Handling Editor Dr. Hunter comments:   
  
The manuscript by Allgayer et al. entitled ‘Assessing spatial patterns of within-stock connectivity provides novel insights for fisheries management’ considers both the dispersal and demography of metapopulations whilst incorporating spatial connectivity into regional fisheries management. The authors challenge the assumption that stocks are well-mixed within assessment regions, notably where dispersal capacity is limited, or where there are specific habitat requirements. Current uncertainty around the sustainable extraction of the chosen study species (North Sea sandeels) and the ecosystem implications thereof make this work both topical and timely, and I have no doubt the work will be of interest to readers of the Marine Functional Connectivity Theme Section and of the Journal more widely.  
  
Overall, the manuscript is well written, if a little lengthy in places, however there are several issues that the authors will need to address before I am able to accept the paper for publication. Firstly, concerning replicability of the work, neither the MerMADE software tool nor the data files used in this study are currently available at the link provided by the authors? – maybe it is their intention to make these available following publication? Secondly, both reviewers have recommended full citation of the MerMADE pre-print currently available on bioRxiv as a useful technical reference to the MerMADE model.   
  
The reviewers have also identified some inconsistencies in the terminology applied which need to be addressed. For example, concerning the removal of fishes from patches, the terms ‘depletion’ and ‘extinction’ appear to have been used interchangeably (the former appears more appropriate given that no patch depletion resulted in 0% population). Further clarity is also required with regards the use of the terms ‘in/out centrality’ and ‘origin/destination centrality’ (in this case the latter feels intuitively more descriptive) – the authors need to ensure that both these examples are applied consistently throughout the document including the figure legends and tables.  
  
In revising your manuscript, the two reviews attached provide excellent guidance which will result in significant improvements. Ideally, I would also like to see a reduction in overall word count, particularly where there is some repetition in the introduction and discussion sections. Hopefully the requested changes are easily achievable, and I would like to see a full rebuttal to each of the referee comments. I wish the authors well with their revisions and look forward to receiving the revised manuscript.  
  
  
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Editorial Office comments:   
  
Please address the following points (noted during the pre-screening of your manuscript at the time of first submission) in your revision:  
  
  
  
  
  
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Reviewer 1 report:  
  
Dear authors,   
  
Thank you for this very interesting article which is, in my view, both timely and important. This study provides a very important step towards this integration of connectivity into regional fisheries management, and highlights the potential pitfalls of the common stock assessment assumption that stocks are ‘well mixed’ within the assessment region for marine animals with low dispersal or habitat specific life-stages. Hence, this paper provides general insights into fisheries management, in addition to specific advances in our understanding of metapopulation dynamics of lesser sandeels in the study region.   
  
The paper is well written and the figures and tables are clear, and I believe that it would be of great interest to the readership of this journal. Furthermore, the authors present a practical modelling tool MerMADE which is a significant advance in terms of the ability to intregrate hydrodynamic landscapes into dispersal processes.   
  
I provide specific comments below, which should be relatively minor to address and which would improve the quality of the manuscript.   
  
Comment 1 – Software and data availability  
The software tool ‘MerMADE’ demonstrated in this study appears to provide a very useful tool for other researchers by providing a flexible method for integrating demography, and dispersal of marine species in a 3D hydrodynamic context. However, as a reviewer, I was unfortunately unable to access this software tool, and I could not find the software at the link provided by the author i.e. <https://github.com/rebekkaallgayer/MerMADE_software> or elsewhere online. This link does provide a user manual and further information on the software, but not the software itself. Perhaps, the author intends to supply this on publication? Making the software available and linking it from this paper would greatly increase both the reproducibility and citability of this work. For full reproducibility, the authors could also provide the data files used in this study with the software. Ideally, this software should be provided at a more permanent hosting repository than github (e.g. Zenodo, <https://zenodo.org/> which is also easy to push work to from github).   
As if stands, given my lack of access to the software, I can not repeat the analysis and am taking that part of the publication as valid on trust. I do however, understand the principles behind the model, and details of the model parameters provided in the main text, and I am happy with these. I recommend that the authors should also cite their software specific pre-print publication “MerMADE: Coupled biophysical, eco-evolutionary modelling for predicting population dynamics, movement and dispersal evolution in the marine environment” on bioRxiv in which readers will find useful technical details.   
  
