Reply Reviewer #2

This manuscript presents an interesting statistical analysis of the connectivity patterns in the Galapagos to infer the coastal locations with a highest probability of accumulating marine macroplastics. In particular, the authors combine concepts from the graph theory, I.e, centrality derived metrics, and a hydrodynamical model to provide a map with the coastlines of the Galapagos Marine Reserve classified according to the optimization in removing marine litter.

Some of the findings are: provide a methodology to cleanup strategy that can be applied even when there are not data about the distribution of macroplastics; among the centralities metrics, the retention rate provide the most useful information to localize regions for cleanup; it is more effective in terms of removing macroplastic to clean at sink points (coastlines with large positive Source-Sink index values: high SSI_sink values) than at source locations.

In general, the authors present a interesting work aimed at improving the Lagrangian identification of coastal locations with high probability to find macroplastic advected by the ocean flow. The strategy adopted by the authors, i.e. the use of outputs of a hydrodynamical model and the methodology employed through the network theory is technically sound, turning out to be appropriate for the scope of this work. This is a good piece of work which could be of interest to some OS readers. However there are some weakness in the manuscript an a revision has to be addressed before publication. An improvement of the methodology description is strongly recommended before a new submission. Some aspects related to the methodology should be better supported and discussed more appropriately in the context of previous literature. The structure and organization of the Introduction section lack in clarity, where sentences are repeated. I think the paper could be considered for publication after major revision. The main issues that need to be clarified by the authors are listed bellow.

We thank the reviewer for the positive and useful feedback and our response to the individual comments can be found below. We have incorporated the specific comments regarding the clarity of the introduction section.

1. *I am not sure if the size of macroplastics (>0.5 cm) allows the macroplastic particles to be considered as Lagrangian particles? I think that to compute Lagrangian particle trajectories one needs to assume that the particle has to instantaneously follow or to be totally constrained to the motion of the fluid flow. Even for particles with finite size, the Maxey-Riley approximation to resolve the equation of motion for inertial particles, assumes that particles are very small (small microplastics?).*

The reviewer is correct that the construction of our transition matrix is based on virtual particles that are passively transported by only surface currents. As mentioned in the manuscript we do not incorporate additional transport processes due to e.g. wind, waves and tides or the 'inertial' effects that are due to the buoyancy, size or shape of the macroplastic. Including these processes with the aim to build a more realistic transition matrix currently still involves many parameterizations and further assumptions and even the application of the Maxey-Riley approximation to oceanographic scale is not yet fully worked out (e.g. Beron-Vera 2021). We do acknowledge, as stated in the manuscript, that these processes likely impact the resulting pathways and therefore the transition matrix, but chose not to include them because we choose to focus on the network-theory component.

Our reasoning for this is twofold. Firstly, it is not our aim to focus on the impact of different processes on the macroplastic pathways within the marine reserve as there are currently no observational means available to validate or interpret the results. Second, the main aim of the paper is to introduce a network methodology that can assess the impact of cleanup strategies when a transition matrix is available. The methodology itself is therefore independent of changes in the transition matrix formation. As mentioned in the manuscript 'the method can be easily extended' by constructing a 'more accurate' transition matrix when including the relevant processes.

We do still specifically focus on macroplastic as removing larger items during beach cleanups is most effective and it allows us to restrict our analysis to surface currents and, based on the timescales of interest, neglect processes like biofouling and fragmentation.

2. *One weakness of the manuscript is the description of the methodology. Some of the definitions are not clear. For example the definition of retention rate, the loss rate, etc. I think the authors can greatly improve the definition of these centrality metrics through mathematical expressions, i.e using equations. For example using the mathematical formalism based on connectivity probabilities between network nodes in the weighted network, starting from the definition of the transition matrix.*

We agree that the retention rate, loss rate and beaching rate can be better understood when adding a mathematical definition. For consistency throughout the manuscript, we now use the edge weight definition to write the expressions for the retention rate ($p_{so=si}$), the loss rate ($p_{so,ocean}$) and the beaching rate ($1 - p_{so=si} - p_{so,ocean}$). We did not include mathematical expressions for the PageRank Centrality and the Betweenness Centrality as these are widely used and known centralities for which the interested reader is referred to the relevant literature.

3. *Have the authors considered that in temporal networks, as the analyzed here, also the synchronous arrivals at a node could impact on the network connectivity metrics? The "cumulated" implicit connectivity is based on adding up all the events of synchronous arrival (see Ser-Giacomi et al, 2021, PRE). However considering implicit connectivity could modify the resulting connectivity patterns*

We thank the reviewer for pointing us to this interesting work and have not yet considered investigating implicit connectivity. We feel that its application to the transition matrix based on connectivity of macroplastic transport between various coastlines and the corresponding interpretation is not straightforward and will likely result in a full study on its own. We have added a suggestion in the discussion section of the manuscript to further investigate this in the future.

