Summary

Bertoncelj et al. introduce SCARIBOS, a regional configuration of the CROCO ocean model for the South CARIBbean Ocean System with a kilometre-scale horizontal resolution of 1/100 degree. The authors use four years (2020-2024) of surface velocity fields output by the model to undertake three Lagrangian particle tracking experiments using the OceanParcels framework to explore the surface connectivity of flows surrounding the island of Curaçao. The Lagrangian experiments allow the authors to investigate (a) potential hotspots of marine pollutants around Curaçao, (b) intra-island coastal connectivity, and (c) the connectivity between the Curaçao coast and the neighbouring Aruba, Bonaire, Venezuelan islands, and the Venezuelan mainland. The authors should be commended for the Figures, especially those relating to the Lagrangian analysis, which convey the central findings both clearly and creatively. The manuscript is generally well written, although a more comprehensive description of the numerical modelling approach (including model validation), further exploration of the role of mesoscale eddies in driving surface connectivity, and an improved discussion on the limitations and wider relevance of the findings is needed. I would recommend the manuscript for publication subject to major revisions addressing the comments made below and the excellent suggestions made by Reviewer #1.

We thank the reviewer for their detailed and very useful comments and suggestions. Below, we respond to each comment in detail and address them accordingly. Line numbers mentioned in the text correspond to line numbers in the revised manuscript.

General Comments

- ***The current title:*** 'Flow patterns, hotspots and connectivity of land-derived substances at the sea surface of Curaçao in the Southern Caribbean' feels like a description of what the author's set-out to investigate in this study but does not give any indication of the main findings or conclusions drawn from the analysis. Although the title is perfectly acceptable in its current form, I wonder whether it could improved to highlight the findings most relevant for stakeholders / policymakers.
  Many thanks for the suggestion. We have decided to stick with the current title.
- ***Model description and validation:*** Given that this is the first documentation of the SCARIBOS simulation and it is likely this will be used by other studies in the future, further details are needed to improve the model description, including highlighting the absence of Stokes Drift, stating whether a current feedback parameterisation is implemented, commenting on key parameterisation choices of ocean physics (e.g. in the surface mixed layer), and including the frequency at which velocity and tracer fields are output (this will be especially relevant for future studies). Moreover, as highlighted by Reviewer #1, the validation of the simulation is currently insufficient to robustly conclude that it accurately represents the circulation of the Caribbean Sea (see specific comments below).

Moving forward, I would suggest broadening the validation beyond Curaçao to the wider Caribbean Sea domain and to use several sources of observations, including surface drifters deployed in the region during the simulated period (2020-2024).

Thank you for pointing that out. We agree that more detailed description of the model is valuable for the future users, and we have added the information accordingly to lines 153-165. Consequently, we also added 4 new citations: Godfrey and Beljaars, 1991 (lines 646-647), Fairall et al., 2003 (lines 642-644), Jackett and McDougall, 1995 (lines 659-660) and Large et al., 1994 (lines 669-670).

Following the suggestion also by Reviewer #1 we have included additional validation by graphically comparing GlobCurrent (Copernicus product) and SCARIBOS for the entire domain for the monthly averages for year 2022. We plotted the comparison in Figure 2. Accordingly, we added lines in 2.3 explaining the methodology (lines 169 to 172) and assessing the performance of the model (lines 176 to 190). Consequently, we changed the figure numbering of the entire revised manuscript to match the changes.

Unfortunately, during our simulation period 2020-2024 almost no drifters from the Global Drifter Program were passing our model domain (see figure below).

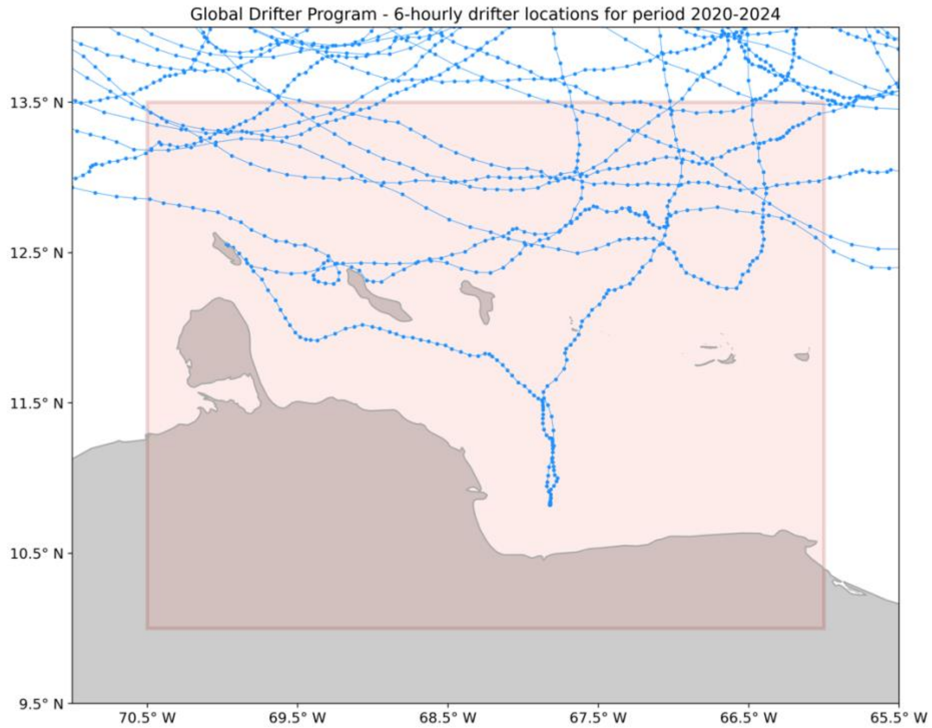


Figure: *Drifter trajectories that passed our model domain (red box) in period 2020-2024. Data retrieved from Lumpkin and Centurioni (2019).*

