

Invited perspectives: Nature-based solutions to mitigate coastal flood risks – Optimizing success through knowledge co-production – Responses to reviewers

Dear Editor,

We would like to thank you and both reviewers for the mostly positive evaluation of our manuscript. As outlined below, we have addressed all of the reviewers' comments and made some changes to our manuscript (as shown in the tracked revised manuscript), which we feel have improved this paper. Apart from specifically addressing the specific reviewers' comments (see our replies below, in red), we have also made some minor clarifications within the text and added some new references to make this paper even more complementary.

To acknowledge contributions in developing the conceptual ideas discussed in this paper and their contribution as part of the revision process, we have also included a new co-author (Joshua Kiesel) in the list of authors. All co-authors have agreed to this addition, and the "Author contributions" section has been updated accordingly.

Kind regards,

Mark Schuerch

Reviewer #1

Overall, I found this to be an extremely well written and accessible piece – I enjoyed reading it, and thought that the authors brought together the complex aspects of saltmarsh management and issues around MR very well. The framework provided is a useful tool for supporting stakeholder and community engagement design, and clearly integrates different types of evidence, knowledge and value within the process. I am happy to recommend the paper for publication subject to minor revisions suggested below.

We thank the reviewer for these positive comments. We have revised the manuscript based on the suggestion below and based on the comments of reviewer #2.

Figure 1 mentions Regulated Tidal Exchanges but these have not yet been described in the text – the focus has been on MR. Suggest editing this so that the text and figure tie together.

As suggested in the below comment, the definition of different MR types has been moved to here (lines 35-38, revised ms). In line 40 (revised ms), we have also included a sentence on the occurrence of RTEs: "Amongst these schemes, 36 (14,522 ha) are Regulated Tidal Exchange schemes (RTEs)."

Line 155: this description of the different types of MR is very useful and I think would have been better placed earlier in the manuscript – it could then be used to address my comment above about Figure 1, and could be cross-referenced to at this point in the paper.

Revised as suggested, see reply to previous comment.

Line 171: suggest adding in some citations to these four lines (particularly line 172)– perhaps some of the work from the CoastWEB project by Merryn Thomas and Erin Roberts, or the RESILCOAST work by McKinley et al., would be useful here (e.g. Uses and management of saltmarshes: A global survey - ScienceDirect)

Suggested work by McKinley et al., 2020 has been included to support this statement. Additionally, the argument here has been strengthened by an additional sentence and two further references (lines

182-183, revised ms): “Consequently, effective MR design needs to be adapted to pre-defined targets to ensure restoration success (Wolters et al., 2005; Gourgue et al., 2022).”

Challenge 3: it may also be worth noting that there is limited research on public perceptions of saltmarshes generally – and that actually, there is a tendency for it to focus on MR projects, as limited as this is.

We have included two phrases in lines 187 and 200 (revised ms) and two references to McKinley et al., 2020 (a&b) in lines 180 and 200 (revised ms).

Line 180: “[...]; large MRs can only be implemented with community support ([McKinley et al., 2020a](#), [...].”

Line 187: “However, available peer-reviewed literature on community perception of MRs, and saltmarshes more widely, is very sparse.”

Line 200: “From the limited literature on this topic and our experience of working with practitioners, key reasons for public opposition to MR schemes include [...] limited understanding of the benefits of the new intertidal habitats, e.g. for coastal protection (...) and other ecosystem services ([McKinley et al., 2020b](#)).”

Figure 3 – I am slightly confused by this image. I like it, but find it difficult to ascertain the direction of travel/ relationships between the different segments. Maybe this isn’t the point but my brain automatically tried to work that out, and I doubt I would be the only one. Is there any way this could be slightly clarified to either include these aspects or changed so that a reader isn’t trying to assign process to the image (there would probably be a tendency to read it as a top-down process).

Figure 3 has been revised slightly to enhance readability and highlight the core elements of the co-production process as compared to the input needed for the iterative co-production process. A circle has been drawn around the figure to discourage the reader from reading it as a top-down process.

I also suggest deleting the ‘s’ so it reads ‘traditional academic knowledge production’ and also suggest changing ‘scientist’ to ‘researcher’ as this sort of process would perhaps need to bring in researchers who would not necessarily identify themselves as ‘scientists’ in the traditional sense of the word.

Revised as suggested.

