Reply Comment #1 – Noam Vogt-Vincent

Firstly, I think this is a great manuscript and I enjoyed reading it – a novel idea with potentially very useful results for those involved in marine debris management efforts on Galapagos (and other remote islands if this methodology were implemented elsewhere). The effectiveness of the clean-up strategies proposed in this manuscript will depend on the veracity of the assumptions a significant proportion of debris undergoes resuspension, but this is clearly stated in the conclusion. So overall, I think this manuscript will be valuable for island managers, and researchers working in this field. I do have some technical questions/requests for clarification though.

We would like to thank Noam Vogt-Vincent for going through our manuscript in so much detail. We in particular liked the suggestion made for improving our sensitivity study to initial distributions using a different type of noise: these new results are a nice addition to our manuscript. Our response to each individual comment can be found below.

1. *I wonder whether you carried out a sensitivity analysis to test whether you released sufficient particles? With such high resolution hydrodynamical data forcing your particle-tracking, significant dispersion can occur over a 60-day integration time with an original separation of <4km (e.g. the off-diagonal cells in the transition matrix are 'noisy', and this is probably why). This in itself is not an issue since practitioners will probably not be using your transition matrix, but it would be nice to know how robust, say, Figure 7 is to particle number. I'm guessing that you were limited to 700k particles due to storage (since you were saving particle positions with a very high output frequency) but, if it is tractable, a quick sensitivity test might provide some assurance.*

Using only half the number of particles, we arrive at the same conclusions as presented in the manuscript, indicating that the number of particles used leads to statistically sound results. We have the hypothesis that the 'noisy' off-diagonal cells are a result of the beaching parameterization used, not the limited number of particles. To decrease this 'noisiness', one could decide to produce a multitude of transition matrices that are every time slightly different due to the beaching parameterization and then take a mean. It is however questionable whether this is the right or realistic thing to do, as improving the underlying macroplastic flow model (e.g. improved parameterizations as highlighted by Moulton et al., 2023, taking into account more relevant (atmospheric) processes) will probably lead to more applicable results.

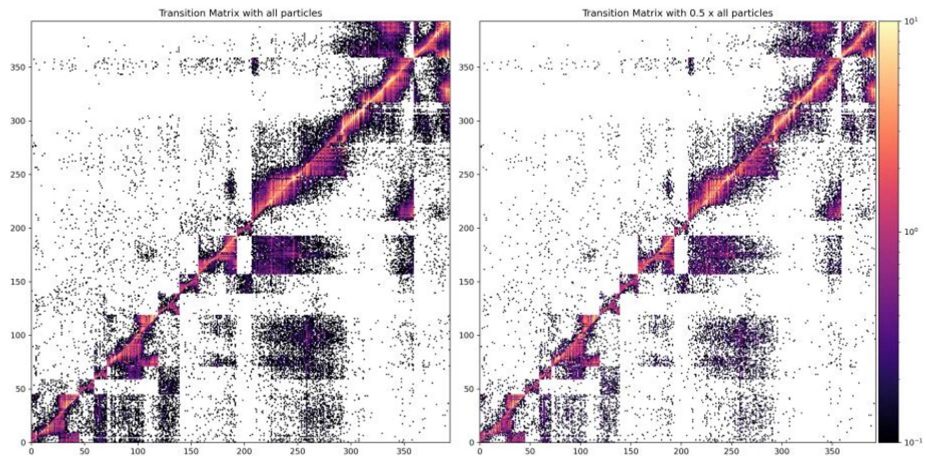

Figure 1 The transition matrix (left) in the original manuscript and (right) using only half the number of particles. We repeated all analysis with the transition matrix on the right hand-side and all results and conclusions do not significantly change.

2. *I’m struggling to completely follow section 2.5. If I understand correctly, the equation on line 162 models the distribution of debris on coastlines in the limit of 100% resuspension. You simulate clean-up as removing the outgoing nodes of a clean-up target cell. You then say that “particles will accumulate at the target cleanup node”, but how can particles accumulate if you’re constantly removing them?*

We agree that the wording we used is confusing and we’ve rephrased the sentence in the revised manuscript. Instead of 'accumulate at the target cleanup node' we changed the text to say: 'After every iteration, *the incoming particles at the target cleanup node are removed from the system* until a steady state is reached, which provides a means to quantify the cleanup impact.'

3. *It’s also not clear to me why you based your definition of steady state on the number of particles in the ocean – does the vector* $\mathbf{v}_t$ *not reach steady state?*

There are indeed three parameters that could be used to find steady state; the number of particles on land ($\sum vt$), in the ocean and the total number of particles removed. The procedure used to define steady state will give different results depending on which of these parameters is used, in particular when a larger fraction of the coastline is cleaned (see Fig. 2 below). Although the number of iterations needed for reaching steady state change, the main conclusions derived from this figure do not change. We do agree that using the total number of particles on land (**vt**) to define steady state is more intuitive and have changed Fig. 4 and 5 accordingly in the manuscript.