Reference:

Lumpkin, R., and Centurioni, L.: Global Drifter Program quality-controlled 6-hour interpolated data from ocean surface drifting buoys, NOAA National Centers for Environmental Information, Dataset, https://doi.org/10.25921/7ntx-z961, [accessed: 17 January 2025], 2019.

- ***Extracting maximum value from the 1/100 degree SCARIBOS simulation***: the Lagrangian analyses presented in the study are all well motivated and provide new insights into surface connectivity in the region. However, on reaching the conclusions of the manuscript, I did not feel that the full potential of this stateof-the-science simulation had been extracted. Firstly, it would be valuable to know if the insights presented are critically dependent on resolving flow structures at 1/100 degree or could similar conclusions be drawn using a much lower resolution. One powerful way to illustrate this would be to spatially coarsen the surface velocity field and repeat one or more of the Scenarios to examine how this impacts marine connectivity. A natural caveat here would be that the coarsened velocity field still originates from one which explicitly represented (sub)mesoscale dynamics, however, identifying the critical threshold of horizontal resolution required to represent connectivity in this region would be an incredibly valuable contribution of this work and would extend beyond the present application to Curaçao.
  Thank you very much for this insightful comment and valuable suggestion. While conducting a sensitivity analysis with different spatial resolutions would indeed provide interesting and important insights, we believe this is beyond the scope of this manuscript. Our choice of a 1/100° resolution was specifically motivated by the need to resolve the shape of Curaçao and its surrounding features accurately. Previous models with coarser resolutions, such as 1/30° (model AmSeas, finest available at the moment), represented Curaçao poorly, with the island's shape incomplete and Klein Curaçao entirely absent (see figure below).

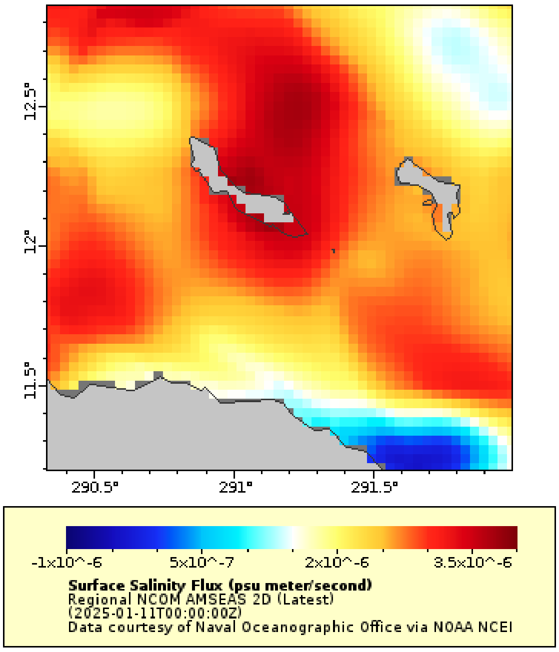


Figure: Model AmSeas with 1/30° spatial resolution (example screenshot). Figure found on https://www.ncei.noaa.gov/erddap/griddap/NCOM_amseas_latest2d.graph?surf_el%5B(2025-01-19T00:00:00Z)%5D%5B(10.066160202026367):(13.399160385131836)%5D%5B(291.16375732421875):(294.49676513671875)%5D&.draw=surface&.vars=longitude%7Clatitude%7Csurf_el&.colorBar=%7C%7C%7C%7C%7C&.bgColor=0xffccccff&.click=?32,134 (last accessible on 19 January 2025).

For the purposes of this study, accurately resolving the island's shape was essential, as the zones in Scenario 2 are relatively small, and the Lagrangian analysis of coastal connectivity requires precision.

Similarly, I think the authors could better exploit the eddy-resolving nature of the simulation to further investigate the role of mesoscale eddies beyond the primarily qualitative descriptions included currently. A valuable example of such an approach is Roach and Speer (2019) - https://doi.org/10.1029/2018JC014845 - which identified the timescales of variability in the flow field which are responsible for the connectivity between the Ross Gyre and the Antarctic Circumpolar Current by coarsening their 5-day mean velocity field to 90-day means and a single time-mean field. This allowed them to separate the connectivity associated with high-frequency (e.g., mesoscale eddies) and lowfrequency (e.g., seasonal variability) variability from the time-mean flow. Given SCARIBOS explicitly resolves the (sub)mesoscale, using such a time-coarsening approach in this study could provide new insights into the role of high-frequency flow features in establishing the surface connectivity around Curaçao. This may also o[er further retrospective justification for the use of a

1/100 simulation and underscore the value of such regional models for informing policy making and marine planning.

