

Marine megavertebrate migrations connect the global ocean

Corresponding Author: Dr Lily Bentley

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The work by Bentley et al. is an excellent step forward in the use of animal movement data to understand how migratory species connect sites across the globe. The work that the authors present is an outstanding task that sets up the scene nicely to better understand how marine ecosystems are inter-connected through large marine vertebrates movement, as well as to better design management strategies across countries and their jurisdictions.

The dataset and the web interface available at MiCO have a huge potential for large-scale efforts and collaborations focused on the interconnectivity of regions of the global ocean.

I commend the authors for their effort and for producing a well-written and robust manuscript. I have added a few comments and questions that I had while reading the paper (see attached PDF), but in general, I think this manuscript should be accepted for publication.

Looking forward to seeing this published

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)

General comments:

The manuscript presents a platform to synthesise information on marine migratory connectivity and inform marine conservation, and reveals some of the first results of the data compilation. It is well written and structured, and the platform is very timely and of high value to help filling the gap between marine science and conservation policy. I have however a few main comments, mostly related to the clarification of some methodological steps and the interpretation of the results.

Regarding the methods, some key points are presented very vaguely, or not mentioned at all; the methods section is focused on some detailed procedures that could go to supplementary material, while some main points are not explained. First, it should be clarified that the study is describing geographical links, but not quantifying the strength of such links (sensu Webster et al. 2002; TREE; [https://doi.org/10.1016/S0169-5347\(01\)02380-1](https://doi.org/10.1016/S0169-5347(01)02380-1)). The final sentence of the abstract can be misleading, as this study does not quantify the connectivity per se; it provides a first estimate of the scale and scope of the geographical links. I'd suggest changing to "We present a first estimate of the previously unquantified (minimum) scale and extent of migratory connectivity across the world's oceans."

Another major point is the lack of definition of what a "site" means; it should be clarified how were the "important sites" identified. In Kot et al, a site is defined as "start and end points for routes", but that is a very vague definition (it would exclude, e.g. the stopover sites used by many species during their migratory journeys). In many cases, the start and end points can be simply the tagging location and the location of the last position due to battery failure, rather sites than are more intensively used by the animals; in such cases, the results would be driven by sampling procedures, rather than the ecology of the species. This can be particularly problematic if the main aim is to inform conservation management. The authors should provide more details in the methods section (or in supplementary material) about the identification of these sites, and show that the sites that were included in the analyses are, in fact, a sample of the important sites for the species – e.g. spawning/ breeding sites, foraging sites, stopover sites (even if currently incomplete and biased towards some life history

stages).

Regarding the results, I think that it should be stated more clearly that what is being presented here is the platform and its (very high) potential to inform marine conservation, but that currently the results are extremely biased towards well-studied regions – and, thus, they should be taken very carefully when considering marine policies; their current value is to inform priorities for future studies, but not necessarily yet priorities for the conservation of most species. On this last point, it would be interesting to estimate the comprehensiveness of the dataset per species/regions.

Minor comments:

Lines 89-90: If the study includes also within-season movements, that means that it is not only about migratory connectivity, but movement connectivity more broadly. Some central place foragers can travel hundreds of kilometres when commuting between breeding and foraging sites (often larger distances than some migrations); why not removing these movements from the dataset? Or, at least, an estimate of the proportion of within-season connections in the entire dataset should be provided;

Lines 98: It is not clear how the “network models” were built (that is not mentioned in the methods sections); at least a basic explanation should be given, as this is important to understand the tool and the results;

Lines 102-109: This paragraph is repeating information that was provided in the previous one;

Lines 111-116: See my main comment regarding the interpretation of the results; the high number of connections in USA and France are very likely linked to the sampling bias; to be able to interpret the results obtained, it would be needed a sensitivity analysis to estimate how representative is the dataset; reference to Fig 1 in line 116 should be removed, as fig 1 only shows results of ectotherms;

Figure 2: These figures are a great representation of the results, but it would be important to provide an estimate of the representativeness of the datasets of each group (e.g. using IUCN range maps); note that a label is missing in the figure of the turtle (“Central America”);

