



EGUsphere, referee comment RC2
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Comment on egusphere-2022-426

Anonymous Referee #2

Referee comment on "Detecting the most effective cleanup locations using network theory to reduce marine plastic debris: a case study in the Galapagos Marine Reserve" by Stefanie L. Ypma et al., EGU Sphere, <https://doi.org/10.5194/egusphere-2022-426-RC2>, 2022

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.

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?).

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.

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

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.

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 (~ 4 km). 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.

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.

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

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.