Thank you for a very interesting suggestion. Although we agree that this approach would definitely offer a new insight into the dynamics of the eddies, we believe that this is a very large additional step and is beyond the scope of the current manuscript.

Specific Comments Abstract

Lines 11-12: Suggest replacing ‘, as these substances…’ with ‘since these substances can be transported towards reef sites by ocean currents’ given that its already implied that the substance has entered the ocean.

Corrected.

Lines 13-16: Suggest combining the two sentences beginning with ‘SCARIBOS, a fineresolution…’ and ‘SCARIBOS covers the…’ to reduce repetition. Then ‘Furthermore,’ can be dropped in the following sentence as it is not needed.

Corrected.

Lines 19-21: Suggest condensing these two sentences to be less ambiguous. For example: ‘Our results reveal two dominant processes influencing the hotspot locations of positively buoyant substances…’

Thank you for pointing that out. We have changed the second sentence to (lines 20-21):

These flow patterns influence hotspot locations, with higher accumulation of positively buoyant substances occurring during eddy events.

Line 24: As a non-domain expert on marine pollutants, I was surprised to see justification of this work as providing valuable information for marine conservation and environmental management at the end of the Abstract. The need for kilometre-scale modelling of ocean connectivity to inform stakeholders’ decision-making struck me as an important motivation for pursuing the study alongside the current ‘problem statement’ highlighting the general decline of coral reef communities.

Thank you for your comment. In this study, we aim to address a specific issue contributing to the decline of coral reef health—one that can be directly addressed through informed policy changes. While our modelling employs still a relatively large scale (1km resolution), our analysis demonstrates that the results have a potential to inform policymakers. For instance, in the coastal connectivity analysis, certain areas are identified as more likely to experience particle accumulation, highlighting zones of potential concern and intervention.

Introduction

Line 28: Suggest being more specific on the rates of coral reef decline, by how much has this changed already? Is this accelerating? This would further strengthen the motivation for the study.

Thank you for this suggestion. We added the information about the rate of decline, but unfortunately, there is no papers yet published on its acceleration. Here is the revised statement (lines 27-29):

However, previous studies have reported a significant decline in coral cover, with more than 50% of living corals on shallow reefs lost between the 1970s and the early 2010s (e.g., Bak et al., 2005; Vermeij et al., 2011; Waitt Institute, 2017).

Lines 30-32: Suggest combining these two sentences: 'are susceptible to accumulating pollutants, bacteria and viruses originating from urban areas, …'

Corrected (lines 30-33).

Lines 34-35: As a non-domain expert, it would be interesting to know how the threat posed by the accumulation of marine pollutants and harmful biological substances around coral reefs compares to other threats, such as marine heat waves and ocean acidification. Why is this threat especially worth investigating?

Marine pollution is indeed one of the main threats to coral reefs, as highlighted in report Reefs at Risk Revisited by Burke et al. (2011). Unlike ocean acidification and thermal stress, which are global issues and more challenging to address directly, the accumulation of marine pollutants and harmful biological substances is a threat that can be mitigated relatively easily through targeted policies and improved local management. This makes it particularly worth investigating. However, we acknowledge that it is not the only threat to coral reefs.

Reference: Burke, L., Reytar, K., Spalding, M., Perry, A.: Reefs at Risk Revisited. Washington (USA): World Resources Institute. 130 pp. URL: https://www.wri.org/research/reefs-risk-revisited. 2011.

Lines 36-46: Suggest revising or restructuring this paragraph as it's currently difficult to follow: it begins with a recognition that remote sources of marine pollutants are important for coral reefs, then proceeds to discuss why existing numerical model simulations are insufficient to represent the local ocean dynamics of the Caribbean, and concludes that developing a SCARIBOS model is the answer to this challenge. I wondered whether an alternative framing of this paragraph could be to highlight that in almost all ocean general circulation models, most notably the CMIP6 ensemble, Caribbean small island states are (at best) represented by a single grid cell or not at all, hence we are currently not in a position to translate global nor regional scale insights to local communities (who may be significantly impacted by unresolved processes). Thus, developing fine-resolution regional configurations like SCARIBOS provides a means to represent these regions more accurately and inform marine conservation and environmental management efforts.

Thank you for your thoughtful suggestion. While we understand your recommendation to reframe the paragraph, we would like to keep it as it is. We have structured the paragraph to first highlight the issue that pollution travels with ocean currents, thereby necessitating the use of regional circulation models. However, we also point out that no model with a fine enough resolution currently exists, and we are addressing this limitation with our work.

Lines 44-47: Suggest refining this conclusion to be more precise. The preceding text highlights that there is only limited high-resolution ocean model data for the Caribbean Sea, but why does it follow that we need a high-resolution model of Curaçao specifically. Is the coral reef environment here particular at risk or subject to emerging risks? Or are the ocean dynamics here representative of the wider region, such that the insights drawn from this study are applicable elsewhere?

Thank you for your feedback. While Curaçao is not uniquely at risk compared to other (neighbouring) islands, our study focuses on Curaçao as a case study due to the scope and objectives of our project. Although Curaçao is project-specific, we believe that our methodology can still be adopted to other regions (and SCARIBOS can potentially be used for other few islands in the domain too).

Line 49: Suggest modifying to 'our research investigates **the** dominant surface ocean current patterns and substance transport **pathways** around Curaçao…'

Thank you. We corrected the sentence based on your suggestion.

