1Summary

Bertoncelj *et al.* describe a 1/100 degree configuration of the CROCO ocean model for the Netherlands Antilles. Using four years of model surface current output combined with Lagrangian particle tracking experiments, they focus on the island of Curaçao and investigate (i) accumulation 'hotspots', (ii) intraisland connectivity, and (iii) the potential for nearby islands and the Venezuelan mainland to act as sources of pollutants for Curaçao. I enjoyed reading the manuscript and, assuming the model output is indeed made publicly available prior to publication, I imagine that these data will be valuable more broadly, for a part of the ocean that appears to be somewhat data sparse. I was particularly impressed with the quality of many of the figures. I recommend the manuscript for publication, but would ask the authors to consider the suggestions below, which can be considered as major revisions. These suggestions mainly relate to (i) more rigorous assessment of model hydrodynamics, and (ii) the clarity of some analyses/discussion, particularly relating to scenario 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

1. The study would benefit from some more rigorous assessment of the hydrodynamic model. The authors show that SCARIBOS has good performance for tidal water levels (at least at one location), but tides presumably only have a minor effect on particle dispersal around Curaçao compared to the prevailing currents and eddies (and reproducing water levels associated with tides does not necessarily mean tidal currents are reproduced well). The comparison in Figure 2 with ADCP data is very good, but this is one snapshot in time. There is some further assessment of the model in section 4.1, but this is vague and qualitative. The manuscript describes SCARIBOS as a whole (which covers a much broader domain than just Curaçao), but the hydrodynamics are only assessed immediately around Curaçao. I would make the following suggestions:
    a. Surface currents (mean state, and ideally variability) should be assessed for the *entire domain* against an (ideally) observational product, e.g. GLOBCURRENT (and this should be done quantitatively and/or graphically, not just described in the text). Remote-sensing surface current products for the region will have a much coarser resolution than SCARIBOS, and will of course not resolve interactions with the islands, but will at least allow the reader to be satisfied that SCARIBOS reasonably reproduces the large-scale current systems and their variability (I did this very quickly by eye and it looks like SCARIBOS does quite well).

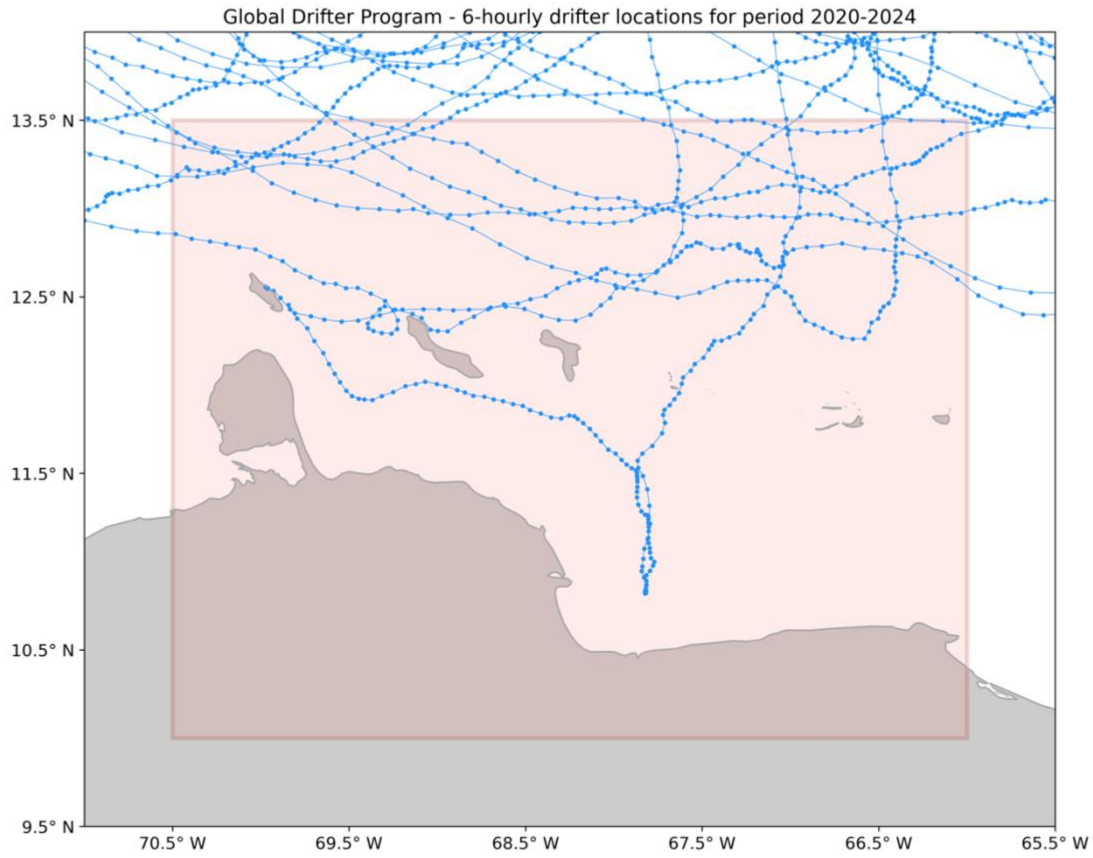
       We appreciate the suggestion to assess the model's performance over the entire domain, despite the study's primary focus being mainly around Curaçao. Following your recommendation, we performed a graphical comparison between the GlobCurrent dataset and SCARIBOS for the entire domain, using monthly averages for the 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 new manuscript to match the changes.

