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 1 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

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REVIEWER 1

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"

The manuscript addresses an outstanding issue in the field of marine (pollution) transport modelling: providing coherent ocean velocity output from the coast to the open ocean that resolves the dominant transport processes from the submesoscale to the basin scale. It does so by introducing a new ocean model configuration for the Indian Ocean with grid refinement towards key coastal regions, inclusion of wave forcing, and a more realistic representation of river discharge. While the grid refinement and inclusion of wave forcing are not based on novel concepts, their combination in this context yields a potentially valuable new tool for marine pollution modelling that may support both sensitivity studies and improved estimates of pollution patterns compared to standard approaches.

The overall structure and presentation of the results is clear, including useful visualizations. However, the writing includes imprecise terminology, and several methodological concepts are insufficiently or inaccurately described. This limits a thorough assessment of the approach and specifically concerns the description and implementation of wave effects (see General Comment 1) and model-observation comparisons (General Comment 2). Additionally, the stated goal of addressing land-sea transfers is not clearly supported by the Lagrangian analyses presented (General Comment 3).

To conclude, while I see the potential of the manuscript to become a relevant addition to the field, I recommend major revisions to clarify key concepts, improve the terminology, and better align overall content and objectives.

We sincerely thank the reviewer for the appreciation of our work, the scope and objectives of which have been perfectly summarized here. We have taken note of the imprecise terminology and insufficient description of methodological concepts that need to be refine, following in priority the 3 general comments raised by the reviewer.

General Comments:

1. Representation of wave effects

The description of wave-related processes is vague, and terminology is inconsistently used. In theory, waves affect Lagrangian transport both (i) directly via Stokes drift and (ii) indirectly through wave-induced modifications of Eulerian currents, including (anti-)Stokes forces such as the Stokes–Coriolis force. After rereading the methods, I was left with the impression that:

- The only wave-related effect considered is the Stokes–Coriolis force, yet, this is also loosely referred to throughout as "Stokes drift forcing" or simply "Stokes drift".
- Lagrangian particles are advected by Eulerian currents only (eventually including the effect of the Stokes-Coriolis force), and not by a combination of Eulerian currents and Stokes drift.

> This first general comment is entirely relevant. The reference we made to studies using an approach similar to ours (L149) - "Following a similar approach to McWilliams and Restrepo (1999); Jordà et al. (2007); Gentil et al. (2022); Weiss et al. (2024b*)*" – did not fully clarify the implementation of wave effects in our simulation.

As the reviewer suggested, we only considered the wave effect on Lagrangian transport indirectly – i.e. option (ii), through wave-induced modification of Eulerian currents, including anti-Stokes forces such as the Stokes-Coriolis force. In our simulations, the Stokes drift is not explicitly added to the Lagrangian advection scheme. Particles are advected only by the Eulerian currents resulting from the hydrodynamic model, in which the effect of waves is dynamically integrated via the Stokes-Coriolis term in the momentum equations, which consistently induces a feedback from the waves on the currents. This online forcing is based on the waves variables provided by the CMEMS WAVERYS 0.2° product (https://doi.org/10.48670/MOI-00022).

If this is correct, the implementation is basic and not fully aligned with the current state of the art (e.g., Couvelard et al., 2020). It also contrasts with recent findings suggesting that both Stokes drift and wave-induced modifications of Eulerian currents are important for Lagrangian transport (e.g., Röhrs et al., 2022; Cunningham et al., 2022; Rühs et al., 2025).

> We agree with this, but consider that one-way forcing is a reasonable compromise for incorporating the effects of waves into physical simulations. Two-way coupling is much more computationally expensive and therefore not feasible for the high kilometer resolution of our grid over the entire Indian Ocean basin. In comparison, Couvelard et al., 2020 used two-way coupling globally at a much coarser resolution of 0.5°. Our approach differs from simpler methods (sometimes also used in the literature), where Stokes velocity field is simply added to Eulerian current field offline and apply to calculate particle trajectories, without dynamic feedback. Our online forcing allows us to generate a consistent response of the current field to variations in Stokes velocities, in particular via the Stokes-Coriolis force, which typically reduces the intensity of surface currents and affects the drift of surface particles, and in particular the large scale connectivity.