Line 50: Do you mean monthly to inter-annual **variability** rather than environmental changes? I found this to be ambiguous as ocean variability is discussed later in the text.

You are correct – it is variability. We have changed it accordingly.

Line 51: Suggest using vertical **extent** here and elsewhere rather than reach in this context.

Corrected.

Lines 59-67: Not entirely sure this paragraph summarising the methodology is necessary on reading the full text since the description of the model and methods directly follows the introduction. As a compromise, it could be condensed to focus on the development of SCARIBOS as an answer to the research question which is nicely presented in the previous paragraph.

Thank you for your suggestion. We agree and we have altered the paragraph to skip the unnecessary information about the model validation (lines 67-72).

Methods

Figure 1: Suggest here and elsewhere ensuring that the colourbar ticks and contour levels are aligned to make it clear that the sea floor in the regions shown in white, for example, lies between 0-500 m depth etc.

Corrected.

Line 92: More appropriate, given the geographical location of this study, to refer to the Atlantic Meridional Overturning Circulation since you are referring to the warm upper limb waters feeding the Florida Straits.

Agreed. We added 'Atlantic' (line 97).

Lines 120-121: Is this really a sufficient model spin-up time to ensure that the Lagrangian experiments are not capturing the ongoing adjustment of the mean flow and eddy kinetic energy fields? How was this determined? Further details should be included here.

A spin-up duration of 4 months was selected based on the assumption that the initial and boundary conditions provided by the GLORYS Copernicus model product enable the system to rapidly achieve a quasi-equilibrium state. The model uses a *'hot start'*, initializing with velocities (and salinity and temperature) interpolated from the GLORYS Copernicus model. Additionally, we analysed the time series of the average eddy kinetic energy (EKE) across the entire domain and observed that the energy is stabilized quickly. The figure below shows the time series for the first 2 years, starting from the initial month of December 2019. This justification has been added to the manuscript on lines 127–129 and 145-146.

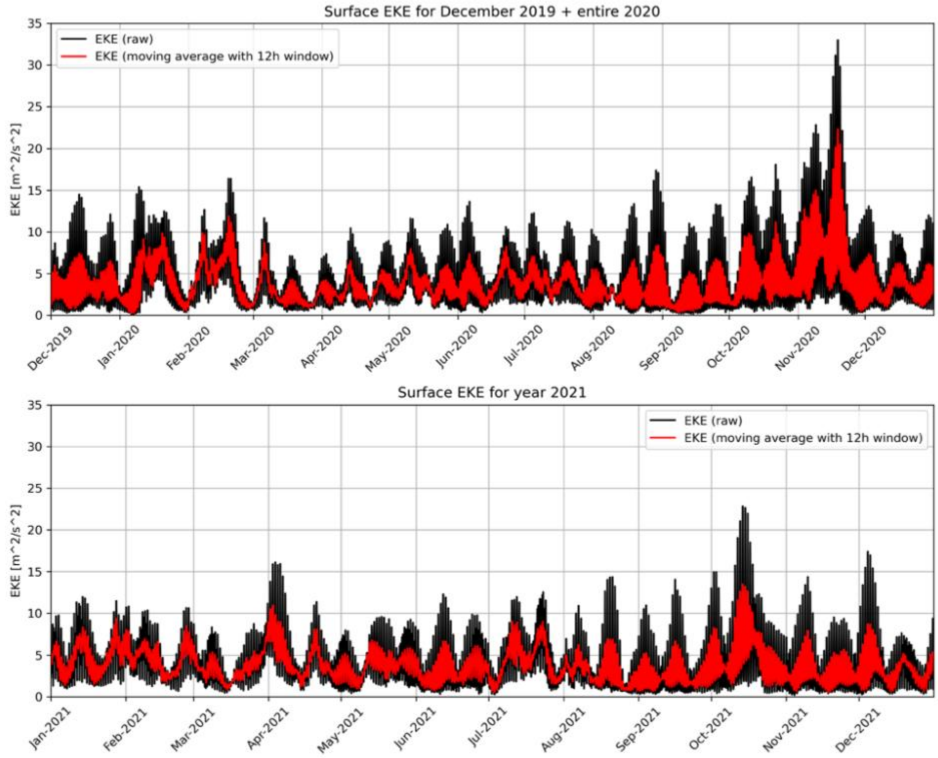


Figure: Time series of Eddy Kinetic Energy (EKE) averaged over entire domain (black line), along with the EKE smoothed by a 12-hour moving average (red line). Time series spans from December 2019 to January 2022.

Lines 122-125: What is the spatial resolution of the bathymetry product linearly interpolated from? What manual adjustments were made to the bathymetry? Further details on how this was undertaken and to what extent this (presumably) improved the resulting flow structures around the island should be included.

Thank you for this comment. We added the spatial resolution of both bathymetry products and we further explained the smoothing of bathymetry and why we made manual adjustments (lines 136-141):

Smoothing of the bathymetry was performed using the CROCO TOOLS product (V1.3.1) to mitigate steep slopes that could cause instabilities in the model. …[]… These adjustments are necessary to correct inconsistencies between the bathymetry-derived land-sea mask and the true coastline, ensuring more accurate representation of coastal features that significantly impact the formation and propagation of eddies around the islands.