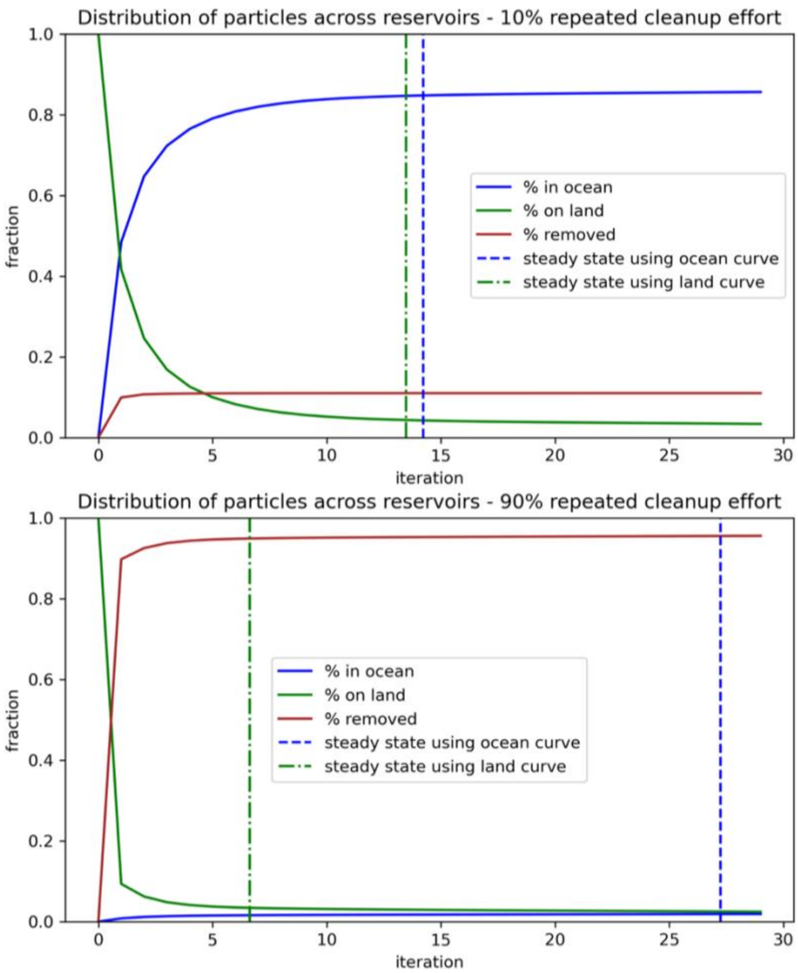

Figure 2 Steady state analysis.

4. *I'm also finding the "benefit" metric quite difficult to follow. "Zero connectivity between different nodes", to me, implies that 100% of resuspended debris enters the ocean, but I don't think this is what you meant?*

Yes, this is exactly what we mean. If there would have been zero connectivity, this would imply that all resuspended plastic is lost to the open ocean and does not beach at another location within the marine reserve. Therefore, when one would clean e.g. 30% of the coastline, you would also remove 30% of the pollution at the first iteration (assuming again that the pollution is uniformly distributed along the coastlines). Every iteration following you will clean 0%, as there is no 'new' plastic arriving from other locations. When there is connectivity between regions, you will not only be cleaning the 'local' pollution but also the pollution that is arriving from other locations. We have defined the 'benefit' metric to quantify this additional cleaning impact one can have due to the connectivity between different locations. We've adjusted the explanation in the manuscript to read:

'The benefit metric indicates the difference (in %) between the total number of particles removed and the number of particles removed *if all non-removed particles were directly lost to the ocean after the first iteration.*'

5. *I'm not 100% convinced by your sensitivity tests to the initial macroplastic distribution (3.3). You've tested how robust your method is to uncertainty in the initial distribution by using a completely random initial distribution, i.e. assuming that the mass of plastic on beaches is completely decorrelated across length scales L > 4km. Is this realistic? Your result that the efficacy of node rankings remains the same with a random initial distribution is not surprising to me, since mesoscale ocean structures are much larger than 4km so will on average still see a 'uniform' distribution of resuspended debris. But given that van Sebille et al. (2019) showed that most debris incident to Galapagos arrives from the East, and that there could be large-scale effects from wind shadows, wake eddies, etc., I'd have expected that there would be some large-scale structure in the distribution of debris. I wonder if a more realistic way to model an uncertain initial macroplastic distribution might be by generating perlin noise with a wavelength larger than 4km (e.g. maybe the length scale of an island).*

This is a good suggestion and we've repeated the analysis with a random initial distribution using different correlation length scales within the range of observed spatial scales of marine debris distributions in the ocean (order of 1-10 km, e.g. Kaandorp et al., 2020). We've decided to add the results as a second panel to our previous random initial distribution test, as both sensitivity studies provide different insight (see Fig. 3 below). The former sensitivity test, based on a completely random initial distribution, shows that with an increased fraction of coastline being polluted, using centrality rankings for cleanup efforts becomes more promising. The new sensitivity test, where the initial distribution is correlated across a spatial scale larger than the grid size of the nodes, shows that using the centrality ranking for cleanup efforts is almost always more successful than knowing where most pollution is located. Important to note here is that this is true as long as the cleanup resources are limited (<10% cleanup effort).

