Final response to discussion comments for manuscript "Non-negligible impact of Stokes drift and wave-driven Eulerian currents on simulated surface particle dispersal in the Mediterranean Sea"

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Format:
comments from referees in black
author's response in blue (with reference to line numbers in original version of manuscript)
author's changes in the manuscript in red (with reference to line numbers in revised version of manuscript)

RC1: 'Comment on egusphere-2024-1002', Tamay Ozgokmen, 02 May 2024

I agree with the main premise & conclusion of this manuscript that Stoke's drift, very near surface currents via coupled modeling are critical to dispersion of surface material (such as oil spills, and microplastics). The manuscript is solid, well written, includes insightful test cases. The authors are informed scientists in this field.
We greatly appreciate the positive feedback on our manuscript and the pointers to additional important references.

The main deficiency of the manuscript is that it is modeling only and lacks real-data verification... which is very very hard, so understandable.
We agree that data verification of our results would be desirable. However, deciphering and separating the individual wave-induced processes that alter Lagrangian surface velocities from observations is indeed beyond the scope of this study, as it poses a scientific challenge on its own (which we discuss in section 5.1, lns 630-649, of the originally submitted manuscript). We specifically designed this study as conceptual model work to be able to not only derive the Stokes drift but also distinguish wave-driven Eulerian currents from non-wave driven Eulerian currents.

Also, some relevant work is not cited:

Curcic, M., S.S. Chen and T. M. Özgökmen, 2016: Hurricane-induced ocean surface transport and dispersion in the Gulf of Mexico. Geophys. Res. Lett., 43, 2773-2781.

Laxague, N.J.M., T.M. Özgökmen, B. K. Haus, G. Novelli, A. Shcherbina, P. Sutherland, C. Guigand, B. Lund, S. Mehta, M. Alday, and J. Molemaker, 2018: Observations of near-surface current shear help describe oceanic oil and plastic transport. GRL, 45(1), 245-249.

The first paper involves testing with a truly coupled ocean-wave-atmosphere model with using real data and the second involves current measurements going all the way to top 1 cm near the air-sea surface (which was very hard to pull off); could be interest to the authors. (I had to declare my identity since I am posting these papers.)

We incorporated the results of Laxague et al. (2018) into our discussion of a revised version of our manuscript, as these results raise an important limitation of all larger-scale applications that typically make use of Lagrangian velocities estimated from Eulerian currents averaged over the upper meter(s) of the water column.
Lines 699-707: "Further, it is important to note that throughout the manuscript we use the term surface to refer to the circulation depth-averaged over the upper meter of the water column (which corresponds to the thickness of the uppermost depth level in our model configuration). This is common for dispersal simulations that rely on ocean models covering scales larger than typical coastal and regional scales. However, Lagrangian velocities in the upper few centimeters of the water column may be significantly stronger compared to those averaged over the upper meter, due to strong vertical shear of the Eulerian currents and Stokes drift arising, for example, from microscale wave breaking and skin friction (Laxague et al., 2018). This implies that, strictly speaking, our results are not directly applicable for particles bound to the upper few centimeters of the ocean, such as non-emulsified and emulsifying oil as well as macro- and mesoplastic, but are more representative for slightly submerged particles at 1m depth, such as microplastics."

While we read Curcic et al. (2016) with great interest, we find their results slightly less relevant for our study. Curcic et al. (2016) provide an impressive example of how Stokes drift can have a substantial impact surface dispersal during extreme events based on the combined evaluation of data from a drifter release experiment and a coupled atmosphere-wave-ocean model. However, extreme events are – as mentioned in section 3.1.3, ln. 264, of the originally submitted version of the manuscript – not the focus of our study. Moreover, we are particularly interested in the relative impact of Stokes drift versus wave-driven Eulerian currents; but the model employed by Curcic et al. (2016) does not incorporate the Stokes-Coriolis and vortex forces and hence does not allow for distinguishing the effect of wave-driven Eulerian currents. Having said all this, given the results of Curcic et al. (2016), it may be interesting to conduct follow-up studies investigating the relative importance of Stokes drift versus wave-driven Eulerian currents during extreme events. We added a respective statement to the discussion in the revised version of the manuscript, including the reference to Curcic et al. (2016).
Lines 628-633: "Open questions to be addressed by these follow-up studies include: […] What role do extreme events play? Stokes drift has been shown to have a significantly increased impact on surface particle dispersion during, e.g., hurricanes (Curcic et al. 2016), but it is still unclear whether the impact of surface wave-driven Eulerian currents is increased proportionally."