Moreover, the explicit addition of Stokes drift in the Lagrangian module could have (i) led to redundancy in the consideration of this effect and (ii) complicated the analysis by introducing a second source of divergence between our simulations that could have complicated our sensitivity tests. We therefore chose this approach because it allowed for a more direct comparison between the three configurations tested for Lagrangian applications in our study (IndOc.HR, IndOc.HR-Sto and IndOc.12).

We acknowledge the references suggested by the reviewer and have included them in the introduction such as (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)."

I recommend that the authors:

- Clearly distinguish between Stokes drift and Stokes–Coriolis forcing throughout the text.

> We have clarified these elements in the text and added equations in order to remove any ambiguity between the concepts of "Stokes drift" and "Stokes-Coriolis forcing", as follows:

- L147 "2.1.5 Stokes drift" changed to "2.1.5 Current-wave interaction"

- L150 adding "consists in an online one-way forcing utilizing the surface Stokes velocity"

Throughout the manuscript, we verified the relevance of the term "Stokes drift » wherever it appeared, replacing it with "wave forcing" or "Stokes-Coriolis force" or "Stokes velocity forcing". In particular, we modified:

- in the title "insights into high-resolution and Stokes drift effects », replaced by "insights into high-resolution and wave forcing effects"

- in the abstract "Stokes drift forcing" was replaced by "wave forcing" (L9), by "Stokes-Coriolis force" (L15) and by "Stokes velocity" (L17)

- in the introduction "Stokes drift forcing » was replaced by "wave forcing" (L82, 86, 90) or "Stokes-Coriolis force" (L83)

- in section 3 (L267) "the wave-induced forcing through Stokes-Coriolis term with IndOc.HR-Sto in subsection 3.2."

- Clarify whether and how Stokes drift is included in the Lagrangian advection scheme.

> We also clarified in section "2.2.2 Lagrangian simulations", (L185) : "In IndOc.HR-Sto, the total current also includes the wave-induced Stokes velocities as shown in eq. 1. In this later case, 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."

- Include relevant formulas and refer to established methods, or detail any deviations from them.

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

- (L171, 176) “Stokes drift” changed to “Stokes velocity” and adding “the effect of surface waves on Eulerian currents via the Stokes–Coriolis term in the momentum equations (see section 2.1.5).”

We think that this clarification meets the reviewer’s expectations and contributes to a clearer understanding of our methodology.

2. Model-observation comparisons

The approach to model validation needs clarification:

- Are all datasets interpolated to a common grid prior to computing correlations?

> To compute correlations between the simulations and the different observation products, the SYMPHONIE model outputs are interpolated onto the corresponding observation grids, i.e. SST from IndOc.HR, IndOc.HR-Sto and IndOc.12 is interpolated to the OSTIA grid (0.05°), SSS to the CCI grid (0.5°), SLA to the CMEMS grid (0.25°). To clarify it, we have added the following sentence at the beginning of Section 3 “Evaluation of the simulations and circulation analysis”: (L268) “For model-observation comparisons through bias and correlation calculations, the SYMPHONIE model outputs are interpolated onto the corresponding observational product grids, i.e. SST to OSTIA (0.05°), SSS to CCI (0.5°), and SLA to CMEMS (0.25°).”

- How might the applied nudging influence the agreement with observations?

Further detail on the nudging (location, depth, timescale) is needed as well, see also specific comment below.

> We have clarified in (L126) (see also responses to comments below) that nudging is applied only in the southeastern part of the domain (south of 50°S and near Australia) where the telescopic grid is coarser than the 1/12° GLORYS forcing, and also vertically over the full water column. None of the three regions evaluated here (ArS, BoB, MoC) are affected by this nudging, thus without direct impact on the agreement showed with observations in our evaluation section. Moreover, the satellite products used for evaluation (OSTIA SST, CCI SSS, CMEMS SLA) are different from the GLORYS dataset used for nudging.