  
Comment 2 – Terminology  
Overall, this paper is very well written, and relatively accessible given the complexity of the topic and the model presented. There are two points regarding terminology which I think may be confusing to readers and should be clarified and standardised throughout.   
a) Extinction vs Depletion  
As part of the model simulations individual patches are decreased by 95% to represent extreme disturbance or fishing events. This is referred to in the methods (and in other parts of the manuscript) as ‘extinction simulations’ or ‘extinction events’. In the discussion, this is referred to as a ‘depletion event’. It would be best to standard this to ‘depletion’ rather than ‘extinction’ throughout. Extinction intuitively implies a complete loss to 0% population.   
b) In out-centrality vs origin/destination centrality   
In my view the terms in-centrality, and out-centrality, are not clearly enough defined and may cause confusion to some readers.   
The clearest definition of these is given on lines – 238 to 241 – “Network analyses identified critical vertices with a high out-degree centrality, which measures how many patches each patch supplies with outgoing successfully settling juveniles, and high in-degree centrality, measuring the number of patches a patch is receiving juveniles from.”   
But this seems to contradict lines 99-103 “using patches as nodes and movement of successful dispersers as weighted edges, to calculate the in- and out-degree centrality measures – the amount of dispersers coming into and coming from a particular patch, respectively – to identify both important and vulnerable patches within the system”. From your figures the in-out centrality seems to be number of patches not number of dispersers? Please clarify this in the text.   
Also, relating to this, it seems like, you use the terms ‘origin-centrality and destination-centrality’ synonymously with ‘out-centrality and in-centrality’ ? e.g. Tables 2 & 3, and Figure 5 (but not Figure 6). Are these terms interchangeable or are they slightly different? As an ecologist I find the words origin and destination more intuitive than in/out, but either is fine. The important thing is to standardise the wording throughout the text tables, and figures and define it clearly. Line 242 states ‘We used these to identify important origin and destination patches, respectively.’ You could define the terms ‘origin-centrality and destination-centrality’ here if you intend to use them thereafter.   
  
Minor Comments below follow their order in the manuscript  
Introduction   
Introduction is clear and well written, but would benefit from further referencing of general statements particularly in paragraphs 2 & 3.   
  
Also, line 47-49 - could have a more fisheries focused reference for the ‘largest fishery in the North Sea’ statement – e.g. e.g. The latest ICES fishery overview for the north sea? - ICES Advice 2022 – <https://doi.org/10.17895/ices.advice.21641360> or   
Engelhard, G. H., Peck, M. A., Rindorf, A., C. Smout, S., van Deurs, M., Raab, K., ... & Dickey-Collas, M. (2014). Forage fish, their fisheries, and their predators: who drives whom?. ICES Journal of Marine Science, 71(1), 90-104.  
I would also change the sentence to ‘one of the largest’, as catches of herring and mackerel are now similar.   
Materials and methods  
2.1. Software   
A sentence or two stating how the software is an advance on alternatives such as RangeShifter would be useful, and a citation to your bioRxiv paper for further details.  
2.2 Seascape  
With regard to the hydrodynamic data – Lines 172 – 175 – you state  
‘We provided hydrodynamic data from March 2004, 2006, 2008, 2010, 2012, and 2014 as inputs to the model to simulate interannual variability in hydrodynamic conditions and take advantage of its dynamic seascape functionality. For simplicity’s sake, we then cycled this decade’s worth of data throughout the 50-year simulation run. ‘  
I didn’t understand why the data comes from 2 year time-steps, and how that is then repeated across 50 years, what happens in the in between years? Or is the model run on a two-year time step?   
Also, please say why March was used (matching dispersal timing I presume?)   
  
Lines 175-178 – ‘what do you mean by ‘significant differences’ here? Was this formally tested, if so, give the test name. If not, then say interannual differences in the connectivity matrix were small (maybe give the range of these).   
  
2.3 Parameterising the model  
Line 191 – add the word ‘eggs’ after 2477  
Line 231 – ‘over the course of a 70-day Pelagic Larval Duration (PLD) would equate to 97% mortality’   
I’m wondering whether this 97% pelagic mortality needs to be included in the Leslie matrix shownTable 1, between Stage 0 and settled juvenile there is a 1 implying full survival. I know the egg mortality is already incorporated by reducing the fecundity. But where is this pelagic pre-settlement mortality applied? If it’s not included in the Leslie matrix because it’s applied in a different part of the model, then I think a comment on this in the legend for table 1 would be useful.   
  
2.4. Local patch extinction   
See terminology comments above.   
Line 259-260 – In order to investigate repeated local extinction events, we increased the frequency to once every other year over a time-period of 50 years.   
This sentence is unclear I don't understand, maybe missing a word? Do you mean that you increased the frequency of the depletion events?   
  
Results  
Line 364 – 366 – Add ‘repeated’ before extinction in the second half of sentence for clarity.   
  
