

Global sea-level rise will create a new biological invasion front

Corresponding Author: Professor Xuan Liu

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

I appreciated the opportunity to re-review a “Over three-quarters of sea-level rise induced inundated areas face future risks of alien marine invasions worldwide.” I am impressed with the sheer number of species explored here and it is addressing a pressing if not under-researched topic and system. There is an abundance of data available to this paper which provides a forward-looking approach towards potential invasive species out to 2100. The revised version shows clear improvements in structure, methodological transparency, and overall rigor. Many of my initial concerns have been carefully considered and addressed by the authors, particularly with respect to adopting a more conservative modeling framework, improving species selection, and expanding sensitivity analyses.

That said, while the manuscript is significantly improved, there remain a few conceptual and methodological issues that should be clarified further. Below I outline where the authors have adequately addressed prior concerns and where some issues remain, referring to relevant sections and line numbers in the revised manuscript