Dear Editor,

We would like to express our gratitude for considering our paper. We have made revisions to the paper based on the feedback we received, primarily from Referee #3. Referee #3 requested the removal of the "Betweenness Centrality" section from the revised version of our paper. Consequently, we have eliminated this section in the revised paper. However, it is important to note that the other two reviewers have accepted the paper and agreed with the improvements we made in the previous revised version. Furthermore, we have provided a comprehensive response to Referee #3's comments regarding the betweenness section.

We would once again like to extend our thanks to the reviewers for their insightful and constructive feedback. We have made every effort to address all their concerns in our revisions, and we eagerly await positive feedback from you.

Yours sincerely,
Saeed Hariri

We thank Referee #2 for its important and helpful comments. The revised text has been improved using most of them. Here is our specific responses to these comments:

Comment: Please state clearly that the effective resolution of 1/9° of the high-resolution model only solves meso-scale processes (~1 km) because you used the grid degradation method described in Levy et al., 2012, and a Lagrangian method using an analytical calculation of streamlines on an Arakawa C Grid. Additionally, please discuss the significance of your results regarding connectivity studies that used oceangeneral circulation models with "higher" velocity field resolution and also Lagrangian method using spatial interpolation with a Runge-Kunta scheme (e.g. 1/16° in Ser-Giacomi et al., 2020 and Legrand et al., 2022, or 1/12° in Assis et al., 2022 and Krumhansl et al., 2023).

The HR velocity fields used in this study were computed on a 1/54° grid: with such a numerical resolution, the effective resolution is 1/9° (as explained Levy et al 2012). This led to a good resolution of the mesoscale dynamics and a partial resolution of the submesoscale dynamics, as also shown in Levy et al 2012.
In the other studies that you are mentioning, the indicated resolution is the grid resolution, not the effective resolution. As you can see, the grid resolution in those studies is less than the grid resolution used here, which implies that the effective resolution (not indicated by the authors) should also be less than our effective resolution. Than we should expect these studies to have less resolution than in our study, not higher resolution.
We conducted the Lagrangian experiments at the effective resolution and not at the full grid resolution, because it would have been computationally much more expensive, with no extra benefit.

Comment: The authors should simplify the message in the method section 2.2.5. The significance of betweenness should be moved to the discussion part. Additionally, the explanation about the transformation of probabilities of connection into distance metrics should be shortened, as it is mandatory when using the Dijkstra algorithm.

Comment: On the new Figure 10, it could be interesting to have the betweenness distribution for all 16 sites with the model resolution as a factor (i.e., three boxplots) in addition to Panel a). The network displayed in Panel b) is very hard to interpret. A solution could be to use transparency and/or a log scale on the distance.

Thank you for your comment; based on the suggestion from Referee #3, we had to remove the betweenness section from the new revised version of our paper.

We thank referee #3 for his important and helpful comments. The revised text has been improved to address most of them. Our specific responses to these comments are as follows:

Comment: Two of the main issues can in my opinion be addressed by removing the parts about betweenness centrality, since the definition of the network is vague and potentially flawed due to the use of different integration times. Moreover, with regards to the research question, I currently do not seethe merit of investigating betweenness centrality when the information in the flow field is reduced to only 16 nodes connected by varying integration times.

Reply to two main issues related to **network construction** and **betweenness centrality**:

We have **removed** the sections on **betweenness centrality**, as suggested by the referee, but it is necessary to add some additional information about the analysis of betweenness we developed in our study:

1. We used **a fixed time step of dt = 1 hour** for all simulations. This approach ensures that each simulation runs at a consistent pace, allowing for accurate comparisons and analysis of results.

However, **the initial deployment time** of the numerical particles in our simulations **varied**. This variation is consistent with the basic principles of Lagrangian studies, where particles are tracked from their initial positions and advected by the flow for the duration of the study. In our case, we set a maximum advection time of 5 years for the particles. It should also be taken into account that in this study we are addressing the **transit time** and **not the residence time** of the numerical particles.

2. Our connectivity analysis is based on the concept of **minimum transit time**. This means that when a digital particle, let us call it particle A, leaves site "i" and arrives at site "j", the first arrival time is recorded and used for our analysis. However, if particle A were to return to site "j" at a later time, that transit time would not be used for our analysis. (See: Jönsson, B., Watson, J.: The timescales of global surface-ocean connectivity, Nat Commun. 7, 11239, https://doi.org/10.1038/ncomms1123, 2016).

To calculate the transit time between sites "i" and "j", we take the average of the minimum arrival times of all numerical particles that traveled from site "i" to site "j". By using the minimum arrival time, we can ensure that the transit time is calculated based on the fastest possible route between the two sites.

