particles after a week, their beaching locations and time, the sub-regions that import/export ML and the seasonality of these distributions.

In my opinion the work carried out by the authors is remarkable. They combine observational and numerical experiments, trying to provide an analysis as wide as possible of the characteristics and fate of the riverine ML in the studied region. Their results are relevant, and a step forward in the complex task of the numerical analysis of ML dispersion. However, there are many aspects of the manuscript, both formal and regarding the methodology and analysis, that need to be improved. For this reason, I recommend a major revision before its publication in Ocean Science.

General comments:

Regarding the manuscript writing, a deep revision is needed. There are many sentences that are incorrect or difficult to understand. Please try to avoid very long sentences containing a lot of information. It is sometimes very complicated to understand what you are trying to point out. The vocabulary and punctuation should also be revised.

About the methodology, the description of the lagrangian model should be extended, particularly describing how does the model simulate the particles beaching. This is crucial to make a proper interpretation of the results. Also, the limitations of the model and the simulations set-up should be included in this section (instead of only the discussion). This way the reader can make a better interpretation of the results.

In the results/discussion sections, you find very interesting results but a deeper analysis of some of these results and more contextualization is missing.

Specific comments:

1. INTRODUCTION

Lines 42-43: what do you mean with "less than a tenth"? With respect to the values given by MPW models? It is unclear what you mean here. Please rephrase.

Lines 45-48: This sentence is too long and difficult to understand.

Line 50: what do you mean by "river waters"? If you mean that the ML remains close to the river mouth you should use this term ("river mouth").

Line 59: What do you mean by "mature"?

Line 79: delete extra "the"

Line 90: I don't fully understand why you differentiate throughout the text riverine and floating litter. Once the litter is in the sea is all marine litter. Moreover, you only simulate the ML at sea, so for your simulation experiment everything is marine litter. This distinction along the text is often confusing.

Lines 94-95: "...parameterized to represent riverine litter trajectories according to their observed buoyancy." → This is not completely true. In your numerical experiment you don't use the observations to characterize the ML particles simulated. You only made a distinction between high and low buoyant particles. But the number of particles released on each simulation is always the same and with similar characteristics.

Line 100: You could include here a short description of the paper sections.

2. STUDY AREA

Line 113: what do you mean by "self-water masses"?

Lines 115-116: "...Tidal currents in the area are quite weak constrained by topography and width on the continental shelf..." → "...Tidal currents in the area are quite weak, constrained by topography to the continental shelf...".

Lines 118-120: This sentence is too long. Rephrase please.

Line 121: Achieving → reaching

Line 127: Very strange to cite a figure from other paper. Include the figure number please.

Lines 129-136: Here you have to be more specific in the description of the results you are citing. Most of these studies are based on Lagrangian simulation of ML particles, many of them using numerical models for the current fields, others using HF radar data. Some of them include windage, with different parameterizations, others don't, etc... You should specify the details of the estimations you are citing and also try to avoid the word "observed", since these results are mostly based on simulations. I would also include here a short summary of the most important sources of uncertainty found by the authors in their different approaches. I think it is important to contextualize the results of the study and the limitations of the state-of-the-art ML modelling.

3. METHODS AND DATA

3.2 DRIFTER OBSERVATIONS

Lines 163-164: This is very interesting. Could you provide a little more information about the batteries and its duration? According to the table some of the buoys worked only for a few days and others for more than 2 weeks. Why this difference? Did you recovered the buoys or were lost?

3.3 HF RADAR CURRENT OBSERVATIONS AND WIND DATA

Lines 179-180: This sentence is too long. Rephrase please.

Line 184: The resolution is 30 km or $0.3^{\circ} \times 0.3^{\circ}$. Both are similar but not exactly the same. Giving two different values is confusing.

Lines 185-186: the weekly periods are first mentioned here. Either described them or indicate the section/table where you describe them below.

