Review of: *Transient Attracting Profiles in the Great Pacific Garbage Patch*, by Kunz et al.

This manuscript applies the theory of *Transient Attracting Profiles* (*TRAP*s, defined as regions of strong attraction identified from the instantaneous velocity field) to identify regions of attraction in the Great Pacific Garbage Patch. Using a 20-year long dataset, the authors track TRAPs through time, identifying regions in the garbage patch that exhibit large numbers of TRAP trajectories, regions that exhibit the longest-lived TRAP trajectories, and regions with the highest average attraction rates. They correlate the location of TRAPs to the edges of mesoscale eddies, identifying a typical quadrupole pattern of eddies around a given TRAP. They also show that drifters are typically attracted to TRAPs, with shorter retention times on average compared to TRAP lifetimes.

Overall, the manuscript provides a novel analysis and is a nice contribution to the field. Below I provide some major and minor comments that I think would help to improve the manuscript.

Major comments:

1. Temporal continuity of TRAPs

The most important issue to address is the temporal continuity of TRAPs. TRAPs by definition, are features that arise in the instantaneous velocity field, and the Serra et al. (2020) paper describes TRAPs as '*short-term attractors'*, and that '*TRAPs necessarily persist over short times'*, with an example of a TRAP existing for several hours. They used a high-spatial resolution HF Radar dataset, along with a high-resolution MIT-MSEAS forecast model with hourly output. Their focus was, of course, on the timescale of hours due to the search and rescue nature of their paper.

Lifetimes of TRAPs in this manuscript are in the timescale of days to almost a year long, and it's not clear to me how spatially proximate detections of TRAPs at consecutive timesteps necessarily determine that these TRAPs are the same object. TRAPs are, by definition, instantaneous features that '*necessarily persist over short times'* (Serra et al. (2020)). They could emerge, persist for hours, and later die, all within a day.

Can the authors provide more evidence on why TRAPs can be tracked on timescales of days (and months), when they may not exist for more than, as I understand, a few hours? Could successive TRAP identifications simply be older TRAPs decaying and newer TRAPs emerging? The comparison with drifter-TRAP pairs shows typical retention times of just a few days, with the largest retention time being 46 days, far shorter than the longest lifetime of a tracked TRAP. As it stands, I don't think there is enough in the manuscript to make that connection, and additional justification is needed.

2. Additional mathematical rigour

Section 2.1 Transient Attracting Profiles. This section would benefit from a more thorough description of the theory of TRAPs. In particular, additional rigour in the mathematics is required to make the method more readable to users. As I understand, $s_i = s_i(\mathbf{x}, t)$ are, in fact, eigenvalue **fields**, and $\mathbf{e_i} = \mathbf{e_i}(\mathbf{x}, t)$ are eigenvector **fields**. The manuscript then describes $\mathbf{e_1}$-lines and $\mathbf{e_2}$-lines, along with local minima of s_1 and local maxima of s_2, which from the current description of s_i and $\mathbf{e_i}$ don't make sense. This section (and later sections) would benefit from more careful notation and rigour.

3. Spatial analysis of TRAP trajectories

Section 3.1 Spatial distribution of TRAPs. I like the spatial analysis, however I think it is hampered by the same problem that spatial analyses using Lagrangian approaches have. Specifically, that trajectories of TRAPs (like Lagrangian particles) that start outside of the domain and later enter the domain (or TRAPs that start in the domain and shortly exit the domain), will be undersampled throughout their true lifetimes, and necessarily have shorter lifetimes (on average) than those that start and remain in the domain throughout their entire lifetimes. Given the size of the domain, the timescales of the largest TRAP lifetimes, and the 20-year duration of the dataset, can the authors comment on any bias this might have on the analysis?