    b. Given the importance of cyclonic eddies for the dispersal dynamics described in the manuscript, quantitative comparison of eddies simulated by the model to observations would also be useful, if possible, particularly since the ADCP observations were during a non-eddying time. The manuscript cites several papers that discuss eddy dynamics in the region, so I would ask the authors to consider whether there are observational data they could use for this assessment.

       Unfortunately, observational data in this area is scarce. The eddies we refer to that are reported in the previous studies are mostly observed in the middle of Caribbean Sea, north of the SCARIBOS domain. Moreover, none of the referenced studies (Carton and Chao, 1999; Murphy, 1999; Richardson, 2005; Van der Boog et al., 2019) covers the period of our simulation (2020-2024). For example, Carton and Chao (1999) focus on latitude around 14° for their analysis, which is beyond our model boundaries. Richardson (2005) analyses surface drifters, out of which very few trajectories reached our domain and none of them are associated with (anti)cyclonic eddies (as shown in fig. 11 of their paper). Likewise, Van der Boog et al. (2019) analyses pathways of anticyclones observed in mid-Caribbean Sea with none passing our domain either.

c. The authors could also consider using Global Drifter Program drifter trajectories – if there are sufficient observations in this region – to compare true surface dispersal pathways (in a statistical sense) to simulated trajectories. This would be useful in the context of inter-island connectivity for Scenario 3.

That is a very good suggestion. However, unfortunately during our simulation period (2020–2024), there were very few Global Drifter Program trajectories passing through our model domain (see figure below).



<u>Figure:</u> 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.

2. I have a few concerns about the hotspot analysis (scenario 1):

   Your suggestions and the suggestions from the Reviewer #2 below about improving/clarifying the HOSTPOT analysis are very insightful and useful. Therefore, we have carefully examined our analysis and plotting, and made (major) changes shown in Fig. 8 in the revised manuscript:

   1. We re-run the HOTSPOT scenario in which we now released the particles every 12 hours instead of 24 hours. We did this to be consistent with the other two scenarios.
   2. Based on Reviewer #2's suggestion, we revised the post-processing metrics to use the 'normalized unique particle count.' This metric tracks the number of unique particles that pass through each bin at least once during the simulation. By normalizing these counts, we ensure comparability across bins and provide a clearer representation of particle distribution patterns.
   3. We calculated the time-averaged normalized unique particle counts across all simulated months. This approach highlights persistent patterns visible throughout most months, such as the shadowing effect along the southern coastline. More details below.
   4. We re-wrote the methodology, results and discussion based on these findings:
      a. **Methodology:** lines 259 to 266, explaining how the normalized particle count is calculated
      b. **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 band is not visible anymore. We now focus on the difference between the northern and southern coastline (Fig. 8a) and monthly variations (this part of the results did not change much). Changed lines 348-370 (most of section 3.2 is largely altered).
      c. **Discussion:** we changed from PDF to hotspots and from particle concentrations to particle counts in sections 4.2 and 4.3. The main conclusions stayed the same.

   a. I'm finding it difficult to see the 'big picture' with this analysis, probably because there is so much variability in Figure 7 – perhaps the authors have tried this already, but I wonder if plotting a time-average (e.g. across the 'normal' state and 'eddy-dominated' states) might make spatial patterns clearer here.