RC2: 'Comment on egusphere-2024-1002', Brandon Reichl, 20 May 2024

This manuscript analyzes the impact of including ocean surface gravity waves when simulating ocean currents that are used for Lagrangian advection. The conventional approach is to consider the effect of surface waves on Lagrangian particles through their phase-averaged Stokes drift. Previous studies have therefore considered adding the Stokes drift to the output of ocean models that did not account for wave driven processes. This study employs an ocean circulation model that is coupled to an ocean surface wave model, and thus considers the impacts of surface waves on the ocean circulation model physics, including through the impact of waves on vertical mixing, surface fluxes, and on the resolved scale currents/advection in the ocean model. The results show that Lagrangian parcel simulations using the ocean circulation model with full wave coupling yields a different result from simulations using the ocean model without wave coupling. This difference cannot be accounted for by adding the Stokes drift to the output of the non-coupled model. The study therefore suggests that the feedbacks of ocean waves to modify the background current should be properly accounted for when using output of numerical ocean circulation models to drive Lagrangian particle simulations.

I found this study to be well-written, thoughtful, and important for the ocean modeling community. I have a few comments on the presentation that I think can better clarify the result and its place within the scope of similar literature.
We greatly appreciate the positive feedback on our manuscript and the concrete and very valuable suggestions for improvements.

General Comments

1. A general comment for definitions of the decomposition in the equation at L50. The Lagrangian current is routinely separated into an Eulerian component and a Stokes drift component that is attributed to the surface wave field. In this study the Eulerian current is further separated into a "non-wave" component U_Enw, and a wave-driven component U_Ew. The non-wave component is later defined from the non-coupled simulation and the wave component is defined from the residual of the coupled simulation minus the non-coupled simulation. This is useful conceptually to decompose the Eulerian current and explain the results. The approach here is pragmatic, but I do think it is important to make the choice of this definition clear early in the paper (e.g., as is conveyed later in Table 2). There are non-linear terms when you back substitute U_Ew and U_Enw into the momentum equations, such that one could consider more refined ways to decompose the wave-driven part, not just from this residual approach.
   Thank you for raising this point. We made sure to highlight our pragmatic choice already in the introduction by adding a respective sentence in the revised version of the manuscript.
   Lines 45-48: "[…] the presence of surface waves alters the Eulerian current field itself via various (partially non-linear and interacting) processes. By pragmatically defining wave-driven Eulerian currents as the residual of the circulation with and without wave forcing, the Eulerian velocity can then be decomposed into a wave-driven component $\boldsymbol{u}_{Ew}$ and a non-wave-driven component $\boldsymbol{u}_{Enw}$ (e.g., Cunningham et al., 2022)."

2. It is important to clarify that for the Wagner et al. (2022, also disclosing that I am a coauthor on that work) work we conducted our experiments on a 25 km ocean-wave model and resolved wave-current interactions at that scale. There are other differences between this work and our simulations (e.g., more wave physics impacts than just resolved-scale wave-current interactions are considered in this work), but I personally did not find it surprising or controversial that a different result may be found at 4km resolution with wave-current interactions resolved at smaller scales. The results may in fact be compatible, perhaps yielding insight into the scales where the resolved scale impacts are important.
   Thank you for providing this alternative interpretation of the differences between Wagner et al. (2022) and our study. In the revised version of the manuscript, we added a respective comment to the discussion section.
   Lines 656-657: "Differences in spatial model resolution may also be a potential reason for the differences between our work and Wagner et al. (2022). "

Specific Comments

L53: I suggest using different language, "explicitly resolved" to me implies simulating the surface phases of waves directly by the ocean model. But I think it is meant that they are sometimes coupled to spectral wave models.
We agree that the formulation was misleading. We use a better formulation of the respective sentence in a revised version of the manuscript
Lines 53-54: "[…] velocity output from ocean-only models without representation of surface wave effects".

L115: I don't think Craig and Banner (1994) is the best reference for Langmuir turbulence. Perhaps McWilliams et al. (1997, doi:10.1017/S0022112096004375) and other more recent reviews (e.g., Belcher et al., 2012 doi:10.1029/2012GL052932, D'Asaro, 2014 doi:10.1146/annurev-marine-010213-135138)?
We replaced Craig and Banner (1994) by McWilliams et al. (1997).
Line 118.

