We sincerely thank the editor and both reviewers for their time and careful consideration of our manuscript. We have significantly improved the overall quality of the manuscript by responding to their helpful and constructive comments.

In the following, we address the comments raised and propose some modifications, including mainly :

- the description and implementation of wave effects,

- the model-observation comparisons,

- addressing land-sea transfers not supported by the Lagrangian analyses of the study.

Our response to each point made by the reviewer 2 is presented below in blue. Our corrections and additions to the manuscript text are underlined here in the responses.

Best regards,

Lisa Weiss and co-authors

REVIEWER 2

Comments on Weiss et al.: "Modeling Indian Ocean circulation to study marine debris dispersion: insights into high-resolution and Stokes drift effects with Symphonie 3.6.6"

To gain a better understanding of the pathways and accumulation of marine debris, significant developments have occurred over the past decade. The main three components which govern the quality of a dispersal model are the marine debris sources (initial conditions), the marine debris transport mechanisms (in other words the influence of the different forcing components namely circulation, wind and waves on the marine debris displacement) and the quality and reliability of the forcing models themselves. In that context, this paper aims to cover the latter two by studying the impact of wave-induced transport and higher resolution, which could presumably give a better description of the circulation, especially close to the coastline.

The paper is well structured and written with a good quality of English. The results are well presented with precise and good-quality figures. It, however, suffers from some unclarities in the modeling used to describe the wave-induced transport. As highlighted by the first reviewer, it is hard to understand whether we are looking at a one-way coupled circulation - wave model, or that the Stokes drift (which is a Lagrangian « thing ») is « simply » added to the circulation. These unclarities lead to troubling the appraisal of the interpretation made by the authors of such a phenomenon. Also, as stressed by the first reviewer, looking at the dispersal of marine debris only from a « synthetic » offshore release scenario feels a bit frustrating from a reviewer's standpoint where all the effort put into having a more precise representation of river discharges and using higher resolution close to the coastline becomes, in turn, irrelevant. By solving those two points (clarifying the narrative and explanations around wave-induced transport and managing expectations on the marine debris dispersal relevance in the objectives) this work should become suitable for publication.

> We sincerely thank the reviewer for the constructive feedback. We have addressed the points raised and the imprecision highlighted by both reviewers, and have made the suggested corrections to provide greater clarity on the method used and its implications for the results presented. In particular:

- A better description of the implementation of wave-driven currents.

– A better justification of the Lagrangian dispersion sensitivity tests, which are a prelude to more realistic dispersion simulations based on river source scenarios, to be published in a future article.

Some modifications made:

- L158: adding the description of equations (1) and (2) , demonstrating how the wave effect is included in the Eulerian current calculation.

- We have completed the following sentence (L150): "[...] this parameterization consists in an online one-way forcing utilizing the surface Stokes velocity (extrapolated below the surface using the peak period)".

- We clarified in section "2.2.2 Lagrangian simulations", L185: "In the case of IndOc.HR-Sto, Lagrangian particles are advected by the Eulerian current fields dynamically modified by wave forcing through the Stokes–Coriolis term, but without any explicit addition of Stokes drift in the Lagrangian advection scheme."

- To avoid misunderstanding about land-sea transfers, we modified the following sentence from the abstract (L4): "To support future studies on land-sea transfers and marine debris dispersion in this complex ocean, we developed a new circulation modeling configuration using the hydrodynamic model SYMPHONIE."

- We have added the following sentence in the introduction (L79): "While the present study does not explicitly address plastic land-sea transfers, it provides the hydrodynamic framework necessary for future analyses focusing on riverine inputs and coastal retention."

Comments (in addition to those given by Reviewer 1):

l.44: HYCOM has a meridional resolution of 0.08° and 0.04° at higher latitudes from the equator

> We have specified this in the corresponding sentence in the introduction (L46) such as: "the 1/12° HYCOM global product (0.08° zonal and 0.08° to 0.04° meridional resolution) (Cummings and Smedstad, 2013; Pottapinjara and Joseph, 2022, used by Chassignet et al. (2021) and van der Mheen et al. (2020) for Lagrangian plastic tracking)"

l. 46: maybe worth mentionning the existence of a global LLC4320 (see Forget et al. 2015 and Rocha et al. 2016 e.g.) at a much higher resolution (1/48°) for 2011

> Thank you for this information we were not aware of. We have added the following sentence (L51): "For shorter simulation periods, much higher resolution is available with the global LLC4320 configuration (1/48° ∼2 km) for 2011 (Forget et al., 2015; Rocha et al., 2016)."

l. 57-60: the explanation on the roles in the impact of wave-induced drift on Eulerian currents, wave-induced drift on Lagrangian particles, and possibly windage which could be added to the latter processes is pretty unclear. An interesting paper on the influence of the differents processes at a global scale (including a split between Ekman currents / geostrophy / tides etc…) is Onink et al. 2019.