Discussion  
The discussion is generally well written, but I felt that the first two paragraphs could be condensed to a few sentences here to allow the discussion to focus more solidly on the implications of the results starting at discussion paragraph 3.   
In the discussion, it would be interesting to read some suggestions about how the conclusions of the model could be tested with other methods if the authors have opinions on this? In future, could genetics, otilith chemistry or tracking methods be used with model testing in mind? Would this give us an independent way to test model parameters or conclusions?   
Line 412 – add a ‘space’ after ‘dispersal’   
Line 498 – ‘de facto closed areas’ would be more precise than ‘de facto MPAs’  
Line 511 – 514 ‘Stock assessments, where the current state of the stock is evaluated by estimating population size and fishing mortality, are not likely to be affected by pre-settlement dispersal, unlike other processes (Cadrin 2020). ‘  
This statement is a little unclear to me in the context. Do you mean that the stock assessments do not incorporate factors such as pre-settlement dispersal (I agree), or that the assessments values (F and SSB) are not affected by pre-settlement dispersal (I’m not sure). i.e. in reality pre-settlement dispersal would affect SSB, but the stock assessment model doesn’t incorporate it explicitly.  
  
Tables –   
Table 1   
See comment above in methods about the 97% mortality in the larval pre-settlement phase and how it is represented in this table (column 1, cell 2).   
I suggest changing the column and row titles to ‘Adult Stage 1’, ‘Adult Stage 2’ and ‘Adult Stage 3’ to avoid reader confusion with ages.   
  
Tables 2 and 3  
See previous comment on origin/destination terminology  
Patch size in km2 would be more intuitive for the reader. (note: line 349 refers to this)  
  
  
Figures   
  
Figure 5   
See previous comment on origin/destination terminology  
  
Figure 6   
I’m not sure if this is needed in addition to figure 5, it could be removed if needed for journal space reasons.   
  
Table A1.   
How was the emigration probability defined? Should this be mentioned in the methods? Similarly, the additional settlement parameters? – can you refer the reader to your other work as a reference?   
  
  
  
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Reviewer 2 report:  
  
I enjoyed Allgayer et al's ms mostly because the authors considered a model with both dispersal and demography. As the authors state this is not often done, there are 100's of recent biophysical modelling papers focusing on dispersal/connectivity but most overlook reproduction and mortality. This work also illustrates nicely a metapopulation approach applied in a marine context, here sandeel dynamics in the North Sea, which is not common either.  
  
(1) The "metapopulation" aspect could actually be highlighted, more maybe by coming back to the "debate" on marine metapopulations in the early 2000's (e.g. Grimm et al. 2003 Marine metapopulations: a useful concept?; Kritzer and Sale 2004: Metapopulation ecology in the sea: from Levins’ model to marine ecology and fisheries science), either in the introduction or discussion, and discussing the use of a Leslie matrix here which is a bit different approach than historical metapopulation marine models to my knowledge (e.g. Hastings and Botsford 2006: Persistence of spatial populations depends on returning home depends).  
  
(2) My main issue with the ms as submitted is the description of the MerMADE model/software which points to a ms (Allgayer et al. 2022, line 115) on github (is that submitted to a journal?) that I found insufficient in particular because there is no information on demography into it (nor in Appendix Table A1, line 127). The information about demography comes much later (Table 1, line 189). I don't understand why this is so but it must be changed.  
  
Minor comments to consider:  
  
(3) line 132 at that stage it is not clear for a reader not familiar with sandeel in the North Sea why there is no (or limited) dispersal between stock assessment areas, maybe a global map showing these areas would help.  
  
(4) 142 I don't see patches in Fig 1, add the patches on the figure or remove the ref to the figure.  
  
(5) 156 why that number (21 millions)?  
  
(6) 174 why not at least use all years 2004-2014 but only some of these (2004, 6, 8, 10, 12, 14)?  
  
(7) 205 I guess the subpopulation could also decrease in size and go to 0 and not stabilize nor grow exponentially.  
  
(8) 228 We need information on swimming implemented in the model, not only speed but also direction.  
  
(9) 267 Fig 2 at that stage it is not clear if patch 26 for example is only the big patch represented or also the small ones around it because all patches have the same color. Please fix that issue.  
  
(10) 294 Fig 3 explain what "volume" is, number of individuals normalised by patch size?   
  
(11) Patches have different sizes (this becomes clear from Fig. 4) and some of the represented quantity will depend on size, which may be an issue, please reflect on this for volume Fig 3, self-recruitment Fig. 4, centrality Fig. 5, etc.   
  
(12) 354 one patch only goes extinct, right?  
  
(13) Figure 6 is not cited in the text, only in Table 2 legend, could be put in appendix. Also there is an issue at the end of the figure legend.  
  
(14) Figure 7 legend says 6 patches but there are only 5. The colors and/or quality of the figure prevent from seeing the curves well.  
  
(15) 398 and 412 missing space after/before the ref.