Publisher's note: this comment was edited on 4 November 2026. The following text is not identical to the original comment, but the adjustments were minor without effect on the scientific meaning.

Review of "Physical connectivity and Lagrangian transport patterns in the Bay of Biscay across 30 years: by Steiger et al

This is a very interesting and carefully-written manuscript that I think could fit well within Ocean Science.

We thank the reviewer for the constructive and useful comments. We have carefully implemented the suggestions and changes into the manuscript and reply to each of the comments below in orange.

My major points are

1. While there is some reflection about more physics-informed clustering of the different subregions in the Discussion section, the definitions of these subregions in section 2.1 seem rather ad-hoc and top-down. It would be good to consider or flag more carefully how sensitive the results are to the definitions of the subregions. And perhaps also mention alternative ways to define the subregions earlier on. The work by Vincent Rossi and Enrico Ser-Giacomi in the Mediterranean comes to mind, for physics-based clustering.

We thank the reviewer for this suggestion and the references that seems highly relevant to our work. We agree that the selected subregions are rather ad-hoc and based on major hydrographic and bathymetric features.

The method used by Rossi et al based on Lagrangian transport networks and community detection seems elegant and could be a good idea for a future study. We would like to point out that the applications are slightly different. While their methods is well suited to identify transport barriers and coherent transport structures (which is actually an interesting alternative to FSLE and FDLD that we have used in our group at AZTI), our objective is to quantify and compare connectivity among predefined fixed regions of the Bay of Biscay over a 30-year period that facilitates temporal comparisons and interpretation of connectivity patterns and which we believe is more difficult with the evolving features.

We have revised Section 2.1 to clarify the rationale for this choice and added a discussion on alternative approaches based on the suggested study, which define regions directly from connectivity patterns. We also acknowledge the sensitivity of our results to the chosen subregions.

"The absolute values of connectivity are expected to vary with the definition of the

subregions, particularly for exchanges involving neighboring regions and boundary zones where moderate changes in boundary placement may reassign particles to different source or destination areas. Nevertheless, because the adopted boundaries largely follow major physiographic features of the BoB, the dominant basin-scale connectivity patterns identified here are expected to be robust. In particular, changes in connectivity between the French shelf and offshore regions largely reflect zonal (west–east) shifts, whereas connectivity associated with the Spanish shelf and slope is influenced by a broader spatial extent that includes zonal and meridional shifts, resulting in more complex, non-zonal transport pathways. Alternative approaches define regions directly from transport patterns by coupling Lagrangian modelling with network-theory methods (Newman, 2010), allowing the identification of dynamically coherent provinces and transport barriers (e.g. Rossi et al., 2014). Such methods provide a more dynamic representation of connectivity but often yield regions that vary in space and time, making long-term comparisons more difficult. To assess connectivity over a 30-year period, fixed physiographic regions provide a consistent and interpretable spatial framework." (ll. 126-137)

We further added this approach to obtain hydrodynamic provinces presented by Rossi et al (2014) in the discussion:
"We nevertheless adopted physically meaningful subareas based on the major hydrographic and physiographic domains of the BoB rather than subdivisions of equal areas or dynamic regions based on a transport network such as presented in Rossi et al. (2014), as our objective was to investigate long-term comparison of connectivity between oceanographically distinct environments. Likewise, we deliberately avoided defining subareas from community maps in order to keep the framework applicable to a broad range of tracers and management questions. Future studies could complement this approach by defining regions based on connectivity communities or Lagrangian transport coupled to network theory (Rossi et al., 2014) and comparing the resulting connectivity patterns with those obtained from the physically based subdivision used here." (ll. 473-479)

2. I find the definition of the ODI (lines 169-174) very difficult to follow. Would it help to add an equation here too, like in the Connectivity matrix? Or otherwise add more words to explain it more carefully?

Thanks for pointing out that the definition was not very clear. We reformulate the description as follows to make it clearer.

"To quantify the diversity of particle origins within the BoB, we define an ODI as the number of distinct source regions represented by particles occupying a given grid cell. For this, we first divide the seeding area into a regular grid defined by 100

equally spaced cells in longitude and latitude (about 0.12º × 0.12º). Then, we count how many unique particle origins exist within each of these grid cells, i.e. from how many different subregions the particles within the cell come from. A monthly ODI is calculated for each monthly simulation, considering all particles seeded within each grid cell across the seven seeding events per month. To obtain a climatological value, we average the monthly ODI over all available years. The ODI is calculated separately for each of the three integration times." (203-209)

3. I find the argument to use the shelf instead of the slope "due to the stronger signal in connectivity and its trend" rather weak. This suggests you look for maximal result (cherrypicking?), instead of an oceanographically-relevant result. Could you phrase this motivation it terms of oceanographic requirements, instead of the outcomes from the data?