L148: But it does neglect the feedbacks of U_Ew on U_Enw. This is a benefit of the approach here, could that be tested here?
While we agree that it would be very valuable to test how this approximation compares to our coupled ocean-wave model approach, we are of the opinion that this requires a study on its own. The Higgins et al. (2020) approximation relies on several rather strong assumptions (most importantly, a constant laminar viscosity instead of a more realistic turbulence model and monochromatic waves), and preliminary analyses of a colleague of ours (unpublished) revealed relatively large sensitivities of simulated dispersal pathways to the exact formulations of these assumptions. In lines 303-305 of the original manuscript we state and rationalize our choice to not include the Higgins et al. approximation in our analyses: "We do not include particle dispersal simulations with the advanced approximation, as this approach is (so far) not widely used and represents an intermediate step between simulations with the basic approximation and our best guess, with presumed limited additional value for answering the research questions outlined in Sect. 1."

L152: “controversy” seems like an overly strong word to me.
We replaced “help solving the long-standing controversy around” by “yield further insights on”.
Line 155.

L166: WaveWatch -> WAVEWATCH.
Thanks for spotting the typo. We corrected it.
Lines 169.
.
L169: despite -> except
We changed the wording as suggested.
Lines 172.

L175: What is the first cell thickness? I can imagine the results could be sensitive if the first cell is particularly thin or coarse.
The thickness of the first layer in our ocean model configuration is approximately 1 m, which is common for ocean models covering scales larger than coastal and regional. We are aware that Lagrangian velocities in the upper few centimeters of the water column may be significantly stronger compared to those averaged over the upper meter, due to strong vertical shear of the Eulerian currents and Stokes drift. This caveat was also mentioned by reviewer 1, and we included a respective discussion in a revised version of our manuscript following the argumentation in our response to reviewer 1.

L200: Cell horizontal interfaces or vertical interfaces or both?
The horizontal velocity components of the Stokes drift are evaluated at the horizontal grid-cell interfaces. This info was added.
Lines 202-204: “Due to the C-grid implementation, the horizontal velocity components of the Stokes drift are evaluated at the horizontal grid-cell interfaces. Divergence of the horizontal Stokes drift velocities induces an additional vertical velocity component w_S, which can be derived from the continuity equation […].”

L220: Are there any citations for this? Which specific “TKE” scheme is it? A k-l type, a Mellor-Yamada, GLS, etc.? Is the momentum flux directed only down the Eulerian vertical current shear or also down the Stokes gradient (e.g., Harcourt 2013, doi: 10.1175/JPO-D-12-0105.1)? This detail could be important since down-Stokes mixing can be an additional source of “anti-Stokes” current.
The TKE scheme is based on a prognostic equation for the turbulent kinetic energy, and a closure assumption for the turbulent length scales as described in the NEMO 4.2.0 manual (https://zenodo.org/records/6334656, p130-135); TKE production via Stokes drift shear is considered (as indicated in ln. 220 of the original manuscript). Couvelard et al. (2020) describes the improvements of the representation of vertical mixing using this modified TKE scheme. The employed TKE scheme is different from the GLS vertical mixing scheme, which uses a flexible approach with a generic length scale, representing various turbulence models (like kepsilon, k-omega, or Mellor-Yamada) in a single framework. We reworked the paragraph describing the TKE scheme to clarify/include the aspects you raised.
Lines 221-231: “The sub-grid scale physics are altered by modifications in the turbulent kinetic energy (TKE) closure scheme to better account for wave-driven mixing, including an advanced Langmuir turbulence parameterization. In the employed closure scheme, the vertical eddy viscosity and diffusivity coefficients are derived from a prognostic equation for the TKE and a closure assumption for the turbulent length scales, as described by Madec and

the NEMO System team (2022). The temporal evolution of TKE is computed via its production through vertical current shear and Langmuir turbulence, its destruction through stratification, its vertical diffusion, and its dissipation. In the coupled simulation, the TKE equation includes an extra term for the production of TKE via Stokes drift shear. In addition, the TKE surface boundary conditions, the mixing length scale, and the dissipation length scale are modified to incorporate wave-induced changes in surface roughness and wave breaking. The Langmuir turbulence parametrization, already employed in the stand-alone ocean model simulation, is expected to be more realistic in the coupled simulation by using the sea-state dependent Stokes drift obtained from the wave model instead of an approximation of the Stokes drift based on the surface wind stress.