Line 140 (and also Line 261): List of species – CMS list is not comprehensive regarding marine migrants; if the analyses only include CMS listed species, it should be clarified that the migratory links of many marine species were not considered here;

Lines 140-151: See main comment regarding the interpretation of the results; without providing an estimate of the comprehensiveness of the dataset, it is premature to take any conclusions linked to the ecology of the species, or the threats affecting them; if the global distribution of sea turtles is restricted to tropical and temperate areas, it would be relevant to look to gaps within their range (and there’s no point in comparing it with the seabirds, as we already know that have a much larger distribution); regarding the seabirds, note that temperate/polar species tend to be more migratory than tropical ones (again, it is fundamental to know the representativeness of the dataset to draw any conclusion); reference to the Great Barrier Reef: are most of the seabird species breeding there migratory?;

Line 256: The MICO system (...) summarizes INFORMATION ABOUT marine migration (does not “summarizes marine migration”); in the following sentence, it should be “connectivity generated by...”;

Lines 265-270: See main comments regarding the definition of a site; it should be clarified here how these sites were identified at the first place from the literature review, as defining the geographic location/boundaries of sites is not a trivial task. The definition of sites can have a major impact on the results (and consequently on all the conclusions);

Lines 292-315: I found this explanation a bit confusing; what does “the largest sites” mean (lines 287, 288)? What’s the size of the buffer used to define metasites (line 294)? Were the data projected to a metric system? (distance metrics are given sometimes in km, others in degrees; it should be standardized or explained more clearly in the procedures);

Line 221: Fisheries are also a primary source of mortality for the other groups;

Line 216: The tool was not built to identify stakeholders, so I don’t think that is possible to “quickly identify” stakeholders with it; it can eventually facilitate the search;

(Remarks on code availability)

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

Dear authors,

I have read the new version of the manuscript and the responses to the first round of comments and I am happy with the responses and changes of this newer version. I think this manuscript should be accepted for publication

(Remarks on code availability)

While I didn't run the examples, I checked the GitHub site and check that the code and data were available

Reviewer #2

(Remarks to the Author)

The manuscript is now greatly improved, and I do not have any major comment to make. I suggest a few minor changes (line numbers correspond to the version without tracked changes):

Line 35: add the word "about", or "up to", before "80,000 km", as the migratory distance of the Arctic terns is not exactly 80,000 km

Line 56: reference number 17, you may want to cite instead the recently published article describing the Seabird Tracking database (Carneiro et al. 2024: <https://doi.org/10.1016/j.biocon.2024.110813>)

Lines 106-107; I suggest changing "other EEZs (including ABNJ)" to "other EEZs or to ABNJ".

Lines 288 and 315 and 316: Add a space between "500" and "km"

Figure 2: "Central America" label is still missing from the figure of the sea turtles

(Remarks on code availability)

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Response to Reviewers: NCOMMS-24-30449A, Bentley et al.

We thank the two anonymous reviewers for their attention to our manuscript, and below outline the changes we have made in response to their helpful comments.

Reviewer comments are in plain text, while *author responses are noted in italics*.

Reviewer #1 (Remarks to the Author):

The work by Bentley et al. is an excellent step forward in the use of animal movement data to understand how migratory species connect sites across the globe. The work that the authors present is an outstanding task that sets up the scene nicely to better understand how marine ecosystems are inter-connected through large marine vertebrates movement, as well as to better design management strategies across countries and their jurisdictions. The dataset and the web interface available at MiCO have a huge potential for large-scale efforts and collaborations focused on the interconnectivity of regions of the global ocean. I commend the authors for their effort and for producing a well-written and robust manuscript. I have added a few comments and questions that I had while reading the paper (see attached PDF), but in general, I think this manuscript should be accepted for publication.