3. The reviewer mentioned that "the authors compute Lagrangian trajectories, but they do not indicate how exactly the information from these trajectories is translated into a network" and also asked how transfer probabilities are computed? This is quite clear and we followed the approach described in our seminal paper for betweenness studies by Costa et al. (2017) (Costa A, Petrenko AA, Guizien K, Doglioli AM. On the calculation of betweenness centrality in marine connectivity studies using transfer probabilities, PLoS ONE, 12(12): e0189021, https://doi.org/10.1371/journal.pone.0189021, 2017). Specifically, transfer probability refers to the probability that a particle will move from site "i" to site "j". These probabilities (a_ij) were then used as weights in our network. However, because these probabilities (a_ij) tend to be very small, Costa et al. (2017) suggested taking the logarithmic inverse of the transfer probability a_ij, which we also applied in our study. This approach allowed us to better analyze and visualize the resulting network. But again, it should be noted that the time step for all simulations is fixed.

4. It is important to note that the distribution of sites in our study was carefully considered and based on a number of different analyses. The referee should consider, however, that we have two types of connectivity: a) one-way transport connectivity (i.e. movement of particles from coastal areas or river mouths to the open ocean or oceans, this is a way to study the movement of larvae or individuals from different marine populations) b) exchange connectivity which is the case of our study, and we are interested here in tracking the exchange of information between different sites distributed in the basin.

However, we would like to respond to the referee's comment about dividing the pool into smaller bins and calculating the interdependence values in each bin. This is completely contrary to the definition of betweenness centrality, since the latter attempts to identify nodes that play an important role in the exchange of information between different sites or parts of the basin.

5. We would like to clarify that we used the betweenness centrality approach. We did develop it. The mathematical definition and application is provided in the following references:

Ser-Giacomi, E., Ruggero Vasile, Emilio Hernández-García, and Cristóbal López.: Most probable paths in temporal weighted networks: An application to ocean transport, Phys. Rev. E 92, 012818, https://doi.org/10.1103/PhysRevE.92.012818, 2015.

Lindner, M., Donner, R.V.: Spatio-temporal organization of dynamics in a two-dimensional periodically driven vortex flow: A Lagrangian flow network perspective. Chaos 27.3 , https://doi.org/10.1063/1.4975126, 2017.

Reply to Comment about Open Science:

We have added this paragraph as a

Data availability

Major parts of the data and codes used in this study are available upon request by contacting the corresponding author at saeed.hariri@io-warnemuende.de. Some sample data and parts of Lagrangian tools are accessible at https://doi.org/10.5281/zenodo.7954707. We encourage the use and sharing of our data and code for further research and scientific advancement. Please note that access to the codes may be subject to restrictions due to privacy or confidentiality concerns.

Reply to the other comments

Comment: In my previous review, I asked the authors to briefly discuss whether parameterizing the missing dispersion in the coarse-resolution simulations may remedy the issue of the dispersion being too low, leading to longer transit times (see comment 3 from initial review). This is still missing from the discussion.

We added this paragraph to discussion part of the paper

In particular, in coarse resolution simulations, the dispersion of particles is degraded. This results in longer transit times. It also limits the connection between water particles at different depths. A possible solution to overcome this problem when integrating Lagrangian trajectories using the velocity calculated in coarse resolution simulations is to parameterize the missing dispersion. Some methods have been proposed in the literature. The simplest parameterization consists in adding a random walk to the successive position of each particle, which is compatible with an advection-diffusion equation and is equivalent to a stochastic "Markovian" parameterization (Berloff & McWilliams, 2002). However, this stochastic parameterization does not reproduce adequately the small-scale ocean dynamics that involves consistency in advection (Klocker et al., 2012; Veneziani et al., 2004). Different Markov parameterizations of higher order have been proposed in an attempt to better reproduce the effect of the small-scale ocean dynamics (Berloff & McWilliams, 2002; Griffa, 1996; Rodean, 1996; Sawford, 1991). Other improved parameterizations include particle looping due to eddy coherence (Reynolds, 2002; Veneziani et al, 2004), as well as relative dispersion between different particles (Piterbarg, 2002). While these methods have been developed and applied to horizontal flows, recent developments include an isopycnal Markov-0 (Spivakovskaya et al, 2007) or shear-dependent formulation (Le Sommer, 2011) and, more recently, an isoneutral Markov-1 formulation (Reijnders et al., 2022). The latter appears to better mimic the coherent behavior of the 3D ocean dispersion at small scales. It would be interesting in future work to evaluate how such methods, applied in a Lagrangian framework, might improve the results we obtained with a coarse resolution field.