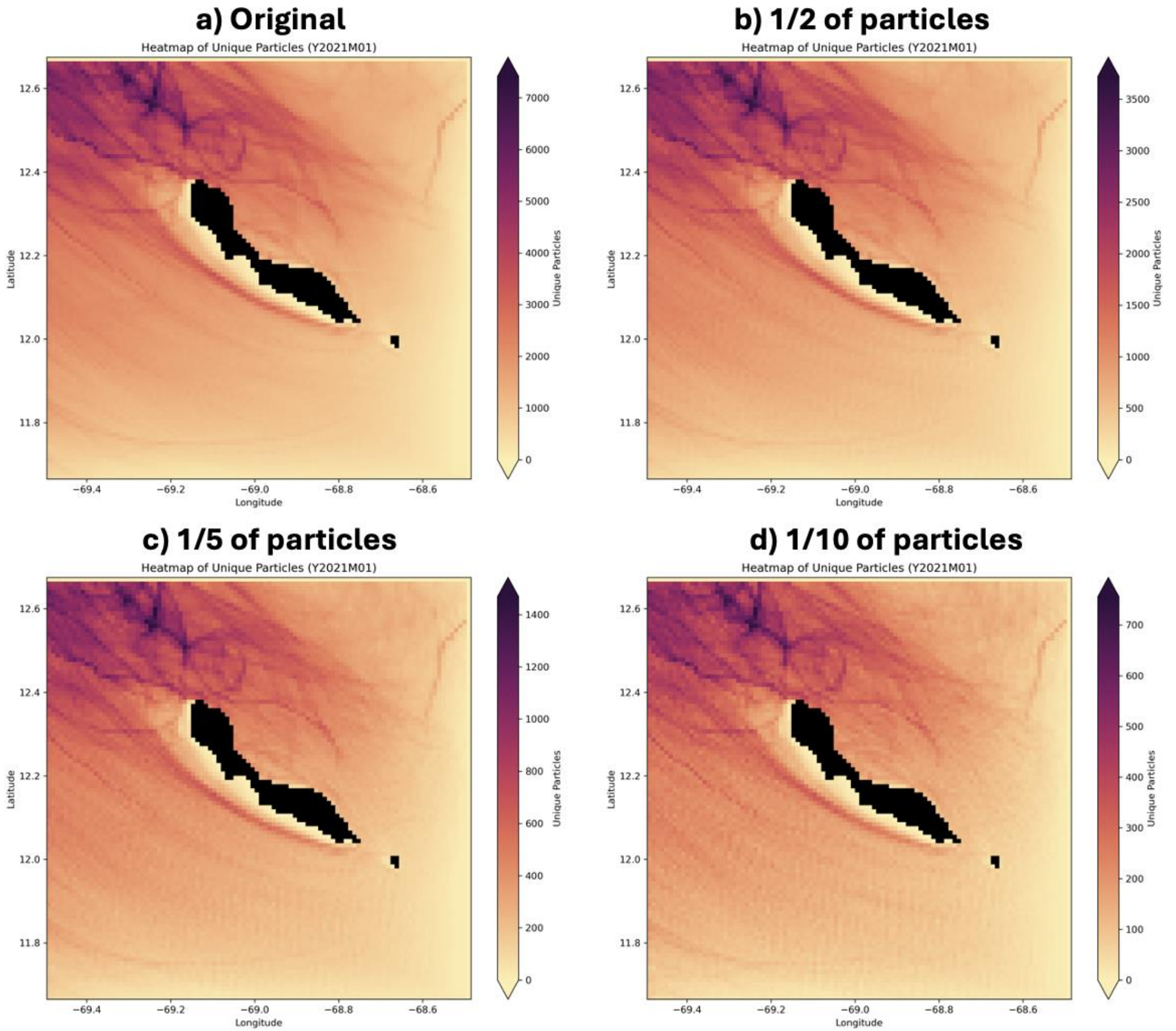
   To see the pattern that is observed across most of the months, we have created the time average of this normalized unique particle counts (averaging over all months, in revised manuscript Fig. 8a). We considered creating time-average across the 'normal' (i.e. 'NW flow') and 'eddy-dominated' state, but we found it too subjective and arbitrary to manually handpick the months associated with these two regimes, since it was often the case that half of the month was 'NW-flow-dominated' and the other half 'eddy-dominated'. Doing careful examination of these states would require much more detailed analysis into the exact distinguishing between the start and end of each state, which is out of the scope of this present manuscript.

b. L47 defines hotspots as "areas where land-derived substances spend considerably more time than in other areas" – however, particles were seeded primarily in ocean grid cells, not coastal grid cells. If hotspots are genuinely supposed to reflect the fate of land-derived substances, particles should only be released from coastal cells (as was the case in Scenario 2). If the analysis is instead intended to identify zones of accumulation more broadly, the decision to release particles in a 1x1 degree square seems arbitrary, and I would be interested to know how sensitive the PDFs in Fig. 7 are to this decision (e.g. does the described accumulation of particles NW of Curaçao persist if particles are seeded over a broader area?). I also wonder if computing the average residence time of particles might be a useful way of identifying hotspots.

We agree with the comment that the original definition of hotspots does not fully align with the analysis presented in the manuscript. In response, we have revised our definition as follows (line 52): *In our study, hotspots are defined as areas where substances are more likely to accumulate than in other areas, potentially leading to increased stress on coral reefs.* This updated definition is also reflected in the Results section (section 3.2, line 347).