Looking forward to seeing this published

Response: *We are glad the reviewer found the manuscript so useful, and we are grateful for their thoughtful comments.*

Minor comments extracted from PDF:

L 70: This transition is rather abrupt, as it is missing a statement of what the overarching goal of the study is. Whilst is implicit in the preceding text, I think it would be worth explicitly stating it,

Response: *Thank you for pointing this out. To smooth the transition between the two existing sentences (here for context) we have included additional text (here in **bold**) on new L 70: "While these taxonomic knowledge repositories have made steady headway in identifying critical areas for megafauna populations, the connectivity generated by movement between the locations has remained in the shadows. **We aim to summarise not only the known key use areas for a core set of marine migrants, but also the critical links between them.** Here we present the first freely accessible, cross-taxa, global dataset describing migratory connectivity in the ocean (MiCO, accessible at: www.mico.eco),..."*

Fig 1: Whilst this is a great illustration for the MiCO interphase, the figure is a bit too crowded and could be a bit confusing. Are there ways to make it a bit cleaner/easier-to-read?

Response: *We have simplified the figure to show one species only, rather than two species. We agree the image is a bit busy and do therefore direct the reader to the dynamic versions available online in the figure 1 caption: "View the dynamic versions of this and over 100 other network models at mico.eco/system/mapper."*

L 143: I wonder if there's also a bias associated to the different tagging/research effort between sites in the global north (and their territories) and the global south that might be worth to explore and it'd bound to help understand the under-representation in tropical areas

Response: *We agree with the reviewer, and include text on new L 160 which discusses this point: "The existence of biases in biological datasets is well-known^{27,35}, and in our system these are observed in both geography and taxonomy. Specifically, geographic biases*

mimicking the wealth and distribution of university researchers was evident (Fig. 3). Data on migratory connectivity has been collected from locations (mostly the USA, UK, and Europe) that broadly reflect the distribution of ecological research published worldwide³⁶.”

Fig 4: I find it hard to visualize a reason why tagged marine mammals are so biased to unknown, unless the majority of the data included comes from cetaceans (as in the case of pinnipeds this information is easily accessible). Or could it be that the information has been collected but has not been made available by the researchers?

Response: *The reviewer is entirely correct that the observed bias towards animals of unknown sex in the “marine mammal” section because almost all the marine mammal data are from cetaceans, we have added text on L 96-97 noting the study contains, “28 marine mammals (21 of these cetaceans)”. The full list of species is viewable on mico.eco/explore.*

L 267: Is there a reason for this? Is this due to data resolution? Animal behaviour?

Response: *We have included the following explanation for sizes of radii on new L 274-279: “A buffer was applied to each centroid to describe the area used. To focus the system on provision of regional to global scale information on connectivity, we used 1° as the smallest buffer for sites, and applied a 5° or 10° buffer to larger sites us to allow for differentiation among the size of areas identified in different studies and to increase the efficiency of reviewers assessing the literature. Using such a coarse resolution for sites also decreased the sensitivity of the sites to potential errors in deciding on the exact location of the centroid.”*

L 277: Can species richness be considered in the identification of metasites?

Response: *As metasites are calculated only within individual species, in this work we do not consider species richness when identifying metasites. However, we agree with the reviewer that this is an interesting angle and have a PhD candidate within our group currently considering how best to synthesise multi-species sites, potentially including species richness as a metric.*

Reviewer #2 (Remarks to the Author):

General comments:

The manuscript presents a platform to synthesise information on marine migratory connectivity and inform marine conservation, and reveals some of the first results of the data compilation. It is well written and structured, and the platform is very timely and of high value to help filling the gap between marine science and conservation policy. I have however a few main comments, mostly related to the clarification of some methodological steps and the interpretation of the results.

Response: *We’re gratified to hear that the reviewer found value in the study and the platform, and we thank them for their constructive comments.*

Regarding the methods, some key points are presented very vaguely, or not mentioned at all; the methods section is focused on some detailed procedures that could go to supplementary material, while some main points are not explained.