> We have clarified these sentences as follow (L61): "Wind and wave processes can significantly influence the dispersion and accumulation of buoyant material such as marine debris (van Sebille et al., 2020). Their impact occurs through multiple mechanisms (Onink et al., 2019): (i) the modification

of Eulerian currents by waves, notably via the Stokes–Coriolis force, (ii) the direct advection of particles by Stokes drift when explicitly included in Lagrangian schemes, and (iii) the windage effect, i.e., the direct wind forcing on partially emerged objects. Different representations of wave effects have also been explored through one-way forcing (Röhrs et al., 2012 ; Cunningham et al., 2022) or two-way coupling frameworks (as shown by Rühs et al., 2025 ; Bajon et al., 2023 ; Couvelard et al., 2020)."

l. 88: from the context it is clear that « tracers » correspond to temperature and salinity but why not explicitly mention them, in a dispersal / Lagrangian paper this can be confusing.

> We have specified "temperature, salinity" instead of "tracers". (L98)

Figure 1: every 50 gridlines instead of meshes, maybe worth adding a red circle to materialize the release of Lagrangian particles

> We have modified the term "meshes" to "gridlines" in the caption of Fig. 1 and added pink circles (to stay consistent with the particle plumes illustrated Fig. 2) to materialize the Lagrangian initialization with the following sentence added to the caption "The three pink circles show the particle release locations for the Lagrangian sensitivity analysis.".

l. 161: rising velocity (singular) - because only one rising velocity is considered for all particles, the value of 1mm/s seems pretty low compared to experienced rising velocities for e.g. mesoscale plastics (see Lebreton et al. 2018, Supplementary Material)

> We have changed for singular "rising velocity". (L194)

We agree with the reviewer that the chosen rising velocity value of 1 mm/s is at the lower end of values typically reported in the literature (such as Lebreton et al., 2018). However it remains within observed ranges for specific plastic types, particularly light fibers or weathered and biofouled debris. Theoretical, laboratory and field studies have shown a wide variability of rising velocities, depending on particle size, shape, density, and biofouling state. Values ranging from as low as 0.01 mm s^{-1} for very light fibers to over 200 mm s^{-1} for highly buoyant foams are reported (Weiss et al., 2024, https://doi.org/10.1007/s11356-024-34635-6). Several studies report also values close to ours for specific particle types, including 0.1–6 mm s^{-1} (Kuizenga et al., 2022, https://doi.org/10.1021/acsestwater.1c00467), 1.6–35 mm s^{-1} (Waldschläger et al., 2020, https://doi.org/10.1016/j.envres.2020.110192) or 6.5 mm s^{-1} for packaging, bags, films to 10 mm s^{-1} for straws, food containers, bottlecaps (Daily et al., 2020). For microplastics, Koi et al., 2016 (doi: 10.1038/srep33882) used rising velocities between 9 and 19 mm/s and between 6 and 8 mm/s for fibers. Jalon-Rojas et al., 2025 (https://doi.org/10.5194/gmd-18-319-2025) used rising velocities between 4 an 78 mm/s. Slow velocities can also result from significant biofouling, which may reduce buoyancy by up to 48 % (Nunez et al., 2023, https://doi.org/10.1016/j.marpolbul.2023.115239). In situ observations have even found buoyant polymers at depth (Int-Veen et al., 2021, https://doi.org/10.1016/j.marpolbul.2021.112876), suggesting that laboratory-derived values do not fully capture real-world behavior.

Our choice was motivated by the objective of representing buoyant debris that can still be mixed within the surface layer, rather than remaining strictly at the surface. This intermediate value is consistent with the range of ocean vertical velocity as stated in the manuscript "intermediate order of magnitude of average vertical velocities of surface currents". It is also consistent with rising velocities between 0.01–100 mm s^{-1} in the upper ocean until 400 m depth simulated by Fisher et al. (2021)

(https://doi.org/10.5194/bg-2021-236), and with our previous sensitivity experiments in the Mediterranean Sea (Weiss et al., 2024), which showed that higher values tend to cause particles to remain at the surface, where additional processes (e.g., windage, Lagrangian Stokes drift), that are not included in our configuration, could become dominant. Using 1 mm s^{-1} therefore provides a balance between representing buoyant particles and allowing interaction with vertical mixing processes, in line with the Lagrangian sensitivity analysis of this study.

We have added clarifications to the following sentence (L193): "Only buoyant particles are considered here (illustrated section 5) with a rising velocity ws = 1 mm s^{-1}, oriented toward the sea surface. This value is within the lower range reported in the literature for specific plastic debris types such as light fibers, weathered or biofouled items (Kuizenga et al., 2022 ; Waldschläger et al., 2020 ; Fisher et al., 2021). It was chosen as an intermediate order of magnitude of average vertical velocities of surface currents, allowing such buoyant particles to be influences by vertical mixing processes in the surface layer rather than remaining strictly at the surface."

