We thank Referee #3 for its important and helpful comments. The revised text has been improved using most of them. Here is our specific responses to these comments:

Main issues

C1. The definition and interpretation of betweenness and betweenness centrality is wrong in several places in the manuscript. Betweenness is claimed to be used to construct a connectivity matrix, but betweenness is a scalar measure of the number of shortest paths between pairs of nodes that pass through a given node (as correctly mentioned on 212-213). It can be used to identify 'bottlenecks' in the flow (l134): regions through which a relatively large amount of transport occurs (Ser-Giacomi et al., 2021). However, on lines 100-101 it is described as the number of shortest paths between nodes, suggesting that it is a measure defined in matrix form between I and j where it is defined a scalar for each node i. In section 3.3.1, betweenness centrality is wrongly used as a measure of transport probability between nodes, whereas the transport probability should simply be defined from the amount of particles that travels from node i to j (see e.g. Froyland et al., 2014).

Moreover, the paper from Costa et al. (2017) is wrongly interpreted as giving a new definition of betweenness, which, according to the author's is different than that of Dijkstra (1959). Instead, Costa et al. use the textbook definition of betweenness centrality (see Newman 2010) and simply use a reweighting of the edges of the transition matrix that is used as the input graph that betweenness is used on. Dijkstra's algorithm is simply a shortest path computation algorithm, which can still be used, next to Brandes' algorithm for Betweenness computation (Brandes, 2001). So, the "Costa versus Dijkstra" distinction is wrong, but plays a quite central role in this paper.

Moreover, the concept of a 'betweenness matrix' in Figure 10 makes no sense, since betweenness centrality is defined per node, not between nodes. It is therefore unclear what these matrices represent, since it cannot be betweenness centrality. Perhaps transition matrices are really used instead, but then it is unclear which purpose the prior definition of betweenness centrality serves.

These misconceptions should be fully cleared up. This can be done by computing the correct betweenness values per station, which should be in interpreted as "how important is one station as a link between other stations?" and by computing transition matrices, which should be interpreted as "for a given station, what is the probability that it ends up at another station?".

R1. We thank the reviewer for raising these points. The section on betweenness centrality has been thoroughly revised and improved. The main objective of this paper is to assess the impact of the OGCM resolution and vertical turbulence on the analysis of connectivity in oceanic flows. In addition to this objective, the concept of betweenness was used as a way to investigate the connection between various stations or sites.

In addition, we have updated the results section by removing all figures related to betweenness centrality and introducing two new figures for a more intuitive and comprehensive explanation of the concept. The new figures provide detailed information on the betweenness values for each node, for a detailed and nuanced understanding of our results (whereas the previous figures were based on the edge betweenness definition).

In short, we add that betweenness centrality is a way to quantify the importance of a node in a network by measuring how many shortest paths between any two nodes in the network pass through that node. By calculating the betweenness of each node, we can identify the most important locations for water transport in the ocean. Therefore, by comparing the betweenness centrality obtained from different OGCM resolutions, we can simply assess how well the models represent the true connectivity patterns in the ocean, and identify areas for model improvement. (Please refer to Sections 2.2.5 and 3.3 Betweenness Centrality for more information)

C2. The manuscript uses an idealized two-gyre model representative of a subtropical and subpolar gyre system as found for instance in the North Atlantic. A qualitative interpretation of how the dynamics differ between the HR and CR cases is useful. However, the results section is very lengthy with quantitative descriptions of connectivity properties of different (links between) stations. Since these exact details bear no relevance to the real ocean, the results section can be shortened and sharpened, as only the qualitative results are relevant with respect to our oceans.

R2. We removed some unnecessary parts of the text especially by revising the introduction and methodology sections and moreover some parts of the result section.

C3. The authors compare a high-resolution flow field to a coarsened version of it. Then, particle trajectories are integrated on both flow fields. Naturally, particles in the HR case will experience dispersion on scales smaller than the 1-degree grid, which leads to a divergence of the trajectories. I invite the authors to make a remark about the role of this subgrid-scale dispersion and on whether it may be simulated.