Minor comments:

1. Overall, the manuscript would benefit from additional editing, for language and grammar, as some parts of the manuscript are a little hard to follow.
2. The abstract contains notation ($\overline{\Lambda}$, and $\overline{\varphi}$) which are somewhat confusing if not explained, I would stick to more plain language.
3. Throughout the manuscript, the authors describe 'attractive regions' in the flow. To be consistent with typical dynamical systems literature, these should be described as 'attracting regions', 'attractors', or 'regions of high attraction'.
4. Line 73, the authors mention 'inevitable errors'. Can they expand on what these errors are? Inevitable in the sense of those that Serra et al. (2020) mention with numerical integration schemes, or other errors?
5. In Section 2.2-2.6, the authors use a maximal arclength of 1°, a search area of $\epsilon = 0.25°$ (which corresponds to the model resolution), and a maximal drifter-TRAP pair distance of 75km. These choices seem a little arbitrary, can the authors comment on why they chose these parameters? Could one, for instance, choose a drifter-TRAP pair distance that is related to the TRAP attraction rate? Weak TRAPs may not influence debris 75km away, but strong TRAPs can?
6. The authors use a 0.25° spatial resolution velocity dataset, which is quite coarse for operational purposes. Would the authors expect similar results (and similar statistics) when using a higher resolution velocity field (e.g. 0.1° eddy-resolving, or even higher submesoscale resolving velocity fields more commonly used for operational purposes)?
7. In the discussion around Figure 4, can the authors give further explanation for why the locations of the strongest average attraction rate, number of TRAP trajectories, and largest average TRAP lifetimes don't correlate well? Could this be hampered by the major point above (point 3)?
8. On lines 425-426, the authors say the computations of OECSs and TRAPs are 'instantaneous'. Do the authors mean these computations are on 'instantaneous datasets'?
9. The paragraph on line 428 describes a debate in the community on whether mesoscale eddies accumulate and transport material, whether the transport by an eddy is largely outside of the eddy core, and whether objective methods exist that identify the periphery of an eddy. This discussion point is missing some references, and would be further enhanced with comments on the following articles which describe the transport by both the eddy core and the periphery of an eddy core:

   Early et al. (2011) (using relative vorticity in an idealised flow),
   Froyland et al. (2015) (using finite-time coherent sets from a transfer operator),
   Denes et al. (2022) (using finite-time coherent sets from a dynamic Laplace operator).
10. The manuscript suggests that the TRAP approach is useful to marine debris cleanup operators, but the analysis is mostly statistical, analysing a large set of TRAP trajectories. A nice-to-have would be a description of how operators may use the TRAP approach in their cleanup operations.
11. The manuscript would benefit from more discussion around the potential applications of the TRAP-tracking approach, mentioned in the very last line of the manuscript (lines 470-

471). The current main application mentioned is marine pollution cleanup, but a broader description of the applications (by expanding the very last line of the conclusion) may benefit a broader audience.

A couple of spelling issues:

1. Line 153, ‘programme’ should be ‘program’.
2. Line 164, ‘Mesoscalle’ should be ‘Mesoscale’.
3. Line 265, ‘view’ should be ‘few’.
4. Line 304, ‘frequenlty’ should be ‘frequently’.
5. Line 436, ‘approx.’ can just be ‘approximately’.

References:

M. Serra, P. Sathe, I. Rypina, A. Kirincich, S.D. Ross, P. Lermusiaux, A. Allen, T. Peacock, G. Haller, Search and rescue at sea aided by hidden flow structures, Nature Communications, 11, (2020)

J. J. Early, R. M. Samelson, and D. B. Chelton, The evolution and propagation of quasigeostrophic ocean eddies, Journal of Physical Oceanography, 41, 1535 (2011).

G. Froyland, C. Horenkamp, V. Rossi, and E. van Sebille, Studying an Agulhas ring’s long-term pathway and decay with finite-time coherent sets, Chaos 25, 083119 (2015).

M.C. Denes, G. Froyland, S.R. Keating, Persistence and material coherence of a mesoscale ocean eddy, Physical Review Fluids 7, 034501 (2022).