Lines 129-131: Is the interpolation scheme used to downscale the GLORYS12V1 velocity and tracer fields bilinear or conservative (conservative-normed)?

The interpolation method used for GLORYS12V1 velocity and tracer fields is spline interpolation, which was a default option in CROCO TOOLS configuration.

Line 132: When applying the surface atmospheric forcing does SCARIBOS account for the current feedback to the atmosphere (CFB), which contributes to the oceanic circulation by damping mesoscale eddies. (i.e., Does CROCCO account for the fact that the ERA5 surface wind stress field acting on the ocean has already 'felt' the surface ocean currents and hence simulations forced without CFB overestimate the mean circulation and the mesoscale activity). An excellent discussion on the use of reanalysis winds to force ocean models in provided in Section 6 of Renault et al., (2020) - https://doi.org/10.1029/2019MS001715.

Current feedback to the atmosphere (CFB) is not taken into account in SCARIBOS. We acknowledge that this leads to overestimation of mean circulation and mesoscale activity. We have added this information to line 160.

Lines 134-135: At what frequency are the 2-D (surface) and 3-D velocity and tracer fields output from the SCARBIOS model? Are daily mean fields being used as the inputs to OceanParcels? This is currently unclear and should be added to both the model description and the description of the Lagrangian experiments undertaken in this study.

Velocity and tracer fields are outputted hourly and the same frequency (1h) is used as input in all Parcels simulations. We have added lines in the new manuscript accordingly:

Lines 129-130: *Model outputs include hourly averages of horizontal and vertical velocities, temperature and salinity, stored for every grid cell in the domain …*

Lines 246-248: *Particle tracking simulations using the Parcels v3.0.3 framework (Delandmeter and Van Sebille, 2019) are conducted to model the movement of passive particles, representing nutrients and pollutants, using hourly velocity fields from the uppermost layer of the SCARIBOS model.*

Lines 189-193: Unfortunately, I do not think that sufficient validation has been undertaken to justify the conclusion that 'SCARIBOS accurately simulates surface-level dynamics'. In this section, the model has been shown to reproduce the sea level timeseries at a single location and time-average currents agree qualitatively with limited observations in magnitude and direction. Reviewer #1 has made a number of excellent suggestions on ways to improve this validation, which I will not repeat here. I strongly support the use of surface drifters to validate the surface flow field. More broadly, I also think that the authors should comment (either in the methods or discussion section) on the somewhat philosophical challenge of undertaking simulations at kilometre-scale resolution in regions where observations are sparse – how do we know what good looks like? This also relates to my more general comment; I think it would be valuable for the authors to consider what is the minimum horizontal resolution needed to investigate connectivity in the Caribbean Sea (see general comments above).

Thank you very much for these suggestions. As already stated above, we decided to make additional validation with the use of GlobCurrent Copernicus product (see Fig. 2 as the new figure in the manuscript and above for the details on new lines explaining the methodology and assessment).

Regarding the suggestion to comment on the challenges of conducting simulations at 1 km resolution in regions with sparse observations, we believe this is already understood by the community. The limitations of such models, particularly in under-observed areas, are common across similar studies. SCARIBOS represents the finest resolution currently available for this region, and like any model, it must be interpreted with caution. As such, we do not believe that further elaboration on this point is necessary.

Line 197: Suggest acknowledging here that the velocity fields do not include Stokes Drift rather than leaving this until the Discussion.