R3. Please refer to Fig. 1 which present the effects of eddies and also vertical turbulence by comparing the HR and CR models. (Lines. 104~111)

C4. The authors refer to several studies that use Lagrangian PDFs, which usually are PDFs of particle velocities (e.g. Pope, 1985) or particle separation. Please make sure that the referenced papers discuss the type of PDFs used in this manuscript. This will sharpen the definition used. Perhaps using the name Transit Time PDF would already be clearer.

R4. It has been corrected. (Lines. 146~147; 166)

C5. The manuscript could benefit from a clearer definition of ‘connectivity’. It can help to often plainly talk about transit times or betweenness (if correctly used), as to avoid confusion between the different concepts.

R5. The correct definition of connectivity has been added in the revised text (please refer to lines 38-39). Note that in this work we evaluate connectivity in its most general definition of the exchange of particles between different sites instead of one-way transport.

C6. The introduction is unnecessarily lengthy and the discussion of specific papers from line 61-89 is not relevant for the methods and analysis in this paper. For example, the paragraph between 73-77 uses several sentences to mention a study that uses community detection, but community detection is not used in this paper. I see no reason to keep it, as examples of connectivity studies are already mentioned earlier.

R6. We revised almost all the main parts of the introduction; in addition, the literature review has been improved by adding more recent studies and references in the field of connectivity analysis (please refer to the Introduction section).

C7. The manuscript does not provide any specific hydrodynamic model configuration code, Lagrangian analysis configuration code, or analysis scripts, making it irreproducible. For example, it is unclear how the authors construct the graph/network on which betweenness metrics are computed. Readers would benefit from seeing the code, as the measures such as betweenness are heavily influenced by the way the network is constructed. This omission of code is not in line with the Open Science standards set by EGU journals. Please link to your NEMO model configuration code, Lagrangian simulation scripts, network generation code and analysis code.

R7. Sample codes have been added to the paper as supplementary information; these include the method of running the Lagrangian package and other details to be used for the simulation of numerical trajectories.

The NEMO code is available here: https://www.nemo-ocean.eu/

Line by line comments

- 17: The authors mention that the PDFs are not Gaussian, but this is not a prior hypothesis: there is no reason to assume a Gaussian structure. It is not relevant to mention this, unless it has specific interpretations, which are not given.
  R: This sentence has been removed from the abstract.

- 35: Connectivity is not just used in the context of species dispersal, but can also describe the exchange of water (properties) more generally, or that of plastic. See e.g. Froyland et al. (2014) or Ser-Giacomi et al. (2015).
  R: It has been added to the introduction section. (Lines 37-38)

- 50: The authors mention that using an advection-diffusion equation assumes uniformity in advection and diffusion coefficients. This is not an inherent assumption in using an advection-diffusion equation, but simply an assumption that is used in the studies mentioned. After all, one can study connectivity using tracers in an OGCM, where advection nor diffusion need to be uniform.
  R: This part has been removed from the revised text.

- 57: What is meant by an "ocean connection"?
  R: In Lagrangian analysis, the "ocean connection" refers to the study of how particles, such as plankton or pollutants, are transported and dispersed in the ocean. Note that this part has been removed from the introduction section.

- 59-60: "Population connectivity has mostly been studied using Lagrangian integration of surface ocean currents". This is what the authors also do. Currently, the sentence hints at the manuscript providing an alternative method, which it does not.
  R: In addition to the 2D connectivity analysis, we also conducted a thorough 3D connectivity analysis for the HR and CR cases. (Please refer to the aim of this study: lines 56~64), although this part has been removed from the introduction section and we are not addressing the case of marine populations directly here. There is no link between this hypothesis and the rest of the paper.

- 93: The concept of a connectivity time is ill-defined and not used by all of the aforementioned examples, such as Rossi et al. (2014). Please provide a precise definition.