Would it be ‘focus groups’ or could it be a range of methods of collaboration between different actors? Could ‘focus groups’ be replaced with ‘collaboration’ or ‘co-working’ or ‘co-design’ or something like that? Focus groups suggests a very specific social science methodology and I think this approach would probably vary in different contexts – there is also no mention of focus groups in the 6 steps.

Revised as suggested. The term ‘focus groups’ has been replaced by ‘collaboration’.

Reviewer #2

1 Does the paper address relevant scientific and/or technical questions within the scope of NHESS?

Response: Yes. The paper clearly complies with the criteria set out in your journal's "Aims and scope" explanation.

We thank the reviewer for this positive comment.

2 Does the paper present new data and/or novel concepts, ideas, tools, methods or results?

Response: Yes. The paper presents an effective review of managed realignment (Sections 2 to 4). Introduced by Section 5, Section 6 (244-298) sets out a stakeholder approach to managed realignment; this approach is to be commended though, as set out below (Question 18), is in need of attention.

We thank the reviewer for this positive comment. We will address the reviewer's concerns around the framing of our proposed stakeholder approach in our response to comment 4b and 7.

3 Are these up to international standards?

Response: No.

a) The opening explanatory sentence ("Scenario building... change mitigation", 249-250) reads like the beginning of a review of the literature. It would help if there were a brief explanation (perhaps a couple of sentences) that would link the introductory remarks given in Section 5 to the model's presentation in Section 6 by outlining the sources beyond Tompkins (2008) that inform the approach on which the model is based, with reference to a few of the key sources.

We have rewritten the beginning of section 6 to enhance these links. For details see response to comments 4b and 7.

b) The justification given for the approach is currently in Section 6. This is that "In reality, scientific enquiry is... subjective" (254-255). While this is entirely plausible and follows from the discussion in 209-216, it needs greater justification than a half sentence. Some brief reference here might be given to post-positivist theory and its explanation of scientific knowledge as situated, in particular reference might be made to a source such as Foucault (1970). This sentence seems to be better suited to the discussion on participative processes in Section 5 (maybe around 216?). Foucault, M (1970) The order of things (Tavistock)

This sentence has been moved into section 5 (lines 224-227, revised ms). A sentence on the concept of post-positivist nature of the proposed approach has been added, incl. a citation of Nursey-Bray et al., 2014, and Foucault, 1970 (lines 227-229, revised ms).

4 Are the scientific methods and assumptions valid and outlined clearly?

Response: No.

a) The explanation of science and engagement (42-62) is confused and currently switches from issue to issue and then back. This section could be rewritten giving: first the scientific problems, second the engagement problems.

The section has been rearranged to describe the scientific problem first (lines 49-55, revised ms) before discussing the public perception of MRs (lines 55-60, revised ms) and finally elaborating on the value of public participation (lines 61-68, revised ms).

b) The stakeholder model (244-298) is in need of clearer and fuller explanation. Part of that might be a rewriting of 245-262, so that first would come the statement of intent (currently 253-254), then general principles (currently 259-262), and then historical approaches and the reasons for changing them.

The paragraphs (lines 245-262, original ms) have been revised to follow the proposed structure. First, we state the intention for the proposed co-production process (lines 260-265, revised ms). Second, we discuss the underlying principles for the proposed approach (lines 266-273, revised ms), and finally, we compare our approach to previous approaches and highlight its advantages (lines 274-286, revised

ms). Particularly this part has been expanded to provide a full explanation for the proposed approach, incl. a range of additional references.

In addition, there should also be a fuller explanation of the principles and evidence behind the stakeholder model (see Question 7).

Further revisions of this section are detailed in our response to comment 7.

5 Are the results sufficient to support the interpretations and the conclusions?

Response: Yes. However, the reasons for implementing the stakeholder model (244-298) are in need of a fuller explanation that demonstrates more clearly than the present version how they follow from Section 5. This would be of particular help in that Section 6 delivers the key thinking (the stakeholder model) of the paper.

This section has been substantially revised based on the reviewer's comments 4b, 5, 6, 7, 13, 16, 14 and 18. For details please refer to our responses to comments 4b and 7.

6 Does the author reach substantial conclusions?

Response: Yes. The conclusions (Section 6) are genuinely interesting, though in need of a fuller justification and explanation.

We thank the reviewer for this positive comment. For fuller justification and explanation, see response to comment 4b and 7.