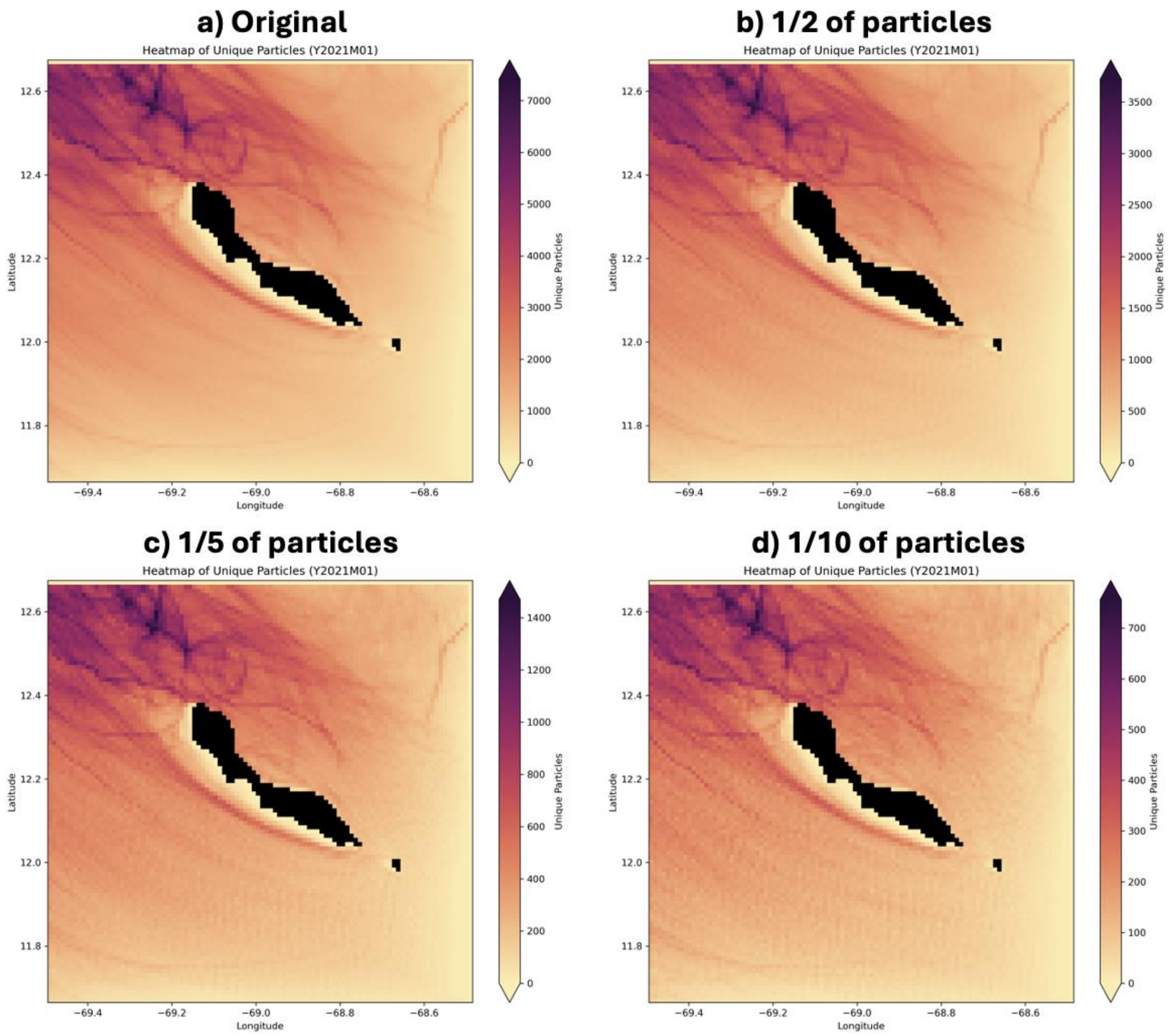
Agreed. In line 248 we have added the following sentence:

These velocity fields do not include Stokes Drift.

Lines 204-205: To be clear, does this equate to releasing a single particle in each 1/100 grid cell? How sensitive are the results of Scenario 1 to these initial conditions given that (sub)mesoscale turbulence is explicitly resolved in this simulation? Conceivably, given how chaotic the underlying velocity field is, a small difference in the initial position of a particle could result in a very different final position following 30 days of advection. An insightful discussion of the chaotic behaviour of Lagrangian trajectories is presented (albeit applied to ocean ventilation in a much coarser OGCM) in MacGilchrist et al. (2017) - https://doi.org/10.1002/2017JC012875.

You are correct, there is one particle per grid cell. Moreover, following the suggestion below we decided to re-run Scenario 1 in order to release the particles every 12 hours (to be consistent with the other two scenarios).

You are right to be concerned about the sensitivity of the results to the particle's initial position. To check this, we conducted a sensitivity analysis to assess the impact of reducing the number of particles released. Specifically, we examined the normalized unique particle counts (see below for more information) when selecting only 1/2, 1/5, and 1/10 of the original particles (still within the 1x1-degree square). As shown in the figure below (with example month January 2021), the results are remarkably consistent across all cases. This demonstrates the robustness of our analysis. Interestingly, the pathways with higher normalized particle counts remain prominent, even with only 1/10 of the original particles. We added an interesting observation from our results in the discussion – linking our results with well-known Lagrangian Coherent (LCS) in a new paragraph in section 4.2 (lines 466-471).



<u>Figure:</u> Normalized unique particle count for January 2021 under varying particle release scenarios: (a) using the original number of particles, (b) using 1/2 of the original particles, (c) using 1/5 of the original particles, and (d) using 1/10 of the original particles.

Lines 205-206: The statement: 'The internal particle simulation timestep is set to 5 min and trajectories are archived every hour' is repeated for all three experiments. Suggest outlining the common features of the three scenarios in a final paragraph to reduce repetition.

Thank you for your suggestion. We made changes accordingly.

Lines 209-210: Did you also consider calculating Lagrangian PDFs by counting the number of unique entries into each given grid cell normalised by the total number of particles (i.e, calculating the likelihood that any given particle will enter a grid cell at least once during its lifetime)? In my experience, this can improve the clarity of Lagrangian PDF plots where recirculation features are dominant and better illustrate the net flow pathways.

Thank you for this very insightful suggestion. We decided to follow your advice and instead of PDFs we calculated the unique particle counts normalized by the total number of particles. We re-wrote the methodology, results and discussion based on these findings:

Methodology: changed lines 259 to 266, explaining how the normalized particle count was calculated.

Results: we re-wrote most of the section 3.2, due to changes in the findings derived from the new figure (Fig. 8).

Main changes are because now we do not have PDF of particle concentration, and therefore the narrow band of the highest PDF in the northern coastline is not present anymore in the results. Previously this was the case because the particles are moving very slowly along this coastline (which resulted in very high PDF), but since we are now only counting the number of unique particles visiting each bin, this narrow stripe is not visible anymore. We now focus on the difference between the northern and southern coastline (Fig. 8a) and monthly variations (this part did not change much). Changed lines 347-370 (most of section 3.2 is largely altered).