Technically, the nudging is implemented through a sponge layer whose intensity decreases with distance to the open boundary. The relaxation coefficient follows a 1/2-cosine shape in the case of constant resolution. In our variable-resolution grid, the sponge is instead defined as a function of the grid spacing Δx relative to a critical resolution $\Delta x_{crit}$ = 12 km such as:

$$sponge = 0.5 * (1 + \tanh [ (\Delta x - \Delta x_{crit}) / \Delta x_{width}])$$

With $\Delta x_{width}$ = 2 km. This function is equal to 0 where the resolution is fine (Δx < 7.5 km), equal to 0.5 where $\Delta x = \Delta x_{crit}$ = 12 km, and approaches 1 where the resolution is much coarser. The dimensionless sponge coefficient is then scaled by the inverse relaxation timescale 1/T0.

3. Land-sea transfers

The abstract sets the goal to address land–sea transfers, but no direct analysis of this is presented. Lagrangian experiments are based on offshore releases, and sensitivity tests focus on grid resolution and wave forcing. The influence of more realistic river discharge, while implemented, is not tested. This feels like a missed opportunity. For example, exploring how coastal retention changes with the new configuration could strengthen the manuscript's relevance considerably. If the authors choose not to pursue additional analyses, I suggest reformulating the manuscript's goals to avoid overstating its scope.

> We acknowledge that the Lagrangian experiments conducted in this study do not address land-sea transfers, focusing instead only on sensitivity tests involving particle releases at sea. This decision was made because we are preparing a second publication focusing on the analysis of Lagrangian simulations based on realistic scenarios at river mouths. These simulations have already been produced and need further analysis. However, their analysis is not included in this first paper due to constraints on the length of the manuscript and also for more clarity. In this first manuscript, we have chosen to demonstrate only the effect that different dynamic forcing choices can have on trajectories. In a second step, we will examine the fate of river particles from the coastal zone to the open ocean. This highlights the importance of integrating river discharge forcing in the best possible way, as described here.

To avoid misunderstanding, 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."

And also we give more details on the perspectives into 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."

Specific comments:

- L 9-10: "Three annual experiments are conducted, alternatively considering Stokes drift forcing and different grid resolutions". This sentence is ambiguous about the exact combinations of Stokes drift forcing and grid resolution used in the three experiments. Please clarify.

> We have clarified the sentence such as (L9) "Three annual experiments are conducted, exploring the effects of wave forcing and spatial resolution. The reference simulation (IndOc.HR) uses high resolution without wave forcing, IndOc.HR-Sto uses the same resolution but includes wave forcing via the Stokes-Coriolis term, and IndOc.12 is a lower-resolution simulation without wave forcing."

- L 39: "Lagrangian and Eulerian dispersion modeling studies can help to fill this knowledge gap". Why are Eulerian dispersion modeling studies mentioned here? Consider omitting "Eulerian" dispersion modelling here, as all examples relate to Lagrangian methods.

> We agree and have removed "Eulerian" from this sentence.

- L 57 ff: "Stokes forcing related to waves can also have a significant impact on the dispersion of floating material […], including indirectly the effect of windage on surface particle drift". This sentence is inaccurate. In principal, Stokes drift, Stokes forces, and windage are individual processes affecting marine matter transport. If Stokes drift is parameterized via an additional transport component in wind direction, the corresponding tuning factor can also be chosen to include windage. But if Stokes drift obtained from a wave model is included in the transport simulations, that does not necessarily include any windage component. Please correct.

> We agree that the original sentence was misleading. We have revised it by adding details on waves processes and windage with literature references (L61 - see comment above).

- L 65 ff: "One of the main challenges in modeling the dispersion of marine debris is the necessity to study the continuity of dispersion patterns from […] coastal scales […] to large scale ocean currents […]". I completely agree! However, as summarized above, I think the current set of analyses unfortunately does not convincingly demonstrate how the new model configuration tackles this.

