Dear Editor and Reviewers,

We sincerely appreciate your thorough review and constructive feedback on our revised manuscript, "*Flow patterns, hotspots and connectivity of land-derived substances at the sea surface of Curaçao in the Southern Caribbean*," submitted to the *Ocean Science*.

We have carefully addressed all the feedback provided and implemented the suggested revisions in the manuscript, with particular attention to the additional validation of the model. Along with the revised version, we are submitting a detailed response letter addressing your comments and suggestions, as well as supplementary material consisting of six figures: Fig. S1 representing the EKE time series and Figs. S2-S6 providing additional model validation analysis.

We hope that these revisions adequately address all concerns and further enhance the manuscript. We look forward to your feedback and hope that the manuscript will be suitable for publication in *Ocean Science*.

We appreciate your time and consideration.

Sincerely,
Vesna Bertoncelj (on behalf of all co-authors)

Response to RC1:

We greatly thank the reviewer for their valuable comments and suggestions on our revised manuscript. Below, we respond to each comment and address them accordingly. Line numbers mentioned in the text correspond to line numbers in the revised manuscript.

L65: I'd suggest changing "community model" to "ocean model" or "regional ocean model".

Agreed. We changed it to "regional ocean model".

L123: The precision for the grid dimensions (5 significant figures) feels excessive – the exact dimensions are in the model configuration file anyway. I would consider just writing 1.1 km.

Agreed. We changed it to 1.1 km.

L127: The EKE plot in the reviewers' response was useful, and I think this is an important justification for the spin-up duration of 4 months. This figure (integrated EKE vs time) would ideally be included in the supplementary materials but, otherwise, I would at least mention this in the main text (i.e. that the 4 month duration allowed EKE to stabilise across the model domain).

We thank the reviewer for this suggestion. We added the sentence in lines 129-130:

> *This duration allowed that the eddy kinetic energy (EKE) stabilized across the model domain (Fig. S1), ensuring the system reached a steady state before the analysis period began.*

Additionally, we added the figure showing the EKE time series as a Supplimenary Figure S1.

L178-180: I don't think this explanation for the difference between SCARIBOS and GlobCurrent is correct. GlobCurrent is based on observed SSH, so the effects of islands are included (since they exist in reality and therefore affect the observations) even if they are not resolved in the gridded GlobCurrent product. GlobCurrent can only capture the geostrophic and Ekman components of the surface flow, so it would not capture any ageostrophic effects of the islands on the flow. However, the differences between SCARIBOS and GlobCurrent are not just immediately around/downstream of the islands. SCARIBOS does not include data assimilation, so some differences will just be due to turbulence. There are some consistent differences though, e.g. most of the westward surface transport in GlobCurrent is north of the islands, whereas SCARIBOS simulates strong flow south of the islands as well. These differences are notable as they could affect the results of the manuscript.

We thank the reviewer for these insightful comments and for pointing out the issue in our explanation. We have revised the paragraph to clarify the differences in strong flow positioning between SCARIBOS and GlobCurrent and the possible reasons for these differences. Moreover, we compared SCARIBOS with GLORYS12V1 model (the model used for initial and boundary conditions) and found strong agreement between these two models (we added this comparison to Supplementary Fig. S2). This suggests that the differences from observational dataset by GlobCurrent likely arise from the choice of the boundary conditions. The changes are in lines 185–193:

> *SCARIBOS effectively captures months with stronger currents, particularly in February, March, April, July and December 2022, where differences arise from the positioning of the core of the current. Notably, SCARIBOS simulates strong flow both south and north of the islands, while GlobCurrent shows stronger currents primarily north of the islands. These differences may stem from SCARIBOS resolving finer-scale flow interactions around the islands, including ageostrophic effects, whereas GlobCurrent, while incorporating the large-scale influence of islands through sea surface height observations, does not resolve their small-scale effects explicitly. When comparing SCARIBOS with GLORYS12V1 (Fig. S2), a much stronger agreement is observed, with both models showing strong currents both north and south of the island. This is expected, as GLORYS12V1 provides the boundary conditions for SCARIBOS, indicating that the differences with GlobCurrent likely arise from the choice of boundary conditions.*

L260: "when particle" -> "when a particle", and "a grid" -> "a grid cell"

We have changed the text accordingly.