Discussion: we changed from PDF to hotspots and from particle concentrations to particle counts in sections 4.2 and 4.3. Main conclusions stayed the same.

Lines 215-216: Why are particles released every 12 hours in Scenario 2 compared with every 24 hours in Scenario 1? Is the initial time of release important relative to the diurnal cycle of atmospheric forcing, what time each day does this take place? It would be helpful for the authors to comment more on these uncertainties.

As mentioned earlier, we adjusted the release interval to 12 hours for Scenario 2 to maintain consistency across all scenarios. While atmospheric forcing might introduce some variability in our region, using a 12-hour interval helps to mitigate any potential bias related to diurnal atmospheric cycles. Moreover, in this region tidal forces are relatively weak, so we believe the precise time of release does not significantly influence the results. If tidal forces were stronger and particles were released at specific tidal phases, this could introduce a bias in the initial deflection of particles.

Lines 225-226: Why are particle released two coastal grid cells away? Presumably, beaching of particles could be a problem when using a numerical time-stepping scheme to determine the trajectories?

Particles are released within the first and second coastal grid cells away from land, forming a ribbon-like area encompassing these two grid cells. We chose to release particles in two grid cells rather than one to better represent the wider area from which they originate. In the OceanParcels code, particles driven by the hydrodynamic model, which is based on a C-grid (in our case CROCO is based on a C-grid), are not expected to beach (i.e., they do not get stuck on land grid cells). Therefore, this issue does not affect our results.

Lines 236-237: Suggest adding a brief description of the locations of the remaining 1% of particles in Scenario 3 which do not leave the domain. Are these locations consistent between particle releases? If so, would regions of high particle persistence be particularly concerning for marine pollution and environmental management?

We appreciate your suggestion and have added the requested information in the results section 3.4, lines 415-418:
Moreover, since our simulations were designed to terminate early if the computations take too long and only 1% of particles remained in the domain, this primarily occurred in the simulations of the Venezuelan mainland, due to the large number of particles released and the low currents near the mainland. Most of the remaining particles are located near the Venezuelan mainland, particularly along the east coast of the Paraguaná Peninsula.

Figure 4: Excellent Figure, the authors have done a great job of visualising the differences between the Scenarios. Suggested modification to the final line of the Figure caption: 'The destination area highlighted around Curaçao represents the region within which particles are tagged as reaching the Curaçao coast.'

Thank you very much for the compliment and for your suggestion! We have changed the caption accordingly (lines 295-296).

Results

Lines 250-257: In Figure 5b, interannual variability appears to dominate over seasonality, so I would suggest caution not to overinterpret monthly behaviour based on four years of surface velocity data. Caveating the discussion by highlighting the limited number of months available to sample (4 instances each) would be one approach.

We added 'inter-annual' variability in this paragraph (line 305) to avoid being too biased with only mentioning 'seasonal'. We do acknowledge the limiting 4 years of simulations at the very beginning of the discussion (section 4.1, lines 429-430):

While definitive conclusions are limited by the four-year simulation period, the observed seasonal to inter-annual patterns, even during El Niño year (2023), provide strong evidence for the robustness of these processes.

Lines 263-266: Why is the analysis restricted to a single meridional cross section? A comparison of the 2-dimensional (longitude-latitude) flow field at various depths would properly account for the spatial dependence of the flow and to make the conclusion that the surface velocity field is representative of the upper ocean flow field more robust.

We chose to focus our analysis on a single meridional cross-section because this specific cross-section provides significant insight into the dynamics of the system. Our analysis concentrates on the zonal flow at this passage, as surface currents clearly indicate that the zonal flow is strong in the region between Venezuela and Curaçao (and definitely much stronger than the meridional flow). While we agree that analysing multiple cross-sections or a 2-dimensional flow field would provide a more detailed spatial perspective, we believe that the data from this single cross-section adequately captures the thickness and behaviour of the surface flow in this region.

Lines 270-272: Are the T-S properties of the westward current consistent with AAIW at these latitudes? This would strongly support the inference.

Following the comment from Reviewer #2 we decided remove mentioning of the AAIW in our manuscript.

Lines 273-280: This paragraph is quite confusing. The opening sentences largely repeat the findings above and third sentence seems to preface the say that the flow field is highly variable. I would also recommend removing 'observed' on Line 277 since (I think) you are still referring to the output of the SCARIBOS model here?

Thank you for pointing this out. We re-wrote this paragraph as (lines 327-331):

Monthly variations of the zonal velocity along the meridional cross-section (Fig. 7b) reveal a dynamic two- to three-layer system, with a distinct westward surface current often overlaying a deeper counter current, as shown in the average profile (Fig. 7a). However, this system sometimes undergoes significant changes under specific scenarios. Firstly, during periods when the northwestward directed surface current dominates, it can extend to greater depths than average, leading to the absence of a distinct undercurrent…

Lines 289-290: This concluding sentence feels slightly disjointed from the preceding text, which is a very nice synthesis. Perhaps, the component of the discussion that is missing is: is it reasonable to assume that the vertical motion of marine substances limited to the upper 10-20m which the surface velocity field is representative of?