L225: Is there a citation for this? Otherwise, it may be better to say the parameterized Langmuir turbulence is expected to be more realistic with the simulated, sea-state dependent Stokes drift than the wind speed based Stokes drift.
We adjusted the sentence as suggested.
Lines 228-231.

L3.1.2: Some discussion of the wave model performance in this configuration would be helpful. Are there obs comparisons in a previous study that can be cited?
The coupled ocean-wave model simulations we employed is described and validated in a technical report (Moulin and Clementi, 2024), as noted in lines 172-173 of the original manuscript. Within this report, among others, the simulated significant wave height is compared to satellite observations (cf., Figure below). The simulations show a good fit to the observations, with a correlation coefficient of 0.956; though the simulations slightly underestimate the significant wave height, especially for lower values. We added statements on the model performance to the main manuscript.
Lines 232-234: "The inclusion of the wave coupling was shown to improve the simulated upper ocean circulation pattern and thermohaline properties, as evidenced, for example, by reduced root mean square errors for temperature and salinity in the upper 150 m in comparison with ARGO observations (see Moulin and Clementi (2024) for a detailed model validation)."
Lines 248-250: The wave characteristics in the coupled ocean-wave model simulations show a good fit to observations, which is illustrated, for example, by a very high correlation in the significant wave height obtained from the simulations and those inferred from satellite observations (correlation coefficient of 0.956, see Moulin and Clementi (2024) for a detailed model validation).

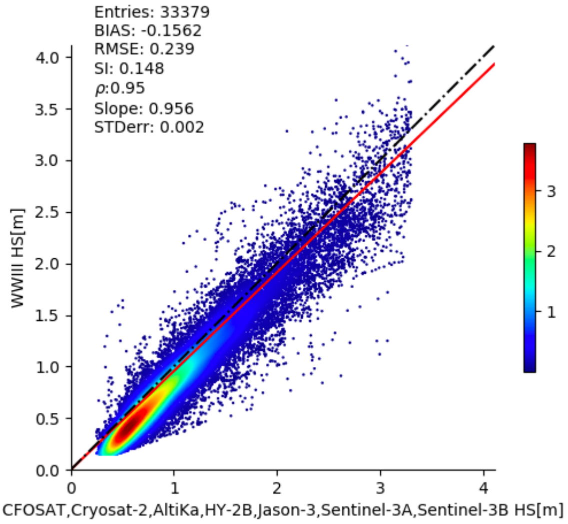

Figure 1: Scatter plot of significant wave height (H_S) [m] providing a comparison between satellite observations and numerical simulations for the period 2019 to 2020. Dot colors refer to the data probability density; black dashed line represents the best-fit (1:1); solid red line shows the satellite-model data fit. Adapted from Moulin and Clementi (2024).

L229: Is there a spectral tail added for the Stokes drift computation?
No, in this simulation we use the default version of WW3 v6.07. In this version, the tail for surface Stokes drift is specifically commented out as it is very sensitive to tail power. We did one simulation adding the tail for the Stokes drift, but results gave larger errors in comparison to observations.

Table 1: It would be inconsistent to include some of these Stokes drift impacts in NEMO without others, so I suggest not splitting into three subcolumns when intermediate experiments aren't attempted. I didn't catch the distinction between the modified TKE scheme [note typo in manuscript] and Langmuir turbulence parameterization, if more details are given as noted by comment at L220 it would help here.
We ask to retain the table in its current form, as it provides an overview of which wave-driven processes are included in the coupled and non-coupled experiments (in particular, as there are coupled ocean-wave modes that use alternative formulations of the primitive equations with different Stokes terms). Regarding the differences between the Langmuir turbulence parameterization and the modified TKE scheme: the non-coupled simulations also already include a basic Langmuir turbulence parameterization in the TKE scheme with an approximation of the Stokes drift based on the surface wind stress. In the coupled simulations, this Langmuir turbulence parameterization is adjusted to use the Stokes drift from the wave model. Moreover, in the coupled simulation the TKE scheme includes extra terms for the wave impact, e.g., to take into account the contribution of Stokes drift shear. As indicated above, we reworked the paragraph on the TKE scheme to clarify these points.