We would like to clarify that we are not specifically investigating land-derived substances in the hotspot scenario (this is addressed in connectivity Scenarios 2 and 3). Instead, this scenario aims to examine how well different areas around Curaçao are connected and to identify 'hotspot' areas visited by a higher number of particles, regardless of their origin. While we acknowledge that releasing particles in a 1x1-degree square is an arbitrary choice, we conducted a sensitivity analysis to assess the impact of reducing the number of particles released. Specifically, we examined the normalized unique particle counts 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, without needing to expand the release area as suggested. Interestingly, the pathways with higher normalized particle counts remain prominent, even with only 1/10 of the original particles selected. We added an interesting observation from our results in the discussion – linking our results with well-known Lagrangian Coherent Structures (LCS) in a new paragraph in section 4.2 (lines 466-471).

Finally, while we agree that computing residence times could provide valuable insights, it falls outside the scope of the current study. The spatial resolution of our model is too coarse to produce meaningful maps of residence times, particularly for coastal cells, which would likely be of greater interest for most applications.



<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.

c. The manuscript discusses hotspots in the context of the Island Mass Effect, evidenced by higher particle densities NW of Curaçao. This may be true, but I wonder if there is physical evidence from the model output (e.g. evidence of convergent surface currents in the lee of the island) that would further support this being due to the IME, as opposed to just being a consequence of NW-ward currents.

We have investigated the convergence of surface currents for two contrasting months. Our findings show convergence in this region, which aligns with the observed higher particle densities, supporting the presence of the Island Mass Effect (IME). Additionally, divergence along the southern coastline contributes to the observed shadowing effect. While we do not include this analysis in the current manuscript, it forms a key part of our ongoing research, which will be presented in the next paper.

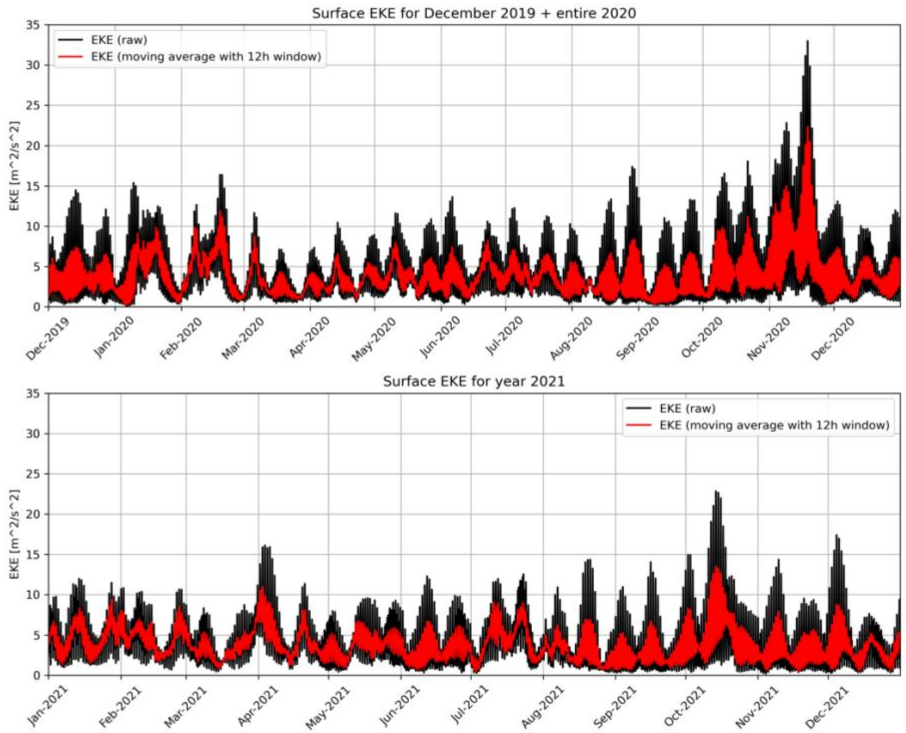
3. I was interested in the Waitt Institute report that was cited in the manuscript, which appears to have various data (e.g. sewage indicators, trash accumulation indices, infrastructure density) that could be directly useful in this study – e.g. weighting coastal particles in Scenario 2 by infrastructure density to weight the connectivity matrix in fig. 8 since some parts of the coast are more likely to generate pollutants than others, or testing whether trash accumulation hotspots agree with predicted hotspot locations. I wonder if these data could be useful for model assessment?

Thank you for this suggestion. We agree that this has a lot of potential, however, in this manuscript we are not focusing on any specific pollutant or substance, and doing that would require much more elaborated and additional analysis on the sources and concentrations of these pollutants. Our experience indicates that pollutant and nutrient concentrations vary seasonally and are influenced by combination of factors like population density, geology, hydrology, and the presence of bays, all of which can create point sources of increased land-derived matter. The complexity of these factors is unfortunately beyond the scope of this present manuscript.