> We agree, and as explained above, we have performed Lagrangian simulations based on realistic river scenarios, but for reasons of manuscript length and clarity, we prefer to dedicate this first publication to the evaluation of hydrodynamic simulations and Lagrangian sensitivity tests, and we are preparing a second publication in which we will analyze the fate of particles released at river mouths, from the coastal zone to the open sea. We therefore propose adding the following sentence (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."

- L 70: "stranding issues" It is not clear what is meant by issues here, please clarify.

> We have done the following clarification (L78): "where the main sources of debris are located and where plastic stranding poses significant socio-environmental challenges"

- L 81: "floating particle" This terminology is recurrently used within the manuscript and implies particles remain at the surface. However, the method part introduces the trajectories as 3D, including vertical displacements (cf. l. 155 ff.). "Buoyant particles" may be more appropriate to use.

> We used the term "floating" considering these particles to be in the upper layer of the ocean with rising velocities, but they are indeed not always at the surface. We agree to replace this term with "buoyant" throughout the manuscript to make it less confusing.

- L 103: "Bathymetry is built from […] with manual verification and modification of the coastline […]. Without further explanations, these kind of manual modifications compromise reproducibility. Publishing the modified bathymetry along with a documentation of the performed changes is strongly recommended.

> We acknowledge the concerns regarding reproducibility. The minor verification and adjustment of the coastline mentioned were performed during the grid generation process using the grid construction module of the SYMPHONIE model, with the aim of verifying key topographic features. The resulting model grid is provided with the manuscript to ensure full reproducibility of the simulations presented in this study. We have also revised this part of the sentence to avoid any misunderstanding or confusion (L113): "Bathymetry is built from an interpolation of the GEBCO-2021 product. Depth ranges from 1 to -7459 m in the domain."

- L 116 ff: "A temporal nudging layer is configured wherever the resolution of the telescopic grid is lower than the 1/12° GLORYS forcing (to the southeast)." This sentence and the corresponding paragraph remain unclear about whether nudging is only applied at the lateral open boundaries or also within the domain. This should be stated explicitely, as it also impacts the interpretability of the validation of modeled SST and SSS (which naturally would be expected to be very good for regions where nudging is applied).

> To complete our response above, we confirm that the temporal nudging is also applied within the domain but only in areas where the horizontal grid resolution is coarser than the 1/12° forcing. As illustrated by Fig. 1, this condition is satisfied only south of 50°S and at the easternmost edge of the domain near Australia. Importantly, it does not impact the interpretation of the SST and SSS validation, since the three evaluated regions (ArS, BoB and MoC in Fig. 1) are not concerned by the nudging. Moreover, the satellite-based products used for the model evaluation are different from the GLORYS forcing used for the nudging. We have updated the manuscript to clarify the spatial extent of the nudging and to explicitly mention this point (L125): "A temporal nudging layer is configured in regions where the resolution of the telescopic grid is coarser than that of the 1/12° GLORYS forcing. This only occurs is the southeastern part of the domain, south of 50°S and around Australia, as shown by the grid resolution in Fig. 1. In these areas, temperature and salinity fields are relaxed toward their better-resolved GLORYS counterpart, over the full water column, using a relaxation time scale that decreases smoothly with the difference in resolution between two grids. Relaxation is therefore greater at the southeastern edge of the domain, with a minimum time scale of 30 days. This nudging does not impact the three regions selected for model evaluation (ArS, BoB, MoC), ensuring that it does not artificially improve the agreement with observations in these areas."

- Figure 1:"blue boxes in the best-resolved regions […] are used to calculate spatial averages […] and Lagrangian trajectories presented in the following". Not clear whether this refers to the big or small blue boxes (there are two frames in each mentioned region); "presented in the following" reads weird in a figure caption (as nothing follows directly)

> We specified as follow in the caption of Fig. 1: " Blue boxes in the best-resolved regions (ArS: Arabian Sea, BoB: Bay of Bengal, MoC: Mozambique Channel) are used to calculate spatial averages of temperature, salinity, sea elevation, energy spectra and Lagrangian trajectories (larger boxes for Figs. 6 and Table 2 ; smaller boxes for Figs. 2, 7, 9 and 10). "

- L 136: "This parameterization uses the surface Stokes drift (extrapolated below the surface using the peak period)" a formula and reference would be helpful.