  R: In the revised draft, this paragraph has been removed from the introduction, and part of it has been moved to the methodology section with corrections. (Section 2.2.3 Lagrangian indices); in general connectivity time in the ocean refers to the time it takes for water masses or particles to travel between two sites/ points in the ocean. This can be important for understanding the transport of nutrients, pollutants, larval marine organisms, and other materials in the ocean.

- 101: This is an example of an incorrect interpretation of betweenness.

  R: This part has been removed.

- 103: This implies that Dijkstra's algorithm is for betweenness computation, but Dijkstra (1959) simply concerns a shortest path algorithm.

  R: We corrected it. (Please see sections: 2.2.5 and 3.3 Betweenness centrality)

- 107-109: Mention what the aim is of constructing such a matrix. How will it benefit your analysis?

  R: We have improved this part and moved it to the methodology section with corrections. (Lines. 158-159)

- 132: "all spatial scales of the modelled velocity". Do other Lagrangian codes not integrate over all spatial scales and just some instead?

  R: It depends on the type of depth level used in the OGCM; some packages cannot do 3D integration on time-varying depth coordinates (as it is the case with ROMS/POM/GETM; where "Z" changes at each time step and location "z = z(i,j,k) 3D matrix"). Note that for the NEMO model we did not have this problem.

- 132-133: from "to better understand" is unnecessary.

  R: It has been removed from the sentence. (Line. 65)

- 171: Please mention over what integration time particles are integrated.

  R: It has been added to the text. (Line. 128)

- 181: It is unclear how often particles are released. Is each particle released at a different initial time-step, or is this done in batches (of which size)?

  R: It was mentioned in line 127 ; random initial time step (between the first day of the first year and the last day of the fourth year).

- 183: Are particles also released up to 150m deep if the mixed layer is shallower than that?

  R: Yes, particles are also released up to 150m deep.

- 187: The specification of the stations still seems arbitrary to me. How are they chosen exactly?

  R: We have expanded the discussion in the paper to justify our approach. There are two aspects to this question. The first is the sensitivity of the results to the exact location of the station, in the vicinity of the station. Since there might have been some ambiguity in the meaning of "station" which could have been understood as a single precise location, we now use the word "site" throughout the paper. By sites, we mean small circular regions of 1° radius. This radius is an upper bound on the largest size of mesoscale eddies. By deploying 100 000 particles at each site, spread at different locations within the site (both on the horizontal and on the vertical), and also at different times, we provide reliable statistical estimates at the scale of each site. The second aspect concerns how the different parts of the model domain are connected with one another. To address this, we considered 16 sites, located in key areas of the model domain, but indeed with a degree of arbitrariness in their exact position. There are 4 key areas in the domain, which are the subpolar gyre, the subtropical gyre, the jets, and the less turbulent regions between the jets. To reduce the arbitrariness of the exact position of the sites, we have positioned several sites in each of the key regions. This is now better explained in the text.

- 193: "5 stations were used". Which?

  R: 1, 3, 8, 15, and 16, information added to the text. (Lines. 141-142)

- 194: "Note that stations 1, 3, 5, 8, 12, and 15 are important". Please explain why.

  R: We have deleted this sentence as it was no longer necessary.

- 198: Taylor (1921) is seminal to the theory of eddy diffusion, but unrelated to Lagrangian PDFs.

  R: The reference has been removed

- 208: Please define the meaning of the 'sample space variable'.

  R: The simplified version of the formula, along with additional information, was added in the revised version of the paper. (Please see **eq. 1**, section 2.2.4 Lagrangian PDF)

- 214: The authors give a textbook definition of betweenness, which was not defined by Costa et al. (2017).

  R: It has been corrected. (please see eq. 2 Lines 204, 205 and 206)

- 216: sigma is the sum of shortest paths, not just the shortest path.

  R: It has been corrected. (Line. 206)

- 220-221: Costa et al. (2017) only proposed redefining the weights of the Lagrangian transition matrix. Furthermore, please explain what aij is, what dij is, and how these are used to compute betweenness in eq (2).

  R: This section has been completely revised. Please see section "2.2.5 Betweenness centrality".