Specific comments

Line	Comment
-	The maps in this manuscript (particularly Figure 1) appear to have been exported in a vector format, but this has made them very large (the size of the manuscript is 17MB) and causes lags when opening the file in a web browser. I would recommend exporting these maps in PNG format instead. Thank you very much for this suggestion. Accordingly, we changed the format of the maps to PNG with 300 dpi.
43	This does not undermine the purpose of this study, but in the interest of accuracy/fairness I would consider citing the NCOM AmSeas model (https://www.ncei.noaa.gov/products/weather-climate-models/fnmoc-regional-navy-coastal-ocean), which covers Curaçao at 1/30 degree resolution. Thank you very much for this suggestion. We have added the sentence in lines 44-47 and citation in lines 597-599.
51	Here and throughout (particularly section 2.3), I would use the word “assess” rather than “validate”. Although most readers will know what you mean, the word “validate” in this context is arguably incorrect (e.g. Konikow & Bredehoeft, 1992). We agree with your suggestion and have changed the wording accordingly throughout the manuscript.
55	Here and throughout, I would suggest being more specific about *which* substances the study is attempting to model (assumptions of positive buoyancy, no degradation, etc. will of course only be relevant for certain types of substances). This is particularly important in the context of section 4.3, as many of the pollutants that affect corals are not neutrally buoyant (e.g. sediment) and/or non-conserved (e.g. many nutrients). Thank you for this suggestion. For better clarification we included non-degradable, positively buoyant substances in the second research question (line 59):

	(2) How do these ocean currents affect the movement and distribution of non-degradable, positively buoyant substances at the ocean surface around the island, contributing to the formation of hotspots? Later in the manuscript we add *non-degradable* (line 248), in section 2.4 where we explain that our particles are: *passive particles, representing nutrients and pollutants… These particles, simulating* ***non-degradable****, positively buoyant substances, move with the surface flow conditions.* We understand that substances such as nutrients and pollutants do not act this way, but we want to stress that we are making an 'idealized' case here. We explain this in section 4.3 (lines 488-491) as: *Our study focuses on passive particles representing pollutants and nutrients, which are positively buoyant and non-degradable. These represent idealized conditions. In reality, many pollutants and nutrients behave beyond these assumptions, such as degradation and interactions with other substances, thereby highlighting the limitations and scope of our approach.*
59	Here and throughout, avoid the word "fine" in the context of resolution: it is subjective and context-dependent, just write the actual resolution. We agree and we have changed in the manuscript accordingly.
Fig. 1	This is a good figure, a couple of minor comments: - The contour increments in the colour map are slightly inconsistent with the tick labels in the colour bar (this is also the case for fig. 4). - The brown land polygons are slightly inconsistent (higher resolution?) with the grey coastline Thank you very much for pointing these details – we have changed them accordingly.
119	I assume the vertical resolution is finer at the surface – if this is the case, I would specify it (and possibly give the range of values of the thickness of the upper layer). Thank you for pointing this out. The vertical resolution is indeed finer at the surface, with the thickness of the upper layer ranging from a few centimetres to approximately 4 meters. We have included this information (lines 125-126).
120	Please justify how it was determined that 4 months was a sufficient spin-up duration. 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 product. 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.

	 *<u>Figure:</u> 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.*
121	How frequently were currents saved (hourly?), and were these snapshots or averages? What was the frequency of current data used in Parcels? We have added the additional information in lines 129-130: *Model outputs include hourly averages of horizontal and vertical velocities, temperature and salinity, stored for every grid cell in the domain …* Additionally, in the description of particle tracking, we mention that Parcels uses the hourly average outputs from SCARIBOS for particle tracking (line 247).
123	I assume that smoothing was performed on the bathymetry (as I believe is standard for ROMS/CROCO preprocessing) since very steep slopes can cause stability issues. If this is the case, I think this should be mentioned in the methods. Indeed, that is the case. We have added this information in lines 136-137: *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.*
124	The comment on "adjusting land grid cells" is very vague – I would briefly add the reason for these adjustments (presumably because the land-sea mask generated from the bathymetry is inconsistent with the true coastline). Indeed, that is the reasoning. We have clarified that in lines 138-141:

	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.
134	What is the source of the river discharge data (and is this based on monthly averages/monthly climatology…)? The source is a database by Dai and Trenberth, 2002, which we have referenced now in line 151: *… based on a climatological river discharge dataset by Dai and Trenberth (2002).* We have accordingly also added a new citation in lines 622-623.
189	Given the relatively limited role of tides in setting marine dispersal, and the fact that the ADCP comparison is based on one snapshot, I do not think we can conclude that "SCARIBOS accurately simulates surface-level dynamics, *making it a reliable tool for tracking surface currents*" on the basis of the evidence presented so far (see general point 1). Since we have added another assessment method following your advice (the GlobCurrent assessment), which specifically targets the surface currents across the entire domain, we decided to keep this statement here.