Response: *As recommended by the editor we have retained a reasonable amount of detail within the manuscript text, as there is no limit to word count for methods. In addition, we have endeavoured to clarify the specific methodological points as noted below in the reviewer’s comments. We hope that this improves the clarity of the work.*

First, it should be clarified that the study is describing geographical links, but not quantifying the strength of such links (sensu Webster et al. 2002; TREE; [https://doi.org/10.1016/S0169-5347\(01\)02380-1](https://doi.org/10.1016/S0169-5347(01)02380-1)). The final sentence of the abstract can be misleading, as this study does not quantify the connectivity per se; it provides a first estimate of the scale and scope of the geographical links. I'd suggest changing to "We present a first estimate of the previously unquantified (minimum) scale and extent of migratory connectivity across the world's oceans."

Response: We agree with the reviewer's point that we do not quantify the strength of connectivity in the manner of Webster et al. We have changed the final sentence of the Introduction (new L 74-75) (as we believe this is the sentence to which the above comment is addressed) to read: "We present a first estimate of the previously unquantified (minimum) scale and extent of migratory links across the world's oceans."

Another major point is the lack of definition of what a "site" means; it should be clarified how were the "important sites" identified. In Kot et al, a site is defined as "start and end points for routes", but that is a very vague definition (it would exclude, e.g. the stopover sites used by many species during their migratory journeys). In many cases, the start and end points can be simply the tagging location and the location of the last position due to battery failure, rather sites than are more intensively used by the animals; in such cases, the results would be driven by sampling procedures, rather than the ecology of the species. This can be particularly problematic if the main aim is to inform conservation management. The authors should provide more details in the methods section (or in supplementary material) about the identification of these sites, and show that the sites that were included in the analyses are, in fact, a sample of the important sites for the species – e.g. spawning/ breeding sites, foraging sites, stopover sites (even if currently incomplete and biased towards some life history stages).

Response: We agree that more clarity was needed in the description of how sites were defined. We have added the following text to new L 267-279: "Sites were identified based on the authors description of either processed data (26.7% of studies) or tracks (73.3% of studies), and were usually associated with a specific behaviour. If no behaviour was noted in the study, the site was listed as an "observation". Reviewers collecting data from the literature identified the centroid of the activity described. While identification of the exact centroid for a given activity was an inherently subjective process, training of reviewers to ensure standardisation a, as well as quality control of datasheets by the review manager, increase reproducibility and confidence in the results. A buffer was applied to each centroid to describe the area used. To focus the system on provision of regional to global scale information on connectivity, we used 1° as the smallest buffer for sites, and applied a 5° or 10° buffer to larger sites us to allow for differentiation among the size of areas identified in different studies and to increase the efficiency of reviewers assessing the literature. Using such a coarse resolution for sites also decreased the sensitivity of the sites to potential errors in deciding on the exact location of the centroid."

Regarding the results, I think that it should be stated more clearly that what is being presented here is the platform and its (very high) potential to inform marine conservation, but that currently the results are extremely biased towards well-studied regions – and, thus, they should be taken very carefully when considering marine policies; their current value is to inform priorities for future studies, but not necessarily yet priorities for the conservation of most species. On this last point, it would be interesting to estimate the comprehensiveness of the dataset per species/regions.

Response: We thank the reviewer for this comment and have text on L 71-72 to emphasise that this is a presentation of the platform itself: "Here we present a platform hosting the first freely accessible, cross-taxa, global dataset describing migratory species connectivity in the

ocean". We agree that estimating the comprehensiveness of the data per species would be an interesting study – we have PhD candidates who are taxa experts engaging with this question in e.g. marine turtles, but an assessment of the representativity of the entire dataset is outside of the scope of this work, which as the reviewer notes presented the system itself. Due to the challenges in producing comparable estimates that account for methodological differences in undertaking this assessment between e.g. colonial breeders (seabirds, pinnipeds) vs. cetaceans, this will be undertaken as part of follow-on projects. The platform does provide an indication of the range coverage of connectivity data, by allowing users to visualise migratory links alongside the best available underlying range maps for the species, and we have added text to indicate this on L 155-156: "The system includes the functionality to visualise migratory links overlaid upon the best available range maps for each species, as coarse indicator of these potential gaps."