L244: Why not spin-up the coupled version from rest? Is it possible that analysis in early 2019 is contaminated from the initial adjustment?
In our opinion, spinning up the coupled model from rest results in more transients/contamination than initializing it with fields from the previously run non-coupled model. Our initialization was chosen to allow for estimating the wave impact as the residual between the two model simulations. Spinning up the coupled model from rest could result in significant differences that are not directly due to the wave impact but rather due to intrinsic variability acting like initial perturbation in the model.

L252: I don't expect these results to be particularly sensitive to this detail, but I'm surprised that the atmospheric fields are only updated every six hours. This seems fairly coarse in time at ~10km spatial resolution. Are the fields interpolated in time to force NEMO and WAVEWATCH?
The ECMWF forcing fields are available 6-hourly, but the forcing fields are interpolated in time for both NEMO and WAVEWATCH and are updated every 1h. We included a clarifying sentence in the revised manuscript.
Lines 264-265: "Both hindcast experiments were atmospherically forced by 6-hourly operational analysis and forecast fields from the ECMWF at 1/10° horizontal resolution. The forcing fileds were interpolated in time to provide hourly updates."

L266: Suggest to clarify if Stokes drift is similarly averaged over 1m.
Thanks for spotting this inaccuracy. We added the clarification in the revised manuscript.
Lines 278-279: "[…] horizontal surface Eulerian current and Stokes drift velocities (both averaged over the uppermost cell of approximately 1 m depth […]".

L295: Specify horizontal grid, vertical grid, or both

We added the information that Stokes drift is obtained on the same horizontal and vertical grid as the Eulerian currents.

Lines 307-308: "It is to note though that the Stokes drift was obtained from the ocean model component on the same horizontal and vertical grid as the Eulerian currents […]."

L302: This seems like a missed opportunity in this study, otherwise it leaves an open question if ocean circulation models need to include full wave physics or the effect can still be partially accounted for via intermediate approaches. It seems very relevant to me to answer the second question. Can the authors offer some comments on its potential utility in Section 5.1?

We agree with the reviewer that a comparison with the advanced approximation by Higgins et al. (2020), which has been implemented for plastic dispersal simulations in Cunningham et al. (2022), would be the next logical step to identify which of the wave-driven processes are key for faithful simulation of particle dispersal (Higgins et al. does not include all and makes a number of strong simplifying assumptions). However, such a comparison would be a non-trivial project requiring significant additional resources, and we ask not to undertake it (also see response to comments above). We do, however, now recommend it as future work and updated the summary and discussion section accordingly.

Lines 638-640: "Specifically, we recommend comparisons between the findings of simulations based on coupled ocean-wave model output and simulations using the advanced approximation by Higgins et al. (2020), to identify which of the wave-driven processes are the most important to include for faithful simulation of particle dispersal."

Table 2: I find this table quite useful interpreting the definitions, it would be useful to refer to this table when defining the u_Enw and u_Ew components.

We are pleased that you find the table useful. However, we prefer to keep it in the method section, as the u_Enw and u_Ew components are sometimes also derived differently, for example by assuming that their interactions are negligible and the total wave-driven currents can be approximated by a few distinct processes (as done in Higgins et al., 2020). To still account for the very valid point you raised, we added more specific information to the introduction, as outlined in the response to your first general comment.

L333: This is a practical approach, but I think this is an important point to make earlier (e.g., when discussing the decomposition). It is important to know that it is defined as a residual and includes all the feedback that would be missed in the intermediate approach.

Based also on your first general comment and the previous specific comment, we included the suggested information already when introducing the decomposition in section 1.

Figure 4: I find the bar plot (panel a) busy and difficult to understand. You may consider if the maps are sufficient on their own to make less effort for a reader to understand the figure.

We understand that the bar plot is perceived as (too) busy. We moved the plot into a supplementary, as it is indeed not necessary for understanding the main storyline, but crucial for the classification of the sub-regions in neutral, winter, and summer types depicted in Figure 5.

New Suppplementary Figure S1.

Figure 6: The differences between the panels are often subtle. I wonder if showing the difference from the 1st experiment instead of the value for the 2nd and 3rd experiment would make a clearer indication of the changes?

We explored the suggested alternative visualization – and (in contrast to what we wrote in our first response to the comments), finally decided to indeed adjust Figure 6 (as well as Figure 8 and 10) and place the original Figure 6 and additional difference plots in the new supplementary as Figures S2 and S3 (as well as Figure S4 – S7). This then also required some minor changes in the text of the respective paragraphs.