- 224: This comparison is only in the supporting information. I suggest to either remove this sentence or to move the comparison to the main text.

  R: It has been removed. Please see section "2.2.5 Betweenness".

- 226-250: Since the paper is mainly about a comparison between HR and CR, the section where transit times are just reported for HR seems irrelevant to me, especially since this quantitative assessment cannot be translated to the real ocean. Instead, qualitative assessments provide a more powerful analysis as these likely commute with the real ocean.

  R: This part of the paper presents the application of Lagrangian PDF in connectivity analysis, as also mentioned by referee #1. Although it presents the results related to the HR model (as our reference model), it can help the readers to follow the dispersion of particles during different periods and as a way to learn more about the connection between different sites.

- 232-233: These conclusions cannot be drawn simply from Figure 4 alone, as this concerns only one release site.

  R: We agree with the reviewer as Fig. 4 shows the dispersion of the particles during different time periods. For this reason, we added other results as supplementary figures (Figure S1~S5).

- 237: “not shown” --> please show this

  R: It is shown as a supplementary figures in the revised version of the paper. (Line. 239)

- 244: “not shown” --> please show this.

  R: It is shown as a supplementary figure in the revised version of the paper. (Line. 245)

- 244-245: Do you have a hypothesis for why this behavior is so different from particles release in station one? After all, those particles cross the main jet too. Why are they not trapped in it?

  R: Particles are subjected to the same forces that drive the movement of water in the gyre. Over time, these particles can be transported by the gyre currents and accumulate in certain areas. This could be due to a number of factors, such as the speed and direction of the currents in these jets, and the interaction between the particles and the water masses in the gyre.

- 248-250: This seems to be at odds with what happens for the particles in station 1, which can cross the main jet.

  R: The results show that the particles deployed from station 2 are nearly six months behind those released from station 1 due to the different types of currents around the stations. This indicates a significant difference in the particle dispersion rate.

- 252-294: This section can be shortened. Details about the distributions are not relevant; only the comparison between the distributions among the CR and HR case are.

  R: We removed some unnecessary parts of the text especially by revising the introduction and methodology sections and moreover some parts of the result section.

- 253-254: I don’t think Gaussian shapes should be expected in any case. See Van Sebille (2011) or O’Malley (2021) for similar transit time PDFs.

  R: We removed this sentence.

- 285: specify “closer”

  R: We meant “similar” (Line. 285)

- 330-332: In the case that particles are assumed buoyant, the 2D assumption is still valid. Only for non-buoyant particles, a simplification using only surface currents may be problematic.

  R: We agree with the reviewer: the buoyancy of particles can influence the movement of water masses in the ocean, as particles that are more buoyant will be more affected by surface currents and wind-driven mixing, while particles that are less buoyant will be more influenced by deep ocean currents. This can lead to changes in the flow patterns that determine the connectivity between different regions, and can therefore affect the accuracy of connectivity models.

- 363: It is unclear how this figure is plotted. Is any smoothing used? Are all values statistically significant? Is there enough data in each region?

  R: To generate this type of figure, we divided the basin into very small bins (1 km^2) and calculated the mean arrival time based on the number of particles in the bin. Near the boundaries, we applied some smoothing using flat shading in MATLAB.

- 371: Please also list the longest transit times associated to those stations.

  R: It has been added (LineS 368-369)

- 371-373: The authors mention that for the mean arrival depth for the shortest and the longest arrival time differ by about 65 meters. Is this result generalizable? I.e. are short arrival times usually associated with shallower depths? If this is not a generalizable result, it can be left out, since it would be anecdotal.

  R: It is more reasonable to have a shorter transit time at shallower depths and this result (difference of about 65 meters) is for one of the sample stations.

- 373-375: Is this a general result or is it anecdotal for this case?

  R: As a sample case it is valid for this station.

- 403: The authors mention that graph theory is used to define hydrodynamic provinces, but this concept (Rossi et al., 2014) is not actually used in this paper.

  R: We removed the reference and this part has been moved to the methodology section as also suggested by Referee #1. Please see section "2.2.5 Betweenness centrality".