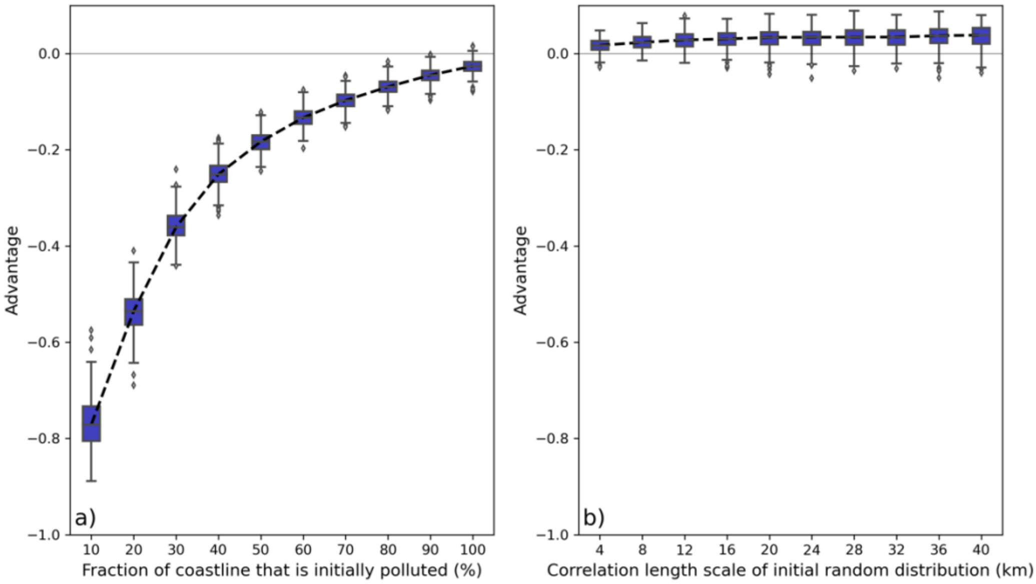

Figure 3 The difference between the total removed particle mass when using the SSIsink centrality and the total removed particle mass if the initial distribution of particles is known. The difference is plotted as (a) a function of how clean the coastline is initially (in %) using a purely random distributed particle weight and (b) a function of the correlation spatial scale used to initialize the random initial distributed particle weight. For both calculations, a cleanup effort of 10% is applied and each calculation is repeated 500 times. Outliers are shown with diamond markers.

6. *This is not a criticism, but I wonder to what extent using a time-mean transition matrix affects your results. For instance, if we have sites A, B and C each releasing 1 unit of debris per time-step with probability P(A→B)=1/6 and P(B→C)=1/6, the probability of transition P(A→C) is 1/36 (over two timesteps). After 12 time-steps, 1/3 units would have been transported from A→C. But if these transition probabilities were time-varying and turned out to be P(A→B)= P(B→C)=1 during time-steps 1-2 and 0 otherwise, 1 unit would have been transported A→C – 3x more than the time-mean case, even though P(A→B)= P(B→C)=1/6 in both cases when averaged over 12 time-steps. This is obviously an artificially bad case and I completely accept that this goes beyond the scope of your study, but I didn't see a mention of this in your discussion of limitations so I was wondering whether you thought this was a limitation (or if there is evidence that this time variability in the transition matrix is not important).*

This is also a valid point. There is a strong seasonal dependence in the flow fields within the marine reserve, so we would expect the results to change (which locations to target) when using for example monthly transition matrices. However, as soon as you start using these you will 're-introduce' the time dimension, which means you also have to start making choices related to resuspension time scales and the frequency of cleanups. We agree that this is outside the scope of this paper, but it is definitely an interesting next step to consider in addition to investigating the implicit connectivity method raised by reviewer #2 and have added these suggestions to the discussion and outlook section of our manuscript.

References

Kaandorp, M. L. A., Dijkstra, H. A., & van Sebille, E. (2020). Closing the Mediterranean Marine Floating Plastic Mass Budget: Inverse Modeling of Sources and Sinks. *Environmental Science & Technology*, *54*(19), 11980–11989. https://doi.org/10.1021/acs.est.0c01984

Moulton, M., Suanda, S. H., Garwood, J. C., Kumar, N., Fewings, M. R., & Pringle, J. M. (2023). Exchange of Plankton, Pollutants, and Particles Across the Nearshore Region. *Annual Review of Marine Science*, *15*(1), null. https://doi.org/10.1146/annurev-marine-032122-115057