Minor comments:

Lines 89-90: If the study includes also within-season movements, that means that it is not only about migratory connectivity, but movement connectivity more broadly. Some central place foragers can travel hundreds of kilometres when commuting between breeding and foraging sites (often larger distances than some migrations); why not removing these movements from the dataset? Or, at least, an estimate of the proportion of within-season connections in the entire dataset should be provided;

Response: We agree that the study expands the strictest definition of migratory connectivity (*sensu Webster et al.*) and considers connectivity caused by multiple kinds of large-scale movements. This text on new L 88 which we hope more clearly articulates the scope of the study: "Defining 'migration' itself is an object of extensive discussion²⁸. Here, we use it to mean movements of at least 100 km between key habitat areas, using the limit from the Global Registry of Marine Species²⁹. This captures both seasonal migratory movements between breeding and non-breeding areas³⁰, and within-season foraging movements for some central-place foragers." We chose not to remove movements that are within-season, because they still represent key links between important areas for highly-mobile species. We agree with the reviewer that the foraging movements of many central-place foragers exceed the distances travelled for other species on their seasonal migrations. For the purposes of this study, we have included all movements, as they represent critical connections between disparate areas.

Lines 98: It is not clear how the "network models" were built (that is not mentioned in the methods sections); at least a basic explanation should be given, as this is important to understand the tool and the results;

Response: We agree that a clearer explanation should have been provided. We have added the following text to the Methods section in new L 371-375: "Network models: Network models were generated in R using the *igraph* package, using metasites as nodes and the metaroutes as edges. The radius of a given metasite was used as the radius for the node, and the weight of the edges was based on the number of individuals associated with the metaroute. Nodes were coloured based on the activities/behaviours occurring in the metasite."

Lines 102-109: This paragraph is repeating information that was provided in the previous one;

Response: We have adjusted the text to avoid this repetition, and incorporated these lines into the paragraph above (beginning L 92) which now reads: "Kot et al. 2023 reviewed literature on ecological connectivity generated by the movements of 173 marine species across 4 taxonomic groups, and we expanded the dataset following that publication. Of the species reviewed, 109 had relevant geospatial information that described connectivity (i.e.,

at minimum, two identifiable locations/sites linked by the movement of at least one individual), including 40 seabirds (938 sites), 34 marine fish (1,198 sites), 28 marine mammals (21 of these being cetaceans) (1,430 sites) and 7 sea turtles (1,910 sites). After extracting information from these papers on site locations, routes (i.e., connections between sites; $n = 5,498$), and behaviours (e.g. breeding, wintering, foraging etc.), we synthesized these into summary metasites ($n = 1,787$) and created interactive connectivity networks for each species (e.g. Fig. 1) which were analysed to assess geographic and taxonomic trends in connectivity. Similar numbers of metasites were generated across taxonomic groups (fishes, 521; marine mammals, 401; sea turtles, 513; seabirds, 352 (Fig. 3)). Note that the “routes”, and therefore metaroutes, were recorded using start and end locations of migratory movements, and do not describe a specific path travelled. We found migratory links exist between almost every region in the world (Fig. 2).”

Lines 111-116: See my main comment regarding the interpretation of the results; the high number of connections in USA and France are very likely linked to the sampling bias; to be able to interpret the results obtained, it would be needed a sensitivity analysis to estimate how representative is the dataset; reference to Fig 1 in line 116 should be removed, as fig 1 only shows results of ectotherms;

Response: We completely agree and have added the following text to emphasise this is likely driven by sampling on L 109-110: “This is likely due to their high number of overseas territories and large investments in sampling effort, rather than any ecological driver.” We have also removed the erroneous reference to Fig. 1.