- 407-408: Betweenness centrality is not at all a measure of transfer probabilities between two stations. That measure should simply be the amount of particles traveling between stations over time.

  R: We corrected the results and descriptions related to the betweenness section. Please see sections 2.2.5 and 3.3.

- 412: Costa et al. (2014) do not have a special definition of betweenness.

  R: We made the correction. Please see sections 2.2.5 and 3.3.

- 414-416: This is not a different type of betweenness, but a different type of graph used to compute it.

  R: We made the correction. Please see sections 2.2.5 and 3.3.

- 422: a_ij and log(1/a_ij) are not distances but weights.

  R: We made the correction. Please see sections 2.2.5 and 3.3.

- 435-437: Please show this claim.

  R: We removed this part based on the new results obtained for betweenness.

- 443: Renaming "connectivity matrix" to "transit time matrix" avoids confusion.

  R: It has been done. (line. 414)

- 465: "High connectivity" and "betweenness" are not the same

  R: This part has been removed from the revised version due to the new results provided for betweenness.

- 485-492: This section could use a stronger conclusion drawn. Currently the conclusion is simply that are differences in transit time between the two runs, but it remains unexplained what the precise reason for this is.

  R: We added more information. (Line. 452-454)

- 490: Betweenness is not a ‘rate of connections’.

  R: It has been corrected. Please see section 3.3.

- 495-496: The authors wrongly claim novelty here about using HR flow fields to describe connectivity patterns in a large-scale basin. Rossi et al. (2014) do the same for the Mediterranean and Reijnders et al. (2021) for the Arctic, which are both not idealized.

  R: We mentioned it because it is new for our study basin (northern Atlantic), and it is not the first one but one of the first ones.

- 503: See previous comment about Taylor not introducing Lagrangian PDFs.

  R: It has been corrected.

- 504: The authors mention that the PDFs are not Gaussian, but this is not a prior hypothesis: there is no reason to assume a Gaussian structure. It is not relevant to mention this, unless it has specific interpretations, which are not given.

  R: It has been removed.

- 511: Please qualitatively describe the differences and draw a conclusion from it.

R: Some transit time values have been added to have a better overview. Furthermore, we made an improvements on conclusion section. (Lines. 506-513)

- 533: Please mention the open and unsolved questions. These are currently not mentioned.

  R: It has been added to the text. (Line.496-500).

- Figure 3a: indicate the release location

  R: It has been added.

- Figure S2 is not referenced in the main text.

  R: It has been added to the revised paper as Fig. S6. (Line. 439)

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Minor/technical comments:

- 51: change “not necessarily verified in” into “unrepresentative of”. The statement is currently too weak.

  R: This sentence has been removed from the introduction.

- 61: “is” --> “has become”

  R: It has been changed. (Line. 34)

- 64-65: Please rewrite the sentence starting with “Based”. It is currently not a correct sentence.

  R: We removed this sentence to shorten the introduction.

- 91: “graph theory” --> “Community detection using graph theory”

  R: We removed this part from the introduction.

109: remove an unnecessary period.

R: It was removed.

146: “lower” --> “less”

R: it was done.

- 184: you cannot perform a property. Could you specify what is meant?

  R: This has been rewritten “analyzed” (Line. 130)

- 230: Higher than what?

  R: Higher than the concentration in the other parts of the basin. Text replaced by “larger”. (Line. 232)

- 336: “the deepest distance” --> “deeper”. Deepest would suggest the deepest possible depth.

  R: It has been corrected. (Line. 335)

- 355: “Mainly” what?

  R: It has been corrected for clarity. (Line. 354)

- 493: This is not a full sentence.

  R: We revised this sentence. (Line. 456)

- In general: The usage of ‘coarse resolution’ is well-chosen and more accurate than ‘low resolution’, but should be mirrored by ‘fine resolution’ rather than ‘high resolution’.

- Figure 3b: Either use ‘modulus of the annual mean velocity’ or ‘annual mean speed’

  R: It has been changed.