> We have moved the concerned references at the beginning of the sentence such as (L149): "Following a similar approach to McWilliams and Restrepo (1999); Jordà et al. (2007); Gentil et al. (2022); Weiss et al. (2024b), this parameterization consists in an online one-way forcing utilizing the surface Stokes velocity (extrapolated below the surface using the peak period)".

- L 140 ff: "In practice, Stokes drift is considered through the transport calculation in the model's Eulerian equations. In parallel, the momentum equations take into account the anti-Stokes term, […]." What Eulerian transport equations exactly, are you referring to tracer transport here? Adding the respective equations would be helpful.

> (L158) We have added the equations (1) and (2), demonstrating how the wave effect is included in the Eulerian current calculation.

- L 157: What type of interpolation scheme is used?

> We thank the reviewer for the question. The interpolation of velocity fields to particle positions is performed using linear interpolation in the three spatial dimensions and in time. This choice is consistent with the second-order Runge-Kutta (RK2) time integration scheme used here. With linear

interpolation, RK2 scheme is exact and provides the same accuracy as higher-order scheme like RK4, while being computationally much less expensive. In contrast, nonlinear interpolation of the current field would require a significantly higher computational coast (scaling from 16 to 256 terms per particles) and may introduce spurious extrema near fronts. Linear interpolation is therefore both efficient and robust for our application. We have added the following sentence (L189): "The current velocities are interpolated linearly in space (3D) and time at each particle position."

- L 164 ff: Please specify the velocity fields that are used for the advection, at best via formula. Specifically, for IndOc.HR-Sto: Did you use only the modelled Eulerian current fields (that include the effect of the Stokes-Coriolis force), or did you use these Eulerian current fields plus Stokes drift?

> To answer this comment, we have already revised section 2.1.5 to clarify our way to consider the wave effect on Lagrangian transport, which is indirectly through wave-induced modification of Eulerian currents, including anti-Stokes forces such as the Stokes-Coriolis force. The Stokes drift is not explicitly added to the Lagrangian advection scheme. Particles are advected only by the Eulerian currents resulting from the hydrodynamic model, in which the effect of waves is dynamically integrated via forcing in the momentum equations.
Moreover we have completely rearranges section 2.2.2, showing the advection equation (eq. 3) and specifying (L185): "In IndOc.HR and IndOc.12, the total current is equal to the resolved Eulerian velocities, i.e., $u = \hat{u}$ and $v = \hat{v}$. In IndOc.HR-Sto, the total current also includes the wave-induced Stokes velocities as shown in eq. 1. In this later case, Lagrangian particles are advected by the Eulerian current fields dynamically modified online by wave forcing through the Stokes–Coriolis term, but without any explicit addition of Stokes drift in the Lagrangian advection scheme."

- L 344 ff: "SLA correlations are slightly lower than those for SST and SSS, due to the complexity of mesoscale dynamics and intrinsic variability." Please explain why SLA is stronger impacted by intrinsic variability than SST and SSS. Could this to some degree also be related to the nudging?