4. *The resolution of the model is too coarse as to resolve submesoscale dynamics. Ignoring submesoscale motions is not a simple matter when it comes to surface material dispersion. It is well known that submesoscales are associated with vertical motion (an extreme case was documented via drifter measurements by D'Asaro et al., 2018 PNAS paper). The submesoscales cannot be removed from the analysis when their impact on the horizontal transport properties is substantial. They also generate high convergence zones, which could impact the connectivity probability between nodes. Please provide arguments to show that by dismissing small scale dynamics in the small region, the applicability of the results obtained here to the real ocean do not become very uncertain.*

The virtual particles are released using the surface currents of a 3D model simulation with a 4 km horizontal resolution. As described by Forryan et al. 2021, the model is able to resolve most of the relevant submesoscale variability and related frontal dynamics. The fact that we only use the surface currents for our studies does not mean that these are non-divergent, in fact, there are strong convergence and divergence zones displayed by the particle trajectories.

The non-resolved (< 4km) small scale dynamics is likely to only significantly impact the transport processes in the near-shore. This is also the region where additional coastal processes such as rip currents, swell, and wind shadow zones of islands become important, that are, regardless of the model resolution, not included. We fully acknowledge that by being able to resolve and include the effect of these processes for the transport of virtual macroplastic particles (which is still an active field of research) would make the transition matrix more accurate. However, as mentioned in our reply to comment (1), the aim is to introduce the network methodology once a transition matrix is formed, not to study the accuracy of the formation of the transition matrix itself.

The reviewer is right that the applicability of the presented 'most effective cleanup regions' for the Galapagos Islands, at this stage, is therefore limited. We agree that we can state this more clearly in the manuscript and in relation to this comment, comment (1) and (5) and comments from other reviewers, we have adjusted both the introduction, method and discussion section of the manuscript to make our aim of our work and applicability of our results clearer.

5. *Numerical diffusion. The spatial and temporal resolution of the model (4km and one day) could originate large numerical diffusion in the computation of the Lagrangian particle trajectories. Note that assuming velocities of 0.6m/s we obtain that in one day (the temporal resolution) the particle could move around 50km, which is 12 times larger than the spatial resolution (~4km). This can be "fixed" by decreasing the time step in the Lagrangian integration scheme, but still some small scale dynamics is missing, and this could affect to the Lagrangian transport associated with the large scale structures.*

In addition to the spatial resolution (comment (4)), our results are naturally also sensitive to the temporal resolution of the model simulations used for the particle tracking. We did already use a smaller time step for the Lagrangian integration scheme (1 hour) than mentioned by the reviewer and as we are using daily-mean surface velocity fields, the potential error in particle displacement is not as large as the reviewer suggests. Furthermore, previous studies have investigated the sensitivity of particle dispersion and connectivity to the temporal averaging of the ocean Lagrangian simulation and show that daily temporal resolution and daily particle releases are sufficient to incorporate connectivity fluctuations due to variable currents (e.g. Qin et al. 2014, Monroy et al. 2017).

Related to comment (4), we expect that mainly the transport in the near-shore is sensitive to the temporal resolution, as e.g. tidal effects become more important. We like to point the reviewer to the changes made in the discussion on the applicability of the presented results.

6. *One of the advantages of the methodology is that it can be used independently of whether there exists available macroplastic distribution data or not. However, a validation exercise could be beneficial, in particular to better support the conclusions.*

We fully agree with the reviewer here, but are unfortunately not able to validate our results at this stage as current available observational means are insufficient. There are still many challenges to overcome to be able to validate Lagrangian particle tracking simulations with observations of marine debris abundance at e.g. beaches or the ocean surface or surface drifter trajectories. As recently reported by Uhrin et al. (2022), challenges are for example the low spatial and temporal resolution of observations and the use of non-consistent measurements units.

Part of our future work will focus on the insights from a recent drifter field campaign to improve some of the parameterizations for macroplastic transport and beaching, but this is not the focus of the current manuscript. Furthermore, local authorities, in collaboration with the Galapagos Conservation Trust, plan to design a measuring campaign to quantify the non-local impact of cleanups, which results will be vital to validate and can hopefully improve the presented methodology in the future.

7. *In the introduction section, I found some long and complicated sentences that could be split. Lines 30-32. Lines 37-39.*

Suggestion followed; both sentences are now split in the revised manuscript to improve readability.

8. *The sentence in line 40: "[...] the connectivity can still, to a first order [...]" could be improved. The connectivity by its self does not inform about aggregation but rather some derived metrics, and under some assumptions. Please clarify it.*

Suggestion followed, we've rephrased the sentence to: '*If the source distribution of macroplastic is unknown, the connectivity can still be used to derive which regions are likely to accumulate macroplastic and from which regions macroplastic is likely to spread to other coastlines, within the limits of the macroplastic flow model accuracy.*'

References

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