Figure 2: These figures are a great representation of the results, but it would be important to provide an estimate of the representativeness of the datasets of each group (e.g. using IUCN range maps); note that a label is missing in the figure of the turtle (“Central America?”);

Response: We completely agree that IUCN range maps are a good estimate for representativeness: the system has the capacity to visualise these along with the migratory links, new text on L 155 indicates this: “The system includes the functionality to visualise migratory links overlaid upon the best available range maps for each species, as coarse indicator of these potential gaps.” We have also added this range maps to the visualisation of new Figure 1 to illustrate the point and note in the figure caption that “The crimson hash indicates the IUCN range map for this species, highlighting range areas which lack connectivity information.” A single quantitative measure of representativeness is challenging across all taxonomic groups; however this is part of our next steps for work using this system.

Line 140 (and also Line 261): List of species – CMS list is not comprehensive regarding marine migrants; if the analyses only include CMS listed species, it should be clarified that the migratory links of many marine species were not considered here;

Response: We agree that CMS-listed and RFMO-managed species are a limited number of the total number of marine species that may be considered to migrate or undertake extensive movements. We have added text at the end of the Introduction (L 70) to affirm this study is for “a core set of marine migrants”, and also have added a sentence and amended another in the Methods (new L 261-265) to more fully explain this: “While our approach maximises the utility of the system for specific policy arenas, we acknowledge that species listed under CMS or managed by RFMOs represent a fraction of all migratory marine species. As such, and despite the breadth of work undertaken here, connectivity for more than two thirds of migratory marine megafauna remains unassessed.”

Lines 140-151: See main comment regarding the interpretation of the results; without providing an estimate of the comprehensiveness of the dataset, it is premature to take any

conclusions linked to the ecology of the species, or the threats affecting them; if the global distribution of sea turtles is restricted to tropical and temperate areas, it would be relevant to look to gaps within their range (and there's no point in comparing it with the seabirds, as we already know that have a much larger distribution); regarding the seabirds, note that temperate/polar species tend to be more migratory than tropical ones (again, it is fundamental to know the representativeness of the dataset to draw any conclusion); reference to the Great Barrier Reef: are most of the seabird species breeding there migratory?;

Response: We agree with the reviewer that because this dataset is unlikely to be representative, ecological conclusions should not be drawn from it. In this section we aimed to illustrate ways in which our data align (or do not) with well-established understanding of species' ecologies, propose potential reasons for this, and highlight where known gaps exist. We have rewritten the section to make this clearer on L 139-142: "For example, the clumped distribution of sea turtle research near the equator is expected, given the largely tropical distribution of this taxon³¹. On the other hand, our dataset displays a clear bias in seabird data collected in the Southern Ocean, despite seabirds being widely distributed, including in the tropics." The seabirds of the Great Barrier Reef are extremely understudied, though they do arrive and depart key breeding colonies for distant sites throughout the year they do not migrate north-south in the typical sense – to avoid confusion we have remove the reference to this system.

Line 256: The MICO system (...) summarizes INFORMATION ABOUT marine migration (does not "summarizes marine migration"); in the following sentence, it should be "connectivity generated by...";

Response: Yes, we agree completely and have rephrased the text on L 254-255 to read as suggested: "The MiCO system (<https://mico.eco/system>) is an online tool that collects, displays, and summarizes information about marine migrations to support policymakers." We have also fixed the typo in the following line, from "generate" to "generated" as noted. Thanks!

Lines 265-270: See main comments regarding the definition of a site; it should be clarified here how these sites were identified at the first place from the literature review, as defining the geographic location/boundaries of sites is not a trivial task. The definition of sites can have a major impact on the results (and consequently on all the conclusions);

Response: As above, we agree that more clarity was needed in the description of how sites were defined. We have added more information in the Methods (new L 267-279): "Sites were identified based on the authors' description of either processed data (26.7% of studies) or tracks (73.3% of studies), and were usually associated with a specific behaviour. If no behaviour was noted in the study, the site was listed as an "observation". Reviewers collecting data from the literature identified the centroid of the activity described. While identification of the exact centroid for a given activity was an inherently subjective process, training of reviewers to ensure standardisation, as well as quality control of datasheets by the review manager, increased reproducibility, and confidence in the results. A buffer was applied to each centroid to describe the area used. To focus the system on provision of regional to global scale information on connectivity, we used 1° as the smallest buffer for sites, and applied a 5° or 10° buffer to larger sites us to allow for differentiation among the size of areas identified in different studies and to increase the efficiency of reviewers assessing the literature. Using such a coarse resolution for sites also decreased the sensitivity of the sites to potential errors in deciding on the exact location of the centroid."