7 Is the description of the data used, the methods used, the experiments and calculations made, and the results obtained sufficiently complete and accurate to allow their reproduction by fellow scientists (traceability of results)?

Response: No. Sections 1-5 are complete and accurate. Section 6, however, is in need of fuller explanation. In particular, the prescription for the model (264-296) is sketchy and the reasons why political practice makes these "shoulds" necessary ought to be explained. This means linking back (very briefly) to the political preferences explained in Section 5, and where necessary (very briefly) explaining the evidence base for observations (such as those at 269-272, 283-285, 291-294).

Additional explanations and/or references have been added for bullet points 1 (one reference added, line 291, revised ms), 2 (multiple explanations and references added, lines 292-297, revised ms), 3 (short explanation and one reference added, lines 304-306, revised ms), 4 (multiple explanations and references added, lines 307-314, revised ms), 6 (explanations and one reference added, lines 323-326, revised ms).

8 Does the title clearly and unambiguously reflect the contents of the paper?

Response: No. The focus of the paper is on managed realignment (as acknowledged in 25-29). Perhaps the paper could more accurately be titled "Managed realignment as a solution to mitigate coastal flood risks – Optimizing success through knowledge co-production".

Revised as suggested.

9 Does the abstract provide a concise, complete and unambiguous summary of the work done and the results obtained?

Response: Nearly so. For the reasons explained above in answer to Question 8, the first sentence (7) seems redundant.

Whilst we have changed the title of our manuscript, we have kept this first sentence. We feel that this first sentence of the abstract is useful to provide the wider context.

10 Are the title and the abstract pertinent, and easy to understand to a wide and diversified audience?

Response: Yes.

We thank the reviewer for this positive comment.

11 Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or appendixes listing them?

Response: Almost. "m²" is used for m² in 153.

Revised as suggested.

12 Is the size, quality and readability of each figure adequate to the type and quantity of data presented?

Response: Yes.

We thank the reviewer for this positive comment.

13 Does the author give proper credit to previous and/or related work, and does he/she indicate clearly his/her own contribution?

Response: Nearly so. As explained in the answer to Question 4, a clearer explanation of why the model has been designed in the way it has would be desirable.

Revisions to more clearly explain why the model has been designed in the way it has, are detailed in our response to comment 4b. The revised text refers to more relevant literature, including incl. additional references that have not yet been included in the original manuscript.

14 Are the number and quality of the references appropriate?

Response: Mostly. Shiers (2014) (182) is a tendentious blog and does not look entirely trustworthy. There is already a refereed, documented example of the same issue in Oliver (2021, 91-92) which is more reliable.

Oliver, S (2021) Land abandoned to the sea: the managed realignment of coastal areas (IB Tauris)

Revised as suggested. Throughout the manuscript, we have also added some more relevant references.

15 Are the references accessible by fellow scientists?

Response: Yes.

We thank the reviewer for this positive comment.

16 Is the overall presentation well structured, clear and easy to understand by a wide and general audience?

Response: Mostly. The suggestion has already been made that 245-262 could be rewritten; the same could also be said of 42-62. In both cases it would help if the authors were to: make their overall point, then proceed from general issues to specific points.

Revisions that address this comment are detailed in our responses to comments 4a and 4b.

17 Is the length of the paper adequate, too long or too short?

Response: Too short. Section 6 needs expanding in order to address the comments made above, perhaps to half as long again as its current length.

Section 6 has been expanded by nearly 50% to address the previous comments as detailed in responses to comments 4b and 7.

18 Is there any part of the paper (title, abstract, main text, formulae, symbols, figures and their captions, tables, list of references, appendixes) that needs to be clarified, reduced, added, combined, or eliminated?

Response: Yes. 42-62 (rewritten) and Section 6 (expanded).

For details on the implemented revisions, see responses to comments 4a, 4b and 7.

19 Is the technical language precise and understandable by fellow scientists?

Response: Yes.

We thank the reviewer for this positive comment.

20 Is the English language of good quality, fluent, simple and easy to read and understand by a wide and diversified audience?

Response: Mostly. There are a few errors: "Avgerage" and "Scheme_Typ" (diagram, following 36), "ca. 3000" (128), "modelling on" (132), "m2" (153, four times), single inverted commas (170), "to considerably vary" (169), "6" (299).

All typos have been corrected.