204	How did you determine the number of particles to be released? Was there a sensitivity analysis? We release them at the original spatial grid of the SCARIBOS model, because this is the finest resolution at which there is meaningful information in the data.
204	Is there a reason why particles were released every 24 hours for Scenario 1, but every 12 hours for Scenario 2? I don't think this is an issue, just seems a bit odd that different release frequencies were used. We have decided to re-calculate the hotspot scenario with release time of every 12 hours to have consistent release frequencies in all scenarios.
256	This mention of El Niño is a bit random – is it relevant to the local hydrodynamics that it's an El Niño year? This is again mentioned in line 368. We believe that it is possible that El Niño played a role in different hydrodynamics in year 2023 (there are notably more low-energy/eddy-dominated months in this year compared to other years), so we decided to mention it in the manuscript.
Fig. 5	I really like these figures showing the monthly state of the model, I think they are very effective. Thank you very much!
267	Have there been hydrographic surveys in the region (or anything else) that supports this vertical velocity profile and the countercurrent? There is (very limited) data on the vertical extent from our expedition, but we decided not to include it here, as this manuscript (except from this paragraph) focuses on surface currents. The 3D circulation will be the topic of a follow-up manuscript.
271	I am not sure about tying this in with the AAIW… even if there is a signature of AAIW here, I would not have expected this to be relevant to the dynamics, which is the topic of this manuscript. We agree that this is not relevant, and since it is bold statement without much additional analysis, we decided to discard it from the manuscript.

Fig. 7

I have a few comments about this figure:

Thank you very much for your careful inspection of this figure and for your useful suggestions!

- I would consider either (i) using a coarser binning grid, (ii) having more frequent particle releases, or (iii) outputting particle locations more frequently, because many of these sub-panels have a checkerboard pattern in the PDF. Using a coarser binning grid may also make the figure easier to interpret (by smoothing out some of the filamentation).

  We have decided to increase particle releases (from 24h to 12h, as mentioned above) and to use more coarser binning grid. We made a binning grid of 100x100, matching the resolution of SCARIBOS. However, with all these adjustments we still did not avoid checkerboard pattern, as this is the nature of the results.

- Have you tried producing a time-mean (or seasonal, or non-eddy/eddy mean PDF)? Because there is so much variability, I am finding it difficult to see the 'big picture' from this figure.

  As already mentioned above, we followed your advice and created time-mean of these results. We agree that this is a valuable contribution to the results, as it shows the general pattern without the reader needing to investigate each month separately.

- Zooming into the sub-panels, there is something going on in the background of Curaçao, see below (is the particle distribution being plotted on top of a raster outline of Curaçao)? I would remove this and keep the background fully white, otherwise this could be mistaken as representing data (high particle density).

  Thank you very much for pointing this out. Indeed, this is a mistake in the plotting. We have repaired this in the revised manuscript (now Fig. 8).