> We do not believe that the nudging plays a significant role in the lower SLA correlations. If we look at Fig. 5, we can see that the discrepancies between the simulations and the observations (bias figures) are spatially consistent for both SST and SLA, i.e. mainly where many eddies are formed such as in the Agulhas system. The SLA correlations are lower, presumably because of the coarse resolution of the satellite-derived SLA product used (0.25°~25 km) compared to the simulations (2 to 9 km), limiting the representation of fine scales in the observations. The SLA is more sensitive to submesoscale and mesoscale variability than tracers like SSS and SST. Even slight differences in the position of eddies or fronts can lead to high variations in the SLA values, or even opposite biases (since SLA represents anomalies, unlike SSS and SST). SST and SSS fields tend to show smoother gradients on the temperature or salinity scales, and are less impacted by small-scale positional mismatches. We reiterate here that the nudging area is minor in the evaluated domain. We have added this hypothesis after the mentioned sentence as follow (L386): "The SLA correlations are slightly lower than those for SST and SSS, due to the complexity of mesoscale dynamics and intrinsic variability, as well as the higher sensitivity of SLA to the location and structure of mesoscale features. The SLA discrepancy might be further amplified by the coarser resolution of the satellite-derived SLA product (0.25°) compared to the model resolution (2–9 km), which can lead to higher decorrelation when comparing fine scale dynamics. SST and SSS are generally less affected by such discrepancies due to their smoother spatial gradients."

- L 358: "This might be attributed to wave-induced surface momentum fluxes" This sentence surprised me, as no information about wave-induced surface momentum fluxes is given in the model description. Please clarify how these type of wave effects were included in the model (see general comment above).

> We agree that the formulation "wave-induced surface momentum fluxes" was misleading, as our IndOc.HR-Sto simulation does not explicitly add wave radiation stress or other surface momentum fluxes from waves. The only wave-related dynamical effect added in this simulation compared to others is the Stokes-Coriolis term, implemented by modifying the momentum equations based on the surface Stokes velocity forcing. We have revised the sentence in the manuscript such as (L404): "This might be attributed to the Stokes-Coriolis forcing, which may modify the surface dynamics and current shear."

- L 363 ff: "For SSS, IndOc.12 clearly outperforms the HR configurations in terms of NRMSE and bias, […] This counterintuitive result may be related to the coarser resolution [of the observational reference]" Please rephrase the "outperforms" statement. If the observational product is too coarse, then better agreement may not indicate better performance.

> Thank you for this relevant comment, we agree that lower error values against a coarse resolution observation do not necessarily imply better model performance. We reworded the sentence to avoid the term "outperforms" such as (L409): "For SSS, IndOc.12 shows lower NRMSE and bias than the HR configurations, particularly in BoB (NRMSE = 6%, bias = -0.04 psu) and MoC (NRMSE = 33%, bias <0.01 psu). Temporal correlations also remain high (R > 0.94 everywhere). This result, although counterintuitive, may reflect a better consistency between the coarser CCI resolution (0.5°) and the smoother representation of coastal salinity gradients in our low resolution IndOc.12 simulation, rather than an improved physical realism."

- L 396: Consider replacing the word "performance" for reason listed in previous comment

> We revised the sentence as follow (L443): "Interestingly, IndOc.12 shows similar or even lower SSS errors in some regions, possibly benefiting from a resolution closer to that of the CCI product."

- Section 3.3 "Description of the regional surface circulation" The title is misleading, as mostly not circulation itself but impact on SST and SSS is discussed. Please also clarify the relevance of this section. Is that still part of the model validation?

> The original title of Section 3.3 may indeed not be fully adapted, as the section focuses primarily on the monsoonal variability of SST and SSS rather than a direct analysis of surface circulation. This section provides a qualitative assessment of the model's ability to reproduce key dynamical processes of the Indian Ocean, through the seasonal description of surface tracers, which is consistent with the known regional ocean patterns described in the literature (Schott and McCreary, 2001; Schott et al., 2009). It is not a formal validation but this section supports the good representation of the simulated circulation and completes the more quantitative evaluation presented in sections 3.1 and 3.2. We revised the section title to: "3.3 Seasonal variability of surface tracers and inferred dynamics". And we specified in the first sentence of the section (L449): "This section interprets the seasonal variability of SST and SSS, and the associated regional circulation patterns, which aligns with key dynamical features described by Schott et al. (2009), using the same current naming (see Fig. A1)."