Lines 292-315: I found this explanation a bit confusing; what does “the largest sites” mean (lines 287, 288)? What’s the size of the buffer used to define metasites (line 294)? Were the data projected to a metric system? (distance metrics are given sometimes in km, others in degrees; it should be standardized or explained more clearly in the procedures);

Response: We apologise for the confusing description. It is fundamentally difficult to describe an iterative process with words and they are usually more easily explained through process diagrams (e.g., Fig S3). We have amended and added to the text on L 311-321 in hopes of providing more clarity. It now reads: “When aggregating sites together with 10° sites, it is possible that sites that were >1000 km away could be selected for inclusion. To ensure that large-scale connectivity between such sites was not lost in the aggregation process, routes and connections were projected to a meter-level Azimuthal Equidistant projection (centred at 0° N, 0° E) and any routes or connections >500km long were not included in the aggregation process for a given Lookup Site. That is to say, any Nearby Site that included a route or connection to a Lookup Site >500km away from it was removed from consideration for grouping (“Removed Site”) with the Lookup Site. To account for the removal of the site, any sites that were: a) greater than 1° from the Lookup Site centroid, and b) within the buffer of the Removed Site, and c) closer to the Removed Site centroid than the Lookup Site centroid were also removed so that they could later be aggregated with the Removed Site (see Fig S3 for the decision flowchart).” We have also added specific values to any mention of large or small sites.

Line 221: Fisheries are also a primary source of mortality for the other groups;

Response: We agree with the reviewer’s comment, and have rewritten the sentence as below, with additional citations: “(a primary source of mortality for turtles⁵⁰, sharks⁵¹, seabirds⁵², and marine mammals⁵³)” (New L 210)

Line 216: The tool was not built to identify stakeholders, so I don’t think that is possible to “quickly identify” stakeholders with it; it can eventually facilitate the search;

Response: We have rewritten this sentence to clarify the system outputs can facilitate government engagement with e.g. appropriate bodies governing potential stressors on new L 213-214, “Our networks can support interested parties to better understand with which Regional Fisheries Management Organisations (RFMOs) species from their jurisdiction are interacting, thus opening the door for engagement from non-fishing nations. ”

NCOMMS-24-30449A Response to Reviewers. *Author responses noted in italics.*

Reviewer #1 (Remarks to the Author):

Dear authors,

I have read the new version of the manuscript and the responses to the first round of comments and I am happy with the responses and changes of this newer version. I think this manuscript should be accepted for publication

We thank the reviewer for their thoughtful participation throughout the review process and for feedback which improved the manuscript.

Reviewer #1 (Remarks on code availability):

While I didn't run the examples, I checked the GitHub site and check that the code and data were available

Code and data are indeed available and additional information has been added as requested by the Author checklist.

Reviewer #2 (Remarks to the Author):

The manuscript is now greatly improved, and I do not have any major comment to make. I suggest a few minor changes (line numbers correspond to the version without tracked changes):

We are grateful for the reviewer's time and attention to detail as well as their comments which improved the manuscript.

Line 35: add the word "about", or "up to", before "80,000 km", as the migratory distance of the Arctic terns is not exactly 80,000 km

Amended as suggested.

Line 56: reference number 17, you may want to cite instead the recently published article describing the Seabird Tracking database (Carneiro et al. 2024: <https://doi.org/10.1016/j.biocon.2024.110813>)

Amended citation as suggested.

Lines 106-107; I suggest changing "other EEZs (including ABNJ)" to "other EEZs or to ABNJ".

Amended as suggested.

Lines 288 and 315 and 316: Add a space between "500" and "km"

Amended as suggested.

Figure 2: "Central America" label is still missing from the figure of the sea turtles

Amended in vectorised version, thanks for catching this.