Therefore, our choice of rising velocity is consistent with the literature, even though it falls within the lower range of values reported. It is also consistent with the choice of forcing used in this paper.

l. 201: 47 rivers vs 46 rivers on line 193, am I missing something - see also Figure A4

> L228: "46" at this line is a mistake, we have changed for "47 individual rivers common to our domain" as in the caption of Fig. 4. However in Fig. A4, there is one river missing that is the Sittang river because only its annual mean discharge value was available in the D&T dataset (as display in Fig. 4), but no monthly mean discharge values were available to include it in Fig. A4, which explains the difference.

paragraph starting l. 215: the discussion there « contradicts » the objective of having a better representation of river discharges given than GLOFAS seems to systematically overestimate the measurements.

> The opportunity to use GloFAS is unique, since the dataset enables to simulate the daily river discharge of many more rivers than previous classical datasets. For example, thanks to GloFAS, we can consider the discharges of 336 rivers, while the D&T climatology only contains data for 47 rivers in this same domain. This argument could already be sufficient to support the idea of a better representation of river discharge, influencing coastal dynamics through freshwater plumes. Nevertheless, as GloFAS is a new operational product from the CMEMS platform, it still needs to be evaluated for different regions, as we have done in our study by comparing this new product with others.

This is also why we are publishing our work in two steps, this first study evaluating the simulations and performing sensitivity tests. Since GloFASv3.1 revealed an overestimation of river discharges, we used the latest version GloFASv4 (as illustrated in Fig. 4 and section 2.4), to perform our multi-annual simulation for realistic Lagrangian scenarios, that will be published in a future study in progress.

We have added the following sentence at the end of section 2.4 to express this idea (L259): "Despite these biases, using GloFAS is an improvement in terms of river discharge representation, as it allows many more rivers to be included at a higher temporal resolution than traditional climatology. However, due to the overestimation observed in v3.1, our subsequent work has adopted the more recent GloFASv4, which will be used for upcoming multi-annual simulations to support future realistic Lagrangian scenarios based on river sources."

Figure 7: the superimposition of daily tracer profiles is hard to read (dark blue and light blue)

> We totally agree with this comment, but we have not found a better way to show that the daily profiles of the two datasets appear to follow the same trend (in addition to the average profiles). While we appreciate the reviewer's advice regarding the display of our future figures, we have chosen to keep this figure as it is for this article.

Figure 8: it would be interesting to split between the different regions and not only focus on the whole domain - in relation with the analysis made after (figure 9) and before…

> This suggestion represents one interesting perspective. However, since it would not offer substantial new information beyond the whole domain or the regional energy analyses already presented in Fig. 9, 10, A6 or A7, we decided not to add further figures.

Figure 12: consider bigger fonts in the blue / red / yellow boxes

> The font size has been changed from 9 to 10.5 in the colored boxes.

l. 622: «its implications for marine debris dispersion in the region », in light of what was said before, marine debris dispersion cannot be viewed independently from release locations. Demonstrating the impact of an improved circulation and transport modeling based on a (very) hypothetical release scenario (which cannot be validated by design) weakens the demonstration. Especially given that so much effort has been put into the river discharges (which could, combined with other socio-economic information, provide useful proxies for marine debris release, see e.g. Meijer et al. 2022)

l. 667: it is now understandable why the coastal releases were not considered so far as it is meant for future studies, so why not state clearly that the implications of such improved modeling on the marine debris dispersal in the region will only be tangible once realistic debris sources will be considered.

> Following those two previous arguments, the reviewer is right to ask for extra clarifying statement to make it explicit that the present work conclusions for marine debris dispersion remain partly hypothetical until realistic source terms are used. While we acknowledge that the present release scenarios are idealized and do not explicitly represent coastal inputs, they remain relevant for studying the dispersion of debris already present in the open ocean. The randomly distributed particle releases over large circular areas for the whole year could reasonably represent floating debris that has been drifting offshore for some time after initial coastal release. This is consistent with observed debris concentrations sampled in these regions, suggesting that our tests, while not suited for detailed coastal-scale analyses, are appropriate for assessing transport and spreading patterns in the open ocean. We have revised our last paragraph such as (L719): " While the present release scenarios are idealized, the implications for realistic debris dispersion will only be fully assessed once coastal and riverine sources will be included."

Figure A4: back to 46 rivers?

> See comment above : In Fig. A4, there is one river missing that is the Sittang river because only its annual mean discharge value was available in the D&T dataset (as display in Fig. 4), but no monthly mean discharge values were available to include it in Fig. A4, which explains the difference.

Figure A9/A10/A11: a slight increase of the font size in the roses could be beneficial

> We recognize that the suggestion is justified, but we can not modify these figures. The multiple subplots leave insufficient space for increasing the font size without overlaying the plots. Since these figures are provided in the supplementary materials, they will be available online where readers can zoom if needed.

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