- L 439 ff: I assume the Eulerian mean kinetic energy is calculated for comparison between the different model runs. However, as the Lagrangian mean kinetic energy would potentially be even more relevant for transport studies, this should be specified at the beginning of this section

> The kinetic energy presented in section 4 corresponds indeed to the Eulerian mean kinetic energy, computed from the simulated velocity field. Analyzing the Lagrangian mean kinetic energy would also be interesting to see how particles are affected by the different forcing current fields. In our case, since the Lagrangian trajectories are computed offline using the analyzed Eulerian current fields, this energy analysis supports the interpretation of differences observed in the Lagrangian experiments. We have added clarification in the manuscript (L488): "Although Lagrangian kinetic energy (KE) computed along particle trajectories, would provide a more direct estimate of the energy experienced by marine debris, we focus here on Eulerian KE derived from the model gridded velocity field. This allows for a consistent comparison between simulations."

- Section 5: The "mean spread distance" is introduced and analyzed. Yet, this seems to refer to the commonly used "displacement". If so, please adopt standard terminology.

> For more clarity and in accordance with standard terminology, we have followed the reviewer's suggestion and changed the term "travel distance" to "trajectory length", i.e. the total length traveled by the particles, and the term "mean spread distance" to "displacement", i.e. the straight-line distance between the initial and final positions of the particles. These changes affect the Figs. 11 and 12, captions and text of section 5.

- L 541 ff: "Similarly, 122 to 143 km separate the median trajectories between IndOc.12 and both HR simulations." What is the median trajectory? The median distance travelled by the trajectories differs by 122 to 134 km? Or the final position of the centroids (average position of all particles) lay 122 to 134 apart?

> Here we meant the median trajectory lengths: the median length of the trajectories obtained from IndOc.12 is 122 km lower than that from IndOc.HR and 134 km lower than that from IndOc.HR-Sto. We clarified the sentence as follow (L591): "Similarly, the median trajectory length obtained from IndOc.12 is 122 km shorter than in IndOc.HR and 143 km shorter than in IndOc.HR-Sto."

- L 600 ff: "The effect of the Stokes Drift is less significant either on the statistics of the distances traveled (Fig. 12) or on the direction of the trajectories (Fig. A11)." This sentence is not clear. What is the less referring to?

> We clarified this sentence such as (L651): "The difference in statistics between IndOc.HR-Sto and IndOc.HR is smaller in the MoC compared to the other two regions, indicating that the effect of Stokes velocity forcing is less significant here, both on the distance traveled (Fig. 12) and on the direction of the trajectories (Fig. A11)."

- Figure 13: Nice visualization! > Thank you very much for your positive feedback!

- Appendix A: please specify all variables and parameters used in the equation. Specifically, define n, the velocity components, and averaging periods for mean velocities.

> We have added the definition of the corresponding variables in the Appendix A, following the equation descriptions.

Technical corrections:

- L 32: “submesoscale are ephemeral (…)” -> submesoscale features are ephemeral : we have made the suggested changes
- L 36: “modeled hypotheses” -> model-based hypotheses : modification completed
- L 59: configurations -> ocean model configurations for the Indian ocean : we have added the suggested clarifications
- L 63: “none of them […] and use ” -> “they do not […] and do not use” : corrections completed
- L 98: The mentioning of the number of cells after the clause about limiting open boundaries is ambiguous. If the total grid size is meant, consider rewriting, e.g., "The grid is a curvilinear Arakawa C-grid (3200 x 2800 cells) covering the entire Indian Ocean […], thereby limiting the number of open boundaries." : Thank you, we have made the suggested changes. We also realized that this section only describes the IndOc.HR grid, we therefore added another sentence to also describe the IndOc.12 grid such as (L114): “The lower resolution configuration (IndOc.12) uses the same features as IndOc.HR, except for its horizontal discretization, which is regular at 1/12°.”
- L 676: There appears to be some redundant text after the acknowledgment section, please remove. : Thank you for pointing out this correction.

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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

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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.

References:


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Rocha, C. B., Chereskin, T. K., Gille, S. T., and Menemenlis, D.: Mesoscale to Submesoscale Wavenumber Spectra in Drake Passage, J. Phys. Oceanogr., 46, 601–620, https://doi.org/10.1175/JPO-D-15-0087.1, 2016.

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