We understand your concerns and we address this matter in the discussion section 4.2, specifically in the last paragraph (lines 472-474), where we acknowledge that we are only considering surface currents and not the full vertical transport dynamics. With slight alternations to the text:

Finally, it is important to note that the particle simulation only considers the surface ocean layer, <u>which we correspond to the top 10-20 m of the water column. While this approach provides a reasonable approximation of average conditions within this depth range, it neglects</u> vertical movements and transport, which are crucial for studying nutrient fluxes. …

Line 295: Suggest adding 'reveals significant monthly and inter-annual variability'. More generally, it would be interesting to assess statistically whether the variability seen between monthly release maps is stochastic versus seasonal-interannual in nature. A similarity metric, such as the Fraction of Unexplained Variance (FUV) could be used to compare months and assess how similar any given month is to its monthly climatology (e.g., what fraction of the PDF shown for April 2023 can be explained by the April (2020-2023) average).

Thank you for your suggestions. We have updated this section of the results, incorporating the term 'inter-annual' in line 357 to better reflect the variability observed.

While we agree that computing metrics such as the Fraction of Unexplained Variance (FUV) could provide additional insights, we believe this analysis extends beyond the scope of our study. The comparison between the PDF (or, in our revised analysis, the unique particle count) and parameters such as current speed or direction is complex. The PDF is influenced by a wide range of factors, including speed, direction, convergence, and eddy kinetic energy (EKE), making the interpretation of such metrics non-trivial. Conducting this type of statistical comparison would require a more extensive investigation that lies outside the primary objectives of this manuscript.

Figure 8: As a non-domain expert, I found the large number of connectivity matrices in (c) to be difficult to interpret and, in contrast to the other Figures presented in the manuscript, to be the least effective at highlighting the key result of this Scenario. Two possible suggestions, which the authors are fully entitled to disregard, would be to replace the Source Zone numbers with geographical names as in (a) colouring font according to their location, and either masking or recolouring the 100% connectivity boxes (this value is known since particles are released here, but is the boldest feature in every subplot).

Thank you for your feedback. We have made several adjustments to improve the readability of the connectivity matrices (in revised manuscript Figure 9). We now placed the zone numbers next to the corresponding zones in panel (a) to make it easier to interpret the results. Additionally, we have changed the display of 100% connectivity values by creating black crosses over the white background.

Discussion and Conclusions

Lines 370-372: This sentence could be clearer, suggest modifying to: 'There is {broad/strong/good} agreement between the surface current vectors simulated by SCARIBOS and those estimated from Lagrangian surface drifters in the Caribbean Sea between 1989 and 2003 (Richardson, 2005).'

Thank you for the suggestion. We have incorporated the proposed modification into the manuscript (lines 185-186). We also moved this paragraph to section 2.3, following your suggestion below.

Lines 369-375: Much of this discussion would be better placed in the methodology section validating the SCARBIOS model. This would ensure readers have greater confidence in the simulation's ability to represent the circulation in the Caribbean Sea before it is applied in the Lagrangian analysis, rather than discussing this retrospectively.

Thank you for the suggestion. We moved this paragraph to lines 184-190.

Lines 376-381: This brief discussion on cyclonic eddies is interesting, but I feel more could have been done in the Results to explore this (see earlier general comments on temporal coarsening), including coarsening the flow field in time to extract the signature of high-frequency flow components on the connectivity and persistence of marine pollution around Curaçao.

We agree this would be an insightful addition, but we believe it goes beyond the scope of our study.

Lines 435-437: This raises an interesting discussion point on the residence times of particles around the coast, however, residence times were not addressed in the Results section. Was this intentional and based on a supplementary analysis of the particle residence times? It would be interesting (perhaps in future work) to combine the findings on the connectivity of positively buoyant marine substances with their residence timescales in coastal regions around Curaçao, since a highly connected reef with a low flushing (high residence) time scale would surely be more susceptible to marine pollutants.

Thank you for your insightful comment. Indeed, exploring residence times is an important topic for future work. We also understand that residence times should be properly accounted for, and we believe this can be achieved more accurately with 3D simulations.

Line 459-460: The absence of wave effects, including Stokes Drift, should be commented on in the methods section as it's an important limitation of the surface velocity field used in the Lagrangian analysis.

Thank you – we added Stokes drift in the methods section (line 152).

Lines 474-484: This concluding section on 'Implications and future directions' could be improved by emphasising the value of the SCARIBOS 1/100 simulation – what have we learnt with this model which is not attainable at lower resolution – and identifying several future questions which will directly inform policymakers and stakeholders. Currently, plastic debris, coral larvae and marine pollution are discussed collectively, but it would be interesting to know how SCARIBOS outputs could be used in each case, thereby underscoring its long-term value as both a scientific and societal resource. For example, could SCARBIOS be used to predict (or train an ML model to predict) pollutant spills and the resulting environmental impacts?

Thank you for your suggestion. We have revised this section to place more emphasis on the value of the 1/100° resolution (lines 542-545):

By taking the advantage of 1/100° resolution SCARIBOS simulation, it is possible to resolve smaller-scale transport pathways and retention zones that would remain unresolved in coarser models. This provides critical insights for predicting how plastic debris accumulates in specific areas, which can directly inform mitigation strategies.

We have also added more detail on how SCARIBOS can be applied to other cases, such as coral larvae dispersal (lines 547-548) and oil spills (paragraph in lines 552-556).