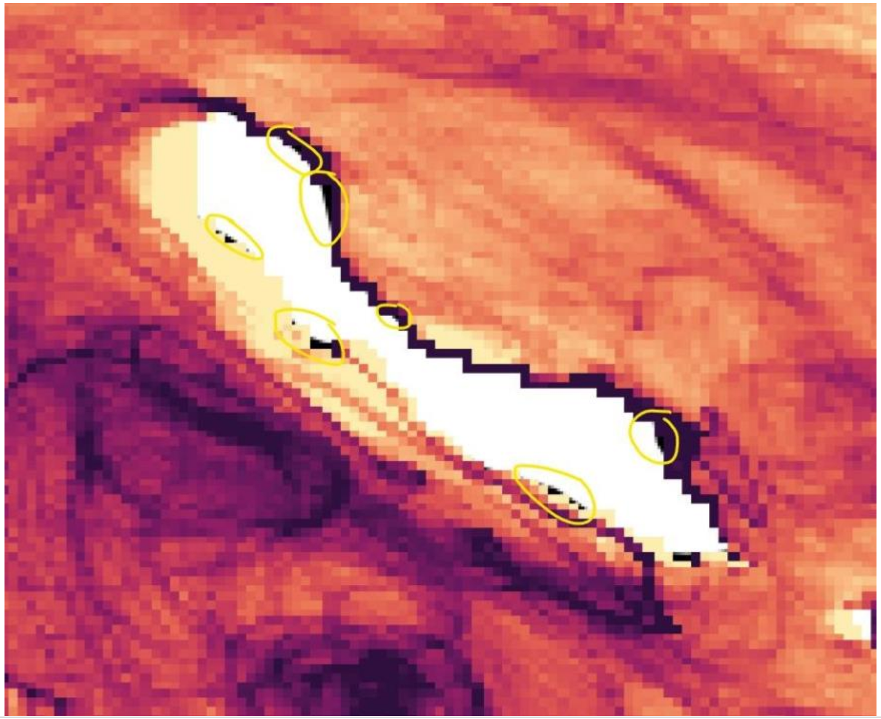


Fig. 8	Panel (a): because the changes in colour between some neighbouring areas are quite subtle, it may be helpful to readers to add numbered labels on the map next to each zone (this would also make it easier to quickly cross-reference between panel (a), and panels (b) and (c)) Panel (c): This is a really cool figure! I wonder if it might be useful to mark (e.g. with horizontal dashed lines) months associated with the passing of major eddies. Thank you very much for these helpful suggestions! We added the numbering to the zones in panel (a) for easier cross-referencing in new manuscript, now Fig. 9. Regarding your suggestion for panel (c), we decided not to mark the months associated with the passing of major eddies. This is because we did not want to manually handpick the months for the two regimes, as it was often the case that the months were split, with half of the month being 'NW-flow-dominated' and the other half being 'eddy-dominated' (as mentioned above).
329	However, most of the particles arriving at Zone 7 come from Zone 8 (and to a lesser extent, Zone 6), which are not highly populated. I understand that this study is not attempting to model any one particular pollutant (and therefore does not use a specific input function) but I do think it is important to qualify that, although Zone 7 may receive the most particles, this does not necessarily mean it will receive the greatest pollution burden. A similar point is made in the paragraph beginning on line 438. Thank you for raising this point. While we understand your concerns, we intentionally avoid linking pollution impact solely to population density. The reason for that is that pollution sources on the island are influenced by various factors, something that other colleagues in the Sealink project focus on at the moment. For instance, complex hydrography, groundwater flow and illegal dumping of pollution in low-density areas. With our unbiased approach we give the chance for a reader to interpret the results based on the pollution source they are interested in.
340	Analyses for Scenario 2 assume that substances of interest have a lifespan of 30 days, which seems quite arbitrary. If you think this goes beyond the scope of this study then I accept that, but it would be interesting to know how sensitive Figure 8 is to particle lifespan. We chose a conservative lifespan of 30 days, as our experiences show that most particles leave the area around Curaçao within just few days.
Fig. 9	This could mess up the layout, but it might be useful to have a reference map (similar to fig. 8) to remind the reader of where these different sources are relative to Curaçao. That is a very good suggestion. We have incorporated a reference map showing the particle locations after the first 50 hours for the example month of April 2020. This map is now included as Fig. 10a.
382	I am struggling to understand the point being made in this paragraph – can you relate this discussion of the IME to your results? Does the model predict large-scale downwelling in the lee of the island? Does the model predict upwelling (and divergence) along the south coast of Curaçao where there appears to be unidirectional offshore transport in surface waters? Thank you for your thoughtful questions. While our model simulations do show upwelling, downwelling and divergence, a detailed analysis will be addressed in a follow-up study. In this manuscript, we focus primarily on the broader surface flow patterns, and we only mention the 3D circulation in a discussion as the preliminary observations.
400	I am not sure I understand why this paragraph is referring to the sticky water effect – my basic understanding (and as stated in the text) is that the 'sticky water' effect specifically refers to currents that are *retentive* but (as discussed in the previous paragraph), currents around the south coast of Curaçao instead appear to be highly dispersive – even though there are some similarities in the physical mechanism, i.e. current diversion around an obstacle. I understand the point being made, but is 'sticky water' the right term to describe it? Likewise in lines 433 and 467. We agree that the term 'sticky' water seems counter intuitive. Our intention was to connect our observed phenomenon to previous studies and use a known term to provide context. While we acknowledge in our manuscript that the phenomenon we observe is quite the opposite, the key