3.5 PARTICLE TRANSPORT MODEL

Here a much more detailed description of the lagrangian model is missing. This is crucial to understand the accuracy of the results. Some of the missing information is:

How does the model solve the movement of the water parcels?

Does it include horizontal diffusion? If so, how it is implemented? Random walk? I understand from table 2 that turbulent diffusion is included, but not explained.

Since the resolution of the HF radar and the wind data are different, I understand that the wind data is interpolated to the HF radar grid, am I right?

How is the wind drag coefficient implemented in the movement equations?

How do you define when a particle is beached? Does the Lagrangian model includes a beaching algorithm? How does it work? This is particularly important since some of your more relevant results are related to the beaching process. A detailed description of how does the model considers a particle beached is crucial to understand your results.

3.5.2 LAGRANGIAN SEASONAL SIMULATION OF RIVERINE LITTER ITEMS

Line 224: Please indicate the total number of simulations (40, if I'm not wrong).

Lines 228-227: This is another key issue that you should underline and also take into account in the discussion section when comparing the results with previous works and observations. You are releasing the ML particles 2.5 miles from the coast. As the authors know for sure, there are numerous coastal processes that traps the ML in coastal areas, especially if the wind drag is taken into account. Therefore, your results are only valid for the fraction of ML coming from the rivers that leaves the coastal area and reaches open sea.

In addition, you are not making any difference between rivers or seasons. You are considering that all the rivers have the same ML input, and that this input is constant along the whole year. Therefore, the spatial distribution and seasonality that you obtain only depends on the river mouth position and the variability of the HF radar current field and the ERA5 wind field.

In summary, you are considering the 8 rivers as constant ML input sources on open sea. I think that it is important to state very clearly these assumptions, and the limitations that imply, in order to make a proper interpretation of your results.

In section 5.3 and 5.4 you address some of these issues, but in my opinion is important to clarify them here, before presenting the results. This way the reader is aware of the model/simulations limitation and can make a better interpretation.

4. RESULTS

4.1 RIVERINE LITTER CHARACTERIZATION

I think these results are very interesting. There is a lack of information on the ML sources in general, and on rivers in particular. This kind of experiments are very useful to start filling these knowledge gaps.

4.2 WIND DRAFT COEFFICIENT FOR DRIFTING BUOYS

Lines 259-260: I don't understand what you mean by "spread out over the rivers inside the HF radar coverage area". Please clarify this sentence.

4.3 SEASONAL TRENDS ON FLOATING RIVERINE LITTER TRANSPORT AND FATE

Lines 277-278: Very interesting result, but depends on the beaching parameterization.

Line 281: Which specific characteristics of the forcing are you considering in this assumption? In my opinion, there are quite different behaviors of the particles depending on the location of the river mouths. For instance, in the Urumea river almost all particles with $C_d = 0\%$ remain in the water after the week period, while for the Deba river the reduction of particles is much higher (~ 200 less on water). Also, there is a clear seasonality, rivers that "lost" more particles in summer and winter are different. A deeper and clearer analysis of this results is missing.

Line 281-283: "When... .. simulations". This sentence is too long and a bit confusing. Please rephrase to be more clear.

Lines 283-286: There is a clear seasonal variability in the beaching regions, particularly for particles not affected by winds. This variability can be only linked to the current field variability. You could mention this here or in the discussion section.

In general, a little more detail in the analysis of figures 9 and 10 is missing. I think they represent very relevant results of the study and a more thorough description would be adequate.

Figure 9: it is very difficult to distinguish the lines corresponding to each river. Please choose clearly different colors for each one.

Figure 10: This figure is very interesting and informative. Please indicate in the figure the region to which each river belongs. Also, I think that if you put Bizkaia above Gipuzkoa, so the regions are ordered counter-clockwise (from W to NE), it would be easier to understand the particles transfers from one region to another.