L310: I'm still not convinced by this mention of El Niño. The authors wrote in their response that they "believe it is possible that El Niño played a role in the different hydrodynamics in 2023 [because] there are notably more low-energy/eddy-dominated months", but a 4-year simulation is nowhere near enough time to confidently attribute something to El Niño (as opposed to some other mode of variability or random chance). If there are other studies that investigate the impacts of El Niño on eddy activity in the region then please cite them and explain the connection, otherwise I do not think it is appropriate to mention El Niño.

We thank the reviewer for pointing this out. We decided to follow the reviewer's advise and deleted the statement (line 350). Furthermore, we deleted the mentioning of El Niño in line 475.

L350: "origin" -> "origins" or "source regions", and "Contrary" -> "Conversely" or "On the contrary"

We have changed these accordingly.

Fig. 9: I preferred the old version of the figure where source=destination cells were filled, although I understand why the authors chose to block them out. However, I would at least suggest exporting sub-panels 9(b) and (c) as vectors, because the rasterised crosses don't look great at low resolution.

We decided to cover the cells with crosses following suggestions from Reviewer #2, as we agree that in this way the most notable highlights in the figure are not the 100% self-connectivity cells. We follow the reviewer's suggestion and have exported the figure now as vector for better resolution.

L438: I'm struggling to see how the results of this study are being attributed to the Island Mass Effect. In the original figures, it looked like there was particle accumulation immediately downstream of the island, but this is no longer the case in the revised figure 8. There's certainly perturbation of the flow, but the IME is more than just perturbation of the flow – it specifically relates to vertical movement. The authors wrote that they have further evidence for this which will be part of a future manuscript, but I am not reviewing that manuscript, and I can't see convincing evidence for the IME in this manuscript.

We follow the reviewer's suggestion and described observations without mentioning Island Mass Effect. The changes are in lines 482-486:

> *The interaction between currents and the island perturbs the strong northwest-directed oceanic flow. This current-island interaction results in generation of eddies in the lee. The strong horizontal divergence leads to significant differences in speed between currents upstream and in the lee of the island. In our study, this effect is particularly notable during periods when the currents are strongly northwest directed. In these periods, vortices form due to the island's influence on flow dynamics, resulting in reduced flow strength in the northwest of Curaçao.*

L449-452: Similarly, I am still unconvinced about this comparison with the Sticky Water Effect. The authors have now clarified that the effect observed in this study is not the Sticky Water Effect… in which case, why mention it at all? Just describe the observations.

We follow the reviewer's suggestion and described observations without mentioning Sticky Water Effect. The changes are in paragraph in lines 488-493:

> *Understanding the movement of particles is crucial for evaluating how substances (e.g. pollutants and nutrients) are transported around Curaçao. The monthly simulations of hotspots around Curaçao reveal a complex dynamic, particularly along the southern coastline, where a notable disconnection exists between the nearshore currents and those passing further offshore. Particles released nearshore are primarily transported offshore by surface currents. Concurrently, particles released from locations further away rarely reach the coast due to the separation between the nearshore flow and the ambient offshore currents that pass at a distance from the island. This results in a low accumulation of particles near the southern coastline.*

L466-471: I would remove this discussion of Lagrangian Coherent Structures from the manuscript. I can intuitively see why regions of higher normalized unique particle count might correspond with attracting LCS, but is this a physically rigorous way of identifying LCS? I am not familiar enough with LCS theory to answer that, but I suspect it is not. This would be an interesting future study, but this discussion feels out of place in the present manuscript given the lack of evidence provided. I certainly think the claim "This highlights the critical role that LCS play in shaping surface substance transport around Curaçao" should be removed, as should the Gomez-Navarro et al. 2024 reference (it does not use the same region or same method, so don't see how it is relevant apart from the fact that it deals with LCS). The Haller (2015) reference is also missing from the reference list.

We agree that this is not enough to make such claims and would be an interesting further study. We decided to follow the reviewer's suggestion and delete the entire paragraph on LCS from the manuscript including the references associated with it.

L509: I cannot see any evidence for the claim that "particles do not spread as far before leaving the area" – no particle dispersion metrics (e.g. Lyapunov exponents) were computed in this study.

We agree with the reviewer's observation and have revised this paragraph (lines 538-542) to focus on the fact that Willemstad, as a city, presents a much higher risk of spreading pollution compared to uninhabited zones:

> *Furthermore, the connectivity analysis indicates that the areas adjacent to the capital city Willemstad (zone 4, Fig. 9a) are also largely exposed. Urban runoff from this region poses a significant threat to nearby coral reef communities, as pollutants released here are likely to have greater impact on coral reefs. While our study does not explicitly model pollutant sources and concentrations, it is reasonable to expect that areas closer to Willemstad face higher risks compared to more remote areas, such as Klein Curaçao, which is uninhabited and unlikely to contribute significant pollution.*

L553-554: The mention of machine learning is a bit random. If the authors really want to include this then OK, but particle tracking is not particularly expensive in the first place, so I don't see why this application is specifically notable.

We thank the reviewer for this feedback. We have removed the mention of machine learning and re-wrote this paragraph to focus more on operational application of the methodology. Revised text in lines 583-586:

> *SCARIBOS simulations and the Lagrangian connectivity pathways could also be applied to predict pollutant spills and their potential environmental impact. By adapting our methodology, the model could be expanded for operational use by incorporating forecasted boundary and forcing conditions. This would enable more accurate predictions of pollutant transport and support the development of rapid-response tools for environmental risk management.*

Data availability: Is there a plan for how readers can access the full dataset if the two contacts were to leave their institution and no longer had access to these email addresses? Is there not a more sustainable solution for making this dataset available?

The model outputs will be available through https://dataverse.nioz.nl/, additionally we provided the general Research Data Management mail.

Response to RC2:

Summary

I gratefully thank the authors for having taken my comments into account and for their efforts to revise the manuscript. This revised version reads well and resolves almost all of my original concerns on the model's limitations and wider relevance of the study's findings. I have just one general comment concerning the updates to the model validation and several minor comments motivated by the authors' revisions. I would recommend the manuscript for publication subject to any minor revisions needed to address the comments made below.

> We greatly thank the reviewer for their valuable comments and suggestions on our revised manuscript. Below, we respond to each comment and address them accordingly. Line numbers mentioned in the text correspond to line numbers in the revised manuscript.

General Comment

While I am very grateful to the authors for responding to mine and Reviewer #1's concerns regarding the rigorous assessment of the SCARIBOS model, I still believe that there is an opportunity to improve the validation of the simulation included in the revised manuscript.

The authors' addition of a graphical comparison between the GlobCurrent dataset and SCARIBOS model is a welcomed one. However, I would strongly recommend regridding the SCARIBOS model output onto the coarser grid of the GlobCurrent data to allow for an equivalent comparison between the two products. This would also enable the authors to present the [model - obs.] bias of the simulated surface current field when compared with GlobCurrent observations. Naturally, there would still be a caveat that the SCARIBOS model includes more than just the Ekman and geostrophic contributions to the surface velocity field, but this would still be more informative than the current visual comparison.

> We thank the reviewer very much for this suggestion – we agree that regridding SCARIBOS improves the comparison, and we have changed it accordingly (Figure 2). Moreover, all the supplementary figures that we have added to the manuscript (as stated below) also include regridded SCARIBOS outputs to match the spatial resolution of the compared model/observation.

On a similar note, I believe that Figure 4 could be improved by visualising only the model surface current velocity vectors co-located with the ADCP observations while retaining the background colour contours representing the surface currents of the entire model domain. This would allow for more of an 'apples-to-apples' comparison since the authors have already selected data for the observational period between 4th and 22nd January 2024.

This is also a very good suggestion! We have changed Figure 4 accordingly.

Finally, I believe greater attention should be paid to validating the SCARIBOS model's ability to represent ocean properties, given that communicating the model's overall fidelity will assist future researchers considering using the simulation in their own work. In particular, I would suggest validating the sea surface temperature and sea surface salinity fields outputs by the SCARIBOS model against relevant observations (e.g., OISSTv2 – see Huang et al., 2021 - and the Multi Observation Global Ocean Sea Surface Salinity product – see Droghei et al., 2016).

We followed the reviewer's suggestions and compared SST and SSS from SCARIBOS with multi-observational products (SST: Guinehut et al., 2012; SSS: Droghei et al., 2016). We incorporated this comparison into the text (lines 177-181 introducing the datasets, and lines 211-228 describing the comparison), making substantial adjustments to Section 2.3, and included the corresponding figures as supplementary material (Figs. S3 and S5).

The agreement in SST is high, while some differences were observed in the SSS fields. To further investigate these discrepancies, we compared SCARIBOS with the GLORYS12V1 model, which provides its initial and boundary conditions. The two models show strong similarities, suggesting that the differences between SCARIBOS and the observational product likely stem from the model dataset used for initialization and boundary forcing. We included comparisons of SST, SSS and surface currents with GLORYS12V1 model in the supplementary material too (Figs. S2, S4 and S6).

Moreover, it also occurred to me that the GO-SHIP / CLIVAR Repeat Section A22 (last completed in 2021) intersects the SCARIBOS model domain, and thus may provide an insightful meridional-cross section with which to compare the model's temperature and salinity field as part of an improved Figure 7 (which does not currently include observations).

We appreciate this suggestion but have decided not to validate the meridional cross-section at this stage. The A22 section intersects our western boundary, making it less representative of the meridional cross-section studied in Figure 7. Additionally, we plan to include a comprehensive 3D validation of our model in our next manuscript, where we will specifically focus on the 3D ocean properties and circulation in the SCARIBOS simulation.

I would like to emphasise that the suggestions above are intended to give the scientific community even greater confidence in the SCARIBOS model, and hence encourage the wider use of this simulation beyond this particular study (which itself will serve as the documentation and validation of the model going forward).

Minor Comments

Line 298-299: When does particle seeding conclude in Scenario 3? I could not determine from the current text whether particles are still being released during the final month of the simulation (i.e., February 2024) and, if so, how are these dealt with in the Lagrangian analysis? For example, do you take into consideration the much longer advection time for those particles released in 2020 compared with those released in 2024?

We thank the reviewer for pointing out the potential confusion. To clarify: as stated at the beginning of the paragraph (line 321), particles are seeded from the first until the last day of each month and are tracked until they leave the domain or until the end of the SCARIBOS simulation period. While the final month (February 2024) has less simulation time left, in practice, most particles exit the domain within 30 days, with only about 1% remaining at most. No special treatment was applied to the last month of February 2024. We have added the statement: *“Most particles exit the domain within 30 days of release.”* to line 328.

Figure 8: Suggest using two separate colorbars for the upper and lower plots given that the colorbar is saturated in the lower plot, but not in the upper plot. This makes the upper plot appear washed out and does not highlight any prominent pathways.

We thank the reviewer for this suggestion. We changed Figure 8 by adding another colorbar.

Lines 391-394: Could you quantify this description of strong flushing events somehow? For example, is it the case that particles released in DJFM are more likely (higher proportion) to leave the model domain via the northern and western boundaries compared with those released throughout the rest of the year?

We thank the reviewer for this suggestion. While our study does not aim at quantifying the portion of particles leaving a certain domain boundary, the reviewer is correct that during the strong flushing events more particles are leaving the domain via the northern and western boundaries. We added a sentence to the paragraph in lines 407-408:

> *Conversely, during strong flushing events characterised by strong northwest-directed currents, particles are pushed further away from the southern coastline, resulting in a greater distance from the land for these particles. <u>During such</u>*

events, particles are also more likely to leave the domain via the western and northern boundaries.

Figure 9: What is special about 2021 for the connectivity of Source regions 6, 7 & 8? This interesting anomalous lack of connectivity feels worthy of comment; what is this related to in the surface circulation?

We thank the reviewer very much for pointing out this valuable observation. We added this observation to the text in lines 435-439 as:

> *A notable period from December 2020 to August 2022 stands out, as connectivity from source zones 6, 7, and 8 is very limited, except to their immediate neighbors. This coincides with a prolonged phase of predominantly northwest-directed surface flow (Fig. 6), which remains strong for most of this period. Interestingly, even during months with weaker flow in this period (e.g., May, June 2021), connectivity from these sources remains minimal.*

Section 3.4: One intriguing question that came to mind re-reading this Section was: what conditions give rise to the anomalously high and low coastal connectivity values shown between Bonaire and the Venezuelan Islands and Curaçao. We'd certainly care strongly about instances of the former, especially in the context of marine pollution and its wider impacts. Are these episodes of enhanced coastal connectivity predictable?

That is indeed an intriguing question. However, based on the analysis presented in this manuscript, no clear connection can be drawn. For example, the absence of connectivity between Bonaire and Curaçao in July and August 2021, despite predominantly strong northwest-directed currents during these months, suggests that the system is more complex than a straightforward link between regional surface flow direction and observed connectivity patterns. Since our focus has been on surface flow around Curaçao, it is possible that during this period, the current was directed more NNW, causing particles to pass just north of the island. A month later, however, connectivity increased to nearly 50%. Without additional analysis, we cannot fully explain these extreme variations, and we believe that predicting such episodes would require further investigation using additional metrics and an overview of the full domain surface flow.