	similarity is the lack of mixing between offshore and nearshore waters, which both phenomena share. Therefore, we prefer to keep the term 'sticky water', but now better explain (lines 449-450): *Although the effect we observe differs fundamentally from the 'Sticky Water effect' (e.g., Andutta et al., 2012; Restrepo et al., 2014), both phenomena share a key similarity: the lack of mixing between offshore and nearshore waters.* We changed the wording similarly in the next paragraph in lines 457-463: *While the predominant northwestward Caribbean Current generally acts as a flushing mechanism, carrying particles away from the island and reinforcing the disconnection between nearshore and offshore currents, the reduced current speed during cyclonic eddies narrows the band along the southern coastline associated with this disconnection. …[]… However, the hotspot analysis indicates that this nearshore disconnected band – similar in behaviour to the Sticky Water effect – although narrower, still persists during these events.*
409	Similar to the point on line 382, is there evidence (e.g. vertical velocities diagnosed from the model) that prove this is due to the IME? As mentioned in my comment on Fig. 7, I think this might be easier to see with a multi-year seasonal mean. Thank you for your suggestion. We have decided to delete this paragraph, as the new hotspot post-processing analysis (normalized unique particle counts) no longer shows this phenomenon. Furthermore, as mentioned earlier, we are not analysing 3D patterns (such as vertical velocities) in the present manuscript and therefore do not have sufficient evidence to support these claims.
419	Change "reduce pollutant concentrations" to "reduce neutrally buoyant pollutant concentrations", since downwelling will *increase* the concentration of positively buoyant pollutants. Thank you very much for this comment. We have changed it accordingly in line 479.
442	It might be useful to mark the location of major settlements on figure 4(b) and/or 8(a). Thank you for your suggestion. However, we have decided to keep it as it is, as we have now added zone numbers next to the zones in Fig. 9a. This makes the figure already clearer and easier to interpret.
444	I am confused by the point being made here. What is meant by "the limited distance travelled by substances such as pollutants" – does that refer to their degradation timescale in the ocean? Surely that depends on the type of pollutant and, regardless, the results in this study still suggest that the south coast probably has low pollutant retention? Similarly, I don't understand the claim that there is "reduced dilution" (L448). Figure 7 shows that particles that start on the south coast quickly move away, and are not replaced by new particles. That sounds like dilution to me. Thank you for pointing this out. To clarify, the statement about the limited dilution is based on the expectation that pollutants closer to the source are less dispersed due to limited travelling distance so far (compared to pollutants travelling large distances). While our study focuses on connectivity and not specifically on pollutant dilution (and neither degradation), we have rephrased the paragraph to make this point clearer. Revised manuscript lines 507-511: *Although the retention times in these areas are relatively short, pollutants released near the source are likely to remain more concentrated locally because the particles do not spread as far before leaving this area, increasing the likelihood of localized impacts on coral reefs. While our study does not explicitly model pollutant dilution, it is reasonable to expect that areas closer to the source will experience higher concentrations compared to areas further away, where pollutants would disperse over larger areas.*
453	"Various substances" - I would suggest giving examples of substances carried by Venezuelan rivers that could be modelled by the approach taken in this study (i.e. positively buoyant, lifespan of longer than a month). Thank you for this suggestion. We added some examples in lines 516-519:

	… various substances, such as microplastics and microorganisms like bacteria and viruses attached to buoyant debris. Additionally, spill events from oil refineries along the western coast of Paraguaná, Venezuela, are among the documented sources of oil pollution in the region (Croquer et al., 2016). With a new reference for Croquer et al. (2016) in lines 620-621.
478	Coral larvae are not always positively buoyant – their buoyancy declines with age (Szmant & Meadows, 2006). As they mature and gain vertical swimming ability, they have some control over their position in the water column, which varies considerably by species (e.g. Mulla et al., 2021; Tay et al., 2011). The point you are making here is fine, but I'd change "…which are also buoyant" to "…which often remain in the upper water column" (or similar). You are correct, thank you very much for pointing this out. We have changed it as suggested (lines 545-546).
479	Since there was no limit on the particle lifespan in Scenario 3, I think you need to quantify the connectivity timescale between Bonaire and Curaçao before you make this claim. I am not sure how coral disease is transmitted, but I assume the pathogens and/or vectors have a limited lifespan in the water column? We quantified the time scale of the fastest 5% of the particles which is roughly 40 hours. We added this information to line 551: *The timescale for such travel can be as fast as 40 hours when the currents are strong and in the right direction.*
482	I would restate in this sentence which specific areas you consider to be "critical areas" – at the moment, this is a very vague and generic statement. We agree and we have added a statement for clarification (line 558): *Identifying critical areas – such as regions with coral reefs that are particularly susceptible to high pollution loads – on a regional scale is essential…*
493	The configuration files should probably be archived on Zenodo or similar prior to publication, since GitHub is not a permanent repository. Thank you for your suggestion. We archived the configuration files at Zenodo, with DOI https://doi.org/10.5281/zenodo.14697794. We added this in reference to lines 569 and 604.

Technical comments

Line	Comment
36	Would rephrase to "Coral reefs are not just impacted by local sources of pollution, but also by broader environmental changes and anthropogenic activities" (otherwise it sounds like "broader environmental changes and anthropogenic activities" are an impact on coral reefs, rather than a source of impact). Very good advice – we have changed it accordingly (lines 37-38).
Fig. 6	Change B) to (b) in the panel label. Changed.
312	Would change "western" to "westward" Changed.
316	This was mentioned earlier, but I would remind the reader at this point where the highly populated areas on Curaçao are. We added zones 3 and 4 (in brackets) for clarification (line 373).
344	Change 9A, 9B etc. to lower case. Changed.
348	Change "a strong signal" to "high particle transport" (or similar).

	Changed to 'high particle transport'.
451	Consider changing "connection" to "upstream connection" Changed.
475	These sentences ("While this study focuses on… pollutants, its methodology can be adapted… for tracking plastic debris") makes it sound like plastic debris is not a pollutant. Indeed, it may come across that way. In our manuscript we introduce our land-derived substances with examples of pollutants from the sewage system. Here we wanted to distinguish between pollutants that behave passively and resemble (dissolved) matter from the pollutants that have very different characteristics (such as size and shape → plastics). We changed in the line 541 from *plastic debris* to *buoyant (macro)plastic pollution too*.

References

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