5. DISCUSSION

5.1 RIVERINE LITTER COMPOSITION

Lines 309-310: I don't understand the meaning of this first sentence, please rephrase it.

Lines 323-324: If you find higher percentage of large pieces (2.5-50 cm) of polystyrene, doesn't it mean that the degradation is lower (not higher) than in the Black Sea or the Mediterranean?

The size of the sampling net grid is 6 cm. Meaning that items smaller than this size will pass through the net. That is probably why you find so few bottle caps or cigarette butts, which are very common. Do you have any estimation of the amount of items between 2.5 a 6 cm that you could have missed (maybe observations at sea near the Deba's mouth)?

5.2 WIND DRAG ESTIMATION

As a suggestion, many of the considerations about the suitability and accuracy of the low cost drifters would be more useful in the methodology section.

I miss a comparison with previous studies on wind drag coefficient for Marine Litter, rather than using results for oil spill or algae. For instance, the work of Pereiro et al (2018) and references therein (<https://doi.org/10.1080/1755876X.2018.1470892>). Critchell et al. (2015) (<https://doi.org/10.1016/j.ecss.2015.10.018>) or Critchell and Lambrechts (2016) (<https://doi.org/10.1016/j.ecss.2016.01.036>).

5.3 SEASONAL RIVERINE LITTER DISTRIBUTION BY REGION

Line 371: delete "but".

Lines 378-380: the last sentence is too long, please consider rephrasing.

5.4 RIVERS AS KEY VECTORS OF RIVERINE LITTER

Lines 384-385: Indeed, what you are showing in this study is the impact of the river mouths as a constant source of ML in the ocean. All the variability described depends only on the HF radar current field and the ERA5 wind field (for those particles affected by wind).

Lines 387-388: I think in your case the socio-economic factors are quite homogeneous in your area of study.

Line 394: What do you mean by "dominant number of rivers"

5.5 MODEL LIMITATIONS

This is key in the processes that you are describing along the whole paper. I would put this whole section in the introduction or the methodology section. Together with a detailed explanation on how your model simulates the beaching process.

In this section I would include an estimation (or at least a description) of the uncertainty specifically related with your model.

As I mentioned in my comments for section 3.5.2, the model and simulations set-up limitations that are previously known could be included in that section for clarity (also for the beaching algorithm). Here I would comment the impact of those limitations on the results.

5.6 RIVERINE LITTER COLLECTION AND MONITORING BY A FLOATING BARRIER

This is very interesting but I don't see how it is related with the results of your study. I suggest to summarize and include it in the introduction section or to clearly point out the relation with your results.

6. CONCLUSIONS

Lines 441-443: Actually, since you don't use real data of the amount of ML transported by the rivers, you are not analyzing the input of inland ML, you are estimating the fate of the ML once it reaches open sea.

Lines 448-449: This comparison should be made with other works estimating wind drag coefficient. In the literature this coefficient ranges between 2-1.5%, so in the range of your estimation. This should be further discussed.

Line 449-451: "The developed... ..Type of items" à This is a very interesting result. Congratulations.

Finally, I wonder why the authors didn't combine the information obtained in the sampling of the Deba river with the numerical results. You estimate that around 68% of the riverine litter collected were low buoyancy items, while the rest 32% were high buoyancy items. Even if you keep the same number of particles in your simulations for both type of items, you could give an estimation based on the observations of the fraction of each type expected to reach the coast. For instance, according to figure 8, in winter 95% of high buoyant particles reach the beach, while for the low buoyant only 25% are beached. This mean that if both type of particles are considered (keeping the fractions observed in the Deba), only 47% of the particles would reach the beaches, 30% would be high buoyant and 17% low buoyant.

Of course, the fractions of the different types of ML observed in the river and the open sea would surely be very different. But as a first approach I think it would be interesting to show these results. Also, I think it would be one of very few (if not the only one) paper combining riverine litter observations and modeling, even with all the limitations previously commented.