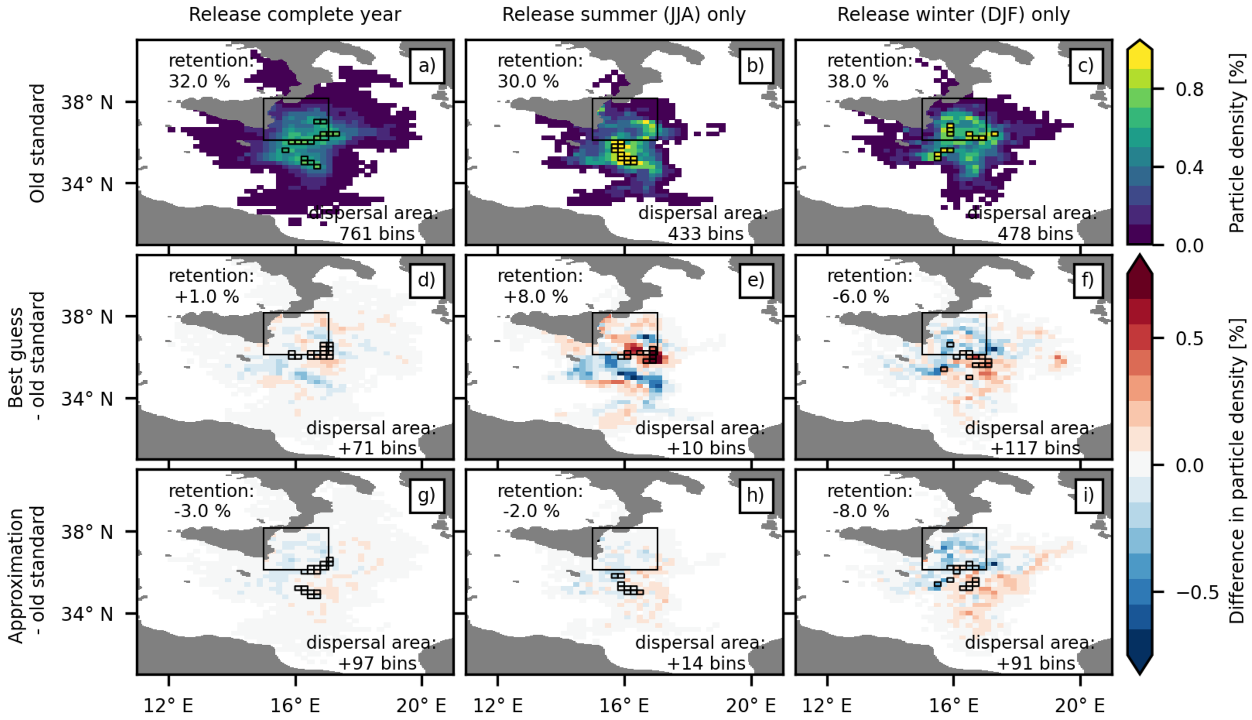

Figure 6: Impact of surface waves on simulated dispersal pattern of particles released in the Ionian Sea (release 1, neutral type region) 30 days after their release. (a)-(c) Particle density per 0.2° x 0.2° bin at the end of the integration period of 30 days (color shading) for the old standard simulation; the bins with the highest particle density, encompassing in total 10 % of the particles, are marked by black borders. Change in the particle density of the **(d)-(f)** best guess and **(g)-(i)** approximation compared to the old standard simulation; highest particle density bins of the best guess and approximation are marked by black borders. Values of the (change in) retention rate (percentage of particles that remain within or have returned to the region's release area, indicated by the large black frame, until the end of the integration period) and the overall dispersal area (total number of bins occupied with particles) are printed.

Line 446: "In the best guess simulation (Fig. S2d-f, […]"
Line 450: "[…] the same holds for changes in the particle density distributions (Fig. 6d-f and Fig. S3a-i)"
Lines 462-463: "Likewise, the total impact of waves on the particle density pattern is well captured by the approximation in winter but not at all in summer (Fig. 6g-i)."