We thank the reviewer for this comment and it is true that this does not reflect the actual reasoning. We have now reformulated this sentence to clarify our choice of using the shelf and removed the statement that we are using the northward extent of Spanish shelf waters as a proxy for the strength of the IPC, as this may lead to confusion given that the IPC is a slope current.

It is now formulated as:
"We calculate the northward penetration of Spanish shelf water (Spanish shelf) onto the French shelf to quantify the interannual variability and potential trend of the poleward transport and penetration of Spanish shelf waters onto the French shelf." (ll. 222-224)

4. The figures could all use a legend for the colours, so that readers don't have to flip scroll back to Figure 1

Okay, we added the legends for the colours in all figures.

Comentado [NS1]: Still TBD

Other minor points

1. Line 8-11: Also briefly mention the relevance of the Bay of Biscay in the abstract?

Thanks, this was a good point. We added one sentence in the abstract about the relevance of the Bay of Biscay:
"The Bay of Biscay is an ecologically important and oceanographically interesting region, where transport pathways are strongly influenced by the interplay of large-scale circulation and mesoscale processes." (ll. 8-10)

2. Line 31 and other places: what do [NS5.1] and similar words refer to?

Apologies for the editing error. These are leftovers from comments that have not been removed. We have removed them from the manuscript.

3. In the caption of figure, perhaps make clearer that these are not three sets of simulations for the different integration lengths – but rather that the analysis uses different subsets of the full simulation data.

Okay.

We corrected the captions of the following figures:

Figure 6: … "Panels show subsets of the full backward simulations after 7 days (a, d, g, j), 30 days (b, e, h, k), and 90 days of integration time"

Figure 7: … "Panels show subsets of the full backward simulations after (a) 7 days, (b) 30 days, and (c) 90 days of integration time."

Figure 8: … "Seasonal cycle of connectivity between subareas for subsets of the full backward simulation after 7 days (a, d, g, j), 30 days (b, e, h, k), and 90 days (c, f, i, l) of integration time."

Figure 9: … "Panels show subsets of the full backward simulations after(a) 7 days, (b) 30 days, and (c) 90 days of integration time."

Figure 4, A4, A5: … "Colours correspond to the origin of particles extracted after 7 (30/90) days of backward integration."

We also made this clearer in the method section 2.3:
"From each 120-days simulation, we extract particle positions at integration times of 7, 30 and 90 days to reflect key physical and ecological timescales (Fig. 2). The integration time is defined relative to the release date of each particle, to analyze all trajectories together that were seeded throughout one seeding month and experience the same duration of backward simulation." (165-168)

4. Line 183: ij should be subscript here?

Yes, thanks for spotting this error. It is corrected now.

5. Line 191: what does "remaining robust" mean here? Be more specific?

Okay, we reformulated to "reducing the influence of extreme outliers."

6. In Figure 3, randomize the lines tso that some regions are not completely on top of others?

Thank you for this suggestion. We have randomized the lines in Figure 3 to only show 10 % of all lines and in the figures in the appendix A1-A3 to show only 5% of all trajectories. However, reducing it more, there would be very few lines left for a

few colours. In the web application that accompanies the paper, it is possible to only select specific regions to better see the trajectories of the particles.

7. Line 281: should this be “dashed” instead of “ligh grey”?

Yes this is correct. We corrected this here and in other figure captions.

8. Line 383: I don’t think I fully follow the argument here. Why would the surface flow be better constrained? Since waves and wind complicate surface flow, I would argue this is even poorer constrained than the sub-surface (geostrophic) flow

Thanks for the comment. Better constraint referred to that the product assimilates satellite products such as SST, which can help constraining surface fields in general. However, it is true that the reanalysis product does not use velocity products for assimilation and that surface flow is influenced by complex processes.

We therefore reformulated this sentence to:
“Moreover, extending the simulations to fully three-dimensional velocity fields would substantially increase computational costs while introducing additional uncertainties associated with vertical transport and turbulent mixing parameterizations.” (ll. 417-420)

Additionally, we added more information on the IBI model surface velocity product (that it includes tidal dynamics and free surface) and we add information on the validation of the surface velocity output: “Although the Copernicus Marine Quality Information Document (QUID) of the used IBI model (Copernicus Marine Service, 2025) reports that near-surface currents may exhibit relatively poor correlations with fixed mooring observations (RMS between 0.1 and 0.5 m s^{-1}), the model nevertheless reproduces the large-scale circulation patterns and surface-drifter climatology that are most relevant for regional transport applications.” (ll 423-426)

9. Line 521: I strongly suggest that you upload the code now – so that this can also be checked for the next iteration of reviews

Okay, we made the code publicly available under the following link: https://github.com/Fundacion-AZTI/steiger-2026-connectivity. This link will also be included in the data availability section.