Comment, L66: “relatively simple”: here the authors minimize the contribution of previous sophisticated methods. For example, the ‘hydrodynamic provinces’ approach in Rossi et al. 2014 is, in my opinion, more sophisticated than computing betweenness centrality and transit times. I suggest removing these two words, to stay neutral.

Thank you. We changed the sentence. Line 65.

Comment, L111: “the vertical velocity is one to two orders of magnitude smaller”: I don’t see this from the image. Please include the standard deviation in HR and CR in order to quantify this.

We removed this sentence from the end of paragraph.

Comment, L127-128: Why is the integration time varying? For constructing a Lagrangian flow network, it is important that integration times are all the same. Otherwise, one introduces a bias into the connectivity matrix that favors some connections over others. Connectivity should be defined with respect to a certain, fixed, timescale (see earlier comment about network definition).

It is important to note that in our study, the Lagrangian time step was set for all simulations to dt= 1 hour (**dx=U.dt**). In addition, we did not use a variable time step in our analysis. However, we chose to deploy the numerical particles at different initial times and the particles continue their motion for the rest of the period of the 5-years long simulation. This is an arbitrary choice, that provide more robustness in terms of the variable initial conditions. As mentioned earlier, we based our connectivity time on the minimum transit time. For example, if particle A started at site "i" and arrived at site "j", we recorded the **first arrival time**. However, if this particle continued to move and returned to site "j" after a certain amount of time, we did not include this time in our connectivity time estimates. This approach has been well established in previous studies such as Jönsson, B., Watson (2016), as cited in our paper. Therefore, deploying particles together at the same initial time for connectivity analysis is not correct and is not necessary.

Comment, L216: It is unclear to me what an improbable trajectory would be. Please elaborate.

We removed the sentence.

Comment, L496-500: This entire paragraph seems redundant. The bulk of this paragraph is in between brackets. Why? Which interdisciplinary methods are meant? I do not think ecology is always necessary for connectivity measures; it only is if ecological connectivity is studied (rather than, say, water mass connectivity).

Thank you for spotting this. We removed the paragraph.

Comment, L507: The 39% reduction: where does it come from? It's not mentioned previously. Is this computed using all site combinations, or only using specific sites as start and end locations?
L510: The 8.4% increase: again, where does it come from? Please show how this is computed. Is this computed using all site combinations, or only using specific sites as start and end locations?

These values are based on all site combinations; We simply divided the difference between the transit time (i.e. "(Transit timeHR3D)-(Transit timeCR3D)" or "(Transit timeHR3D) - (Transit timeHR2D)") by the transit timeHR3D.

Comment, Figure 5: The authors should elaborate on why the CR case is less smooth than HR (in 1 to 15 and 10 to 12)? I would expect HR includes coherent structures that can trap and release particles in batches, or form blocking patterns, whereas I would instead expect these features to be smoothed out in CR, leading to a smoother spreading of travel times.

Thank you for your comment. The CR (coarse resolution) is less smooth than the HR due to the dispersion process. In the HR (high resolution) case, the simulated ocean dynamics disperses the particles more than in the CR case and the numerical particle concentration in the HR case is smoother.
In the HR (high resolution) case, the flow field is more turbulent and contains more small-scale dynamical structures than in the CR (coarse resolution) case. These small-scale features can trap and release particles in batches or form blocking patterns, resulting in high particle concentrations in some regions. However, due to the chaotic nature of the flow field, these concentrations are not maintained and the particles are eventually dispersed throughout the domain, resulting in a smoother concentration distribution.
In contrast, the CR simulation has a smoother and more predictable flow field, resulting in a more uniform dispersion of particles and a less fluctuating concentration distribution. This may result in a less smooth concentration distribution than in the HR simulation.

Comment, Figure 10b: This figure is illegible. Please use the adjacency matrix representation of the network instead.

We removed the figure.

Comment, Figure 11b: indicates the differences, but it is not clear enough which quantity is subtracted from which. Please mention this.

Thank you. We changed it: CR3D-HR3D

Technical corrections

Line by line:

L61: “high resolution velocity fields: give a spatial scale
it was done Line 60

L65: “litterarure”: literature
it was done Line 65

L73: “relevant amount of transfers across a graph (a specific location in the domain) passes through”: please clarify this vague wording

this line was removed from the revised version.

519: Sabrina Speich should be abbreviated as SS instead of not abbreviated as SP

thank you, it was corrected.