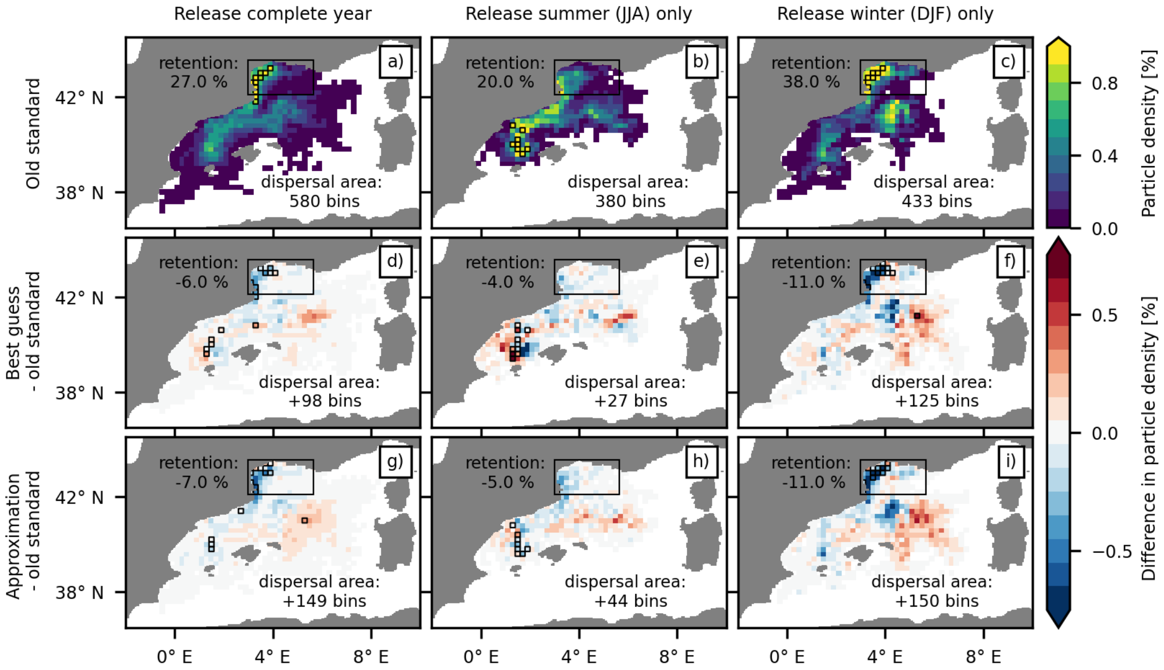

Figure 8: Impact of surface waves on simulated dispersal pattern of particles released in the Gulf of Lion (release 2, winter type region) 30 days after their release. (a)-(c) Particle density per 0.2° x 0.2° bin at the end of the integration period of 30 days (color shading) for the old standard simulation; the bins with the highest particle density, encompassing in total 10 % of the particles, are marked by black borders. Change in the particle density of the (d)-(f) best guess and (g)-(i) approximation compared to the old standard simulation; highest particle density bins of the best guess and approximation are marked by black borders. Values of the (change in) retention rate (percentage of particles that remain within or have returned to the region's release area, indicated by the large black frame, until the end of the integration period) and the overall dispersal area (total number of bins occupied with particles) are printed.

Line 510: "In the best guess simulation (Fig. S4d-f, black bars in Fig. 9, Table 3) […]"
Lines 517-518: […] the total impact of waves on the particle density pattern is in general well captured by the approximation (compare Fig. 8g-i with Fig. 8d-f)
Lines 529-530: "Wave-driven Eulerian currents appear to impact important details in the particle density pattern in summer (Figure S5b,h)."
Line 533: "[…] impact of Stokes drift and wave-driven Eulerian currents together interrupts the connection between the Gulf of Lion and Mallorca, while connecting pathways still exist if only Stokes drift alone is added (Fig. S4e,h)."

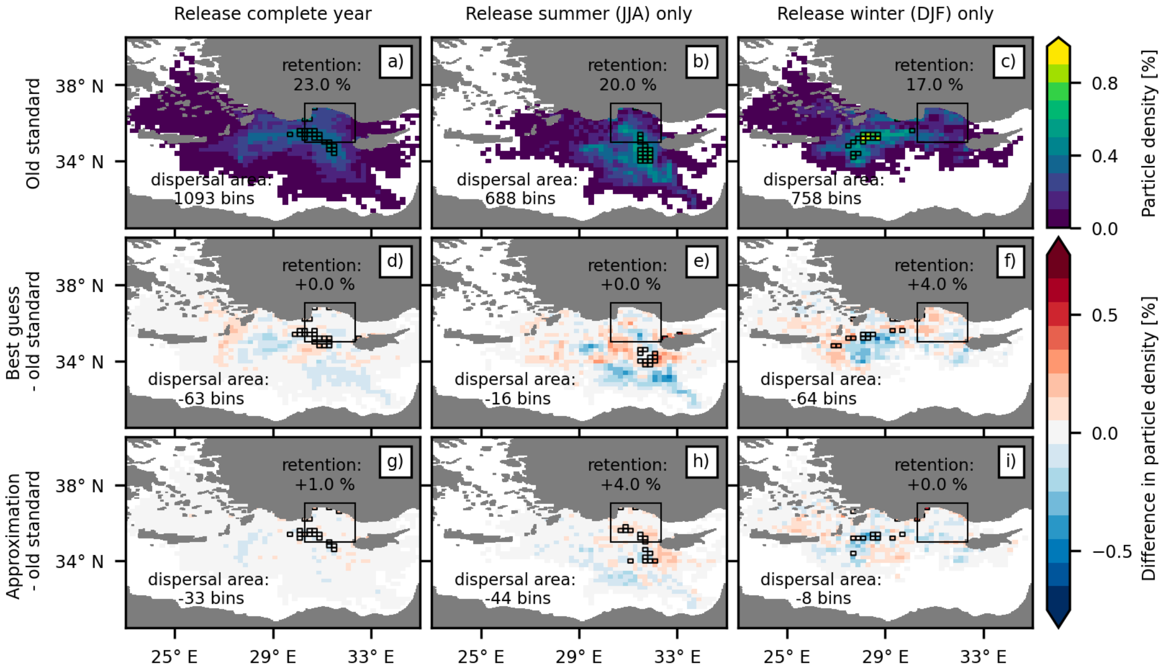

Figure 10: Impact of surface waves on simulated dispersal pattern of particles released in the Gulf of Antalya (release 3, summer type region) 30 days after their release. (a)-(c) Particle density per 0.2° x 0.2° bin at the end of the integration period of 30 days (color shading) for the old standard simulation; the bins with the highest particle density, encompassing in total 10 % of the particles, are marked by black borders. Change in the particle density of the (d)-(f) best guess and (g)-(i) approximation compared to the old standard simulation; highest particle density bins of the best guess and approximation are marked by black borders. Values of the (change in) retention rate (percentage of particles that remain within or have returned to the region's release area, indicated by the large black frame, until the end of the integration period) and the overall dispersal area (total number of bins occupied with particles) are printed.

Line 563: "In the best guess simulation (Fig. S6d-f […])"

Lines 575-578: "As expected for a summer type region, all net changes (red lines in Fig. 11, Fig. 10d-f) between the best guess and old standard simulations are dominated by changes in Eulerian currents (yellow lines in Fig. 11, Fig. S7a-c), Stokes drift (blue lines in Fig. 11, Fig. S7d-f) has a small net effect. Nevertheless, using the basic approximation (cyan line in Fig. 11, Fig. S6g-i), yields a small improvement over employing the old standard for several of the calculated dispersion measures (Table 3)."

L609: Suggest to clarify what is meant by intrinsic variability in this context. It is aneddying model, so I did wonder if 2 years is sufficient experiment length for all the statistics?
We removed the reference to intrinsic variability, as it is not very fitting at this point.

L625: This may be true and is a worthwhile point, but I don't know that this has really been tested in this work. This study shows differences in the Eulerian (gridded mean) fields between coupled and non-coupled, which weren't found in Wagner et al. (2022). As mentioned in the general comments there are other differences between these works, particularly the horizontal grid-spacing, which could explain the different conclusions.

We are of the opinion that our combined analysis of gridded-mean speed and diverse Lagrangian dispersal measures indeed suggests that analyzing Lagrangian velocities in an Eulerian framework is insufficient for estimating the impact of certain flow components such as Stokes drift and wave-driven Eulerian currents on large-scale particle dispersal patterns, as the impact of different wave processes varies spatially and temporarily in the gridded fields and moreover differs between the different dispersal measures. That implies that the impact of waves on Lagrangian dispersal cannot easily be predicted based on the impact on (averaged) Eulerian gridded fields.
But as mentioned in our reply above, we included a comment on other differences such as the spatial resolution in an updated version of the manuscript.

Final remark

In addition to the changes made in response to the reviewer's comments, we added a line to thank the reviewers to the acknowledgments, and a link to a GitHub repository that contains all code necessary to reproduce the results and figures of the manuscript in the code and data availability section. Moreover we corrected a typo in Table 3 and a slightly misleading formulation of relative percentages changes in lines 455, 513, and 572.

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