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”

Siren Rühs[1], Ton van den Bremer[2], Emanuela Clementi[3], Michael C. Denes[1], Aimie Moulin[3], and Erik van Sebille[1]

[1]Institute for Marine and Atmospheric research Utrecht, Utrecht University, 3584 CS Utrecht, The Netherlands
[2]Faculty of Civil Engineering and Geosciences, Delft University of Technology, 2628 CD Delft, The Netherlands
[3]CMCC Foundation - Euro-Mediterranean Center on Climate Change, 73100 Lecce, Italy

Format: original comments in black, response in blue

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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 will make sure to highlight our pragmatic choice already in the introduction by adding the following sentences in a revised version of the manuscript: "[…] 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 a revised version of the manuscript, we will add a comment to the discussion section mentioning that the difference in 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 will use a better formulation of the respective sentence in a revised version of the manuscript: "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 appreciate the suggestion and will replace Craig and Banner (1994) by McWilliams et al. (1997).

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 will replace "help solving the long-standing controversy around" by "yield further insights on".

L166: WaveWatch -> WAVEWATCH.

Thanks for spotting the typo. We will correct it.

L169: despite -> except

We will change the wording as suggested.

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 will make sure to include 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 will be added.

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 will rework the paragraph describing the TKE scheme to clarify/include the aspects you raised.

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 will adjust the sentence as suggested.

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.

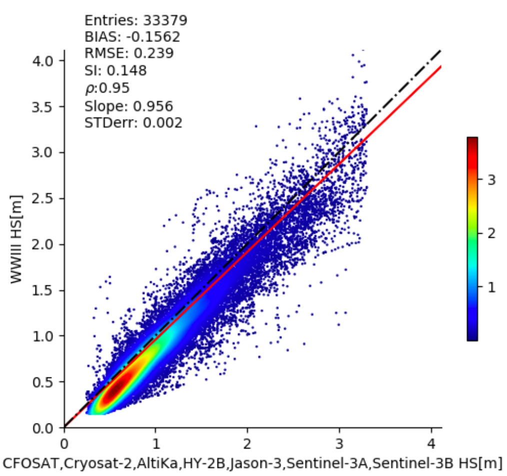

Figure 1: Scatter plot of significant wave height (H_S) [m] providing a comparison between satellite observations and numerical simulations for the period 2019 to 2020. Dot colors refer to the data probability density; black dashed line represents the best-fit (1:1); solid red line shows the satellite-model 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 will rework 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 will include a clarifying sentence in the revised manuscript.

L266: Suggest to clarify if Stokes drift is similarly averaged over 1m.

Thanks for spotting this inaccuracy. Will be changed to "[…] 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 will add the information that Stokes drift is obtained 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 will update the summary and discussion section accordingly.

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 will add 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 will include 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 will move 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.

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 of the results in Figure 6 (as well as Figure 8 and 10) but found the original version of the Figures to overall better support the messages in the text. However, as individual aspects are indeed clearer observed in the difference plots, we will include respective Figures in the supplementary.

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 will remove 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 to the other Wagner et al. (2022) related comments, we will comment on other differences such as the spatial resolution in an updated version of the manuscript.

References

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