Summary

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) intra-island 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.

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

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.

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?

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.
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.
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).
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).
59	Here and throughout, avoid the word “fine” in the context of resolution: it is subjective and context-dependent, just write the actual resolution.
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
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).
120	Please justify how it was determined that 4 months was a sufficient spin-up duration.
121	How frequently were currents saved (hourly?), and were these snapshots or averages? What was the frequency of current data used in Parcels?
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.
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).
134	What is the source of the river discharge data (and is this based on monthly averages/monthly climatology…)?
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).

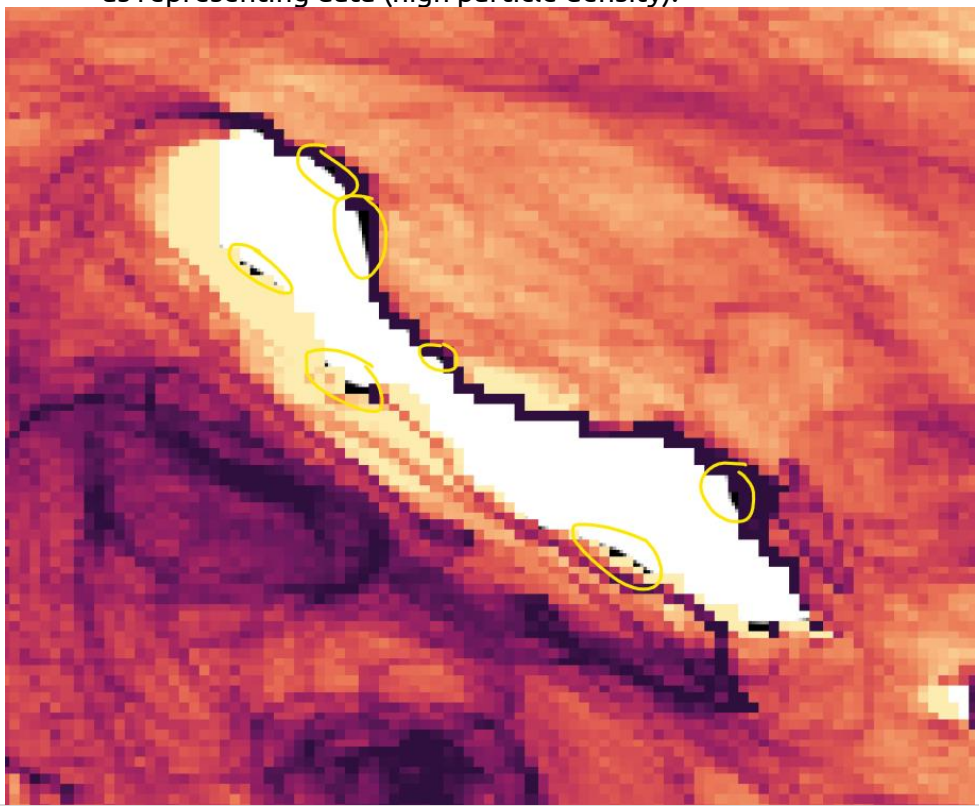
204	How did you determine the number of particles to be released? Was there a sensitivity analysis?
204	Is there a reason why particles were released every 24 hours for Scenario 1, but every 12 hours for Scenario 2? I don't think this is an issue, just seems a bit odd that different release frequencies were used.
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.
Fig. 5	I really like these figures showing the monthly state of the model, I think they are very effective.
267	Have there been hydrographic surveys in the region (or anything else) that supports this vertical velocity profile and the countercurrent?
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.
Fig. 7	I have a few comments about this figure: - 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). - 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. - 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). 

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.
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.
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.
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.
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?
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.
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.
419	Change "reduce pollutant concentrations" to "reduce neutrally buoyant pollutant concentrations", since downwelling will *increase* the concentration of positively buoyant pollutants.
442	It might be useful to mark the location of major settlements on figure 4(b) and/or 8(a).
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.
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).
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).
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?
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.
493	The configuration files should probably be archived on Zenodo or similar prior to publication, since GitHub is not a permanent repository.

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).
Fig. 6	Change B) to (b) in the panel label.
312	Would change “western” to “westward”
316	This was mentioned earlier, but I would remind the reader at this point where the highly populated areas on Curaçao are.
344	Change 9A, 9B etc. to lower case.
348	Change “a strong signal” to “high particle transport” (or similar).
451	Consider changing “connection” to “upstream connection”
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.

References

Konikow, L. F., & Bredehoeft, J. D. (1992). Ground-water models cannot be validated. *Advances in Water Resources*, *15*(1), 75–83. https://doi.org/10.1016/0309-1708(92)90033-X

Mulla, A. J., Takahashi, C. L. S., & Nozawa, Y. (2021). Photo-movement of coral larvae influences vertical positioning in the ocean. *Coral Reefs*. https://doi.org/10.1007/s00338-021-02141-7

Szmant, A. M., & Meadows, M. G. (2006). Developmental changes in coral larval buoyancy and vertical swimming behavior: Implications for dispersal and connectivity. *Proceedings of the 10th International Coral Reef Symposium*, *1*, 431–437.

Tay, Y. C., Guest, J. R., Chou, L. M., & Todd, P. A. (2011). Vertical distribution and settlement competencies in broadcast spawning coral larvae: Implications for dispersal models. *Journal of Experimental Marine Biology and Ecology*, *409*(1–2), 324–330. https://doi.org/10.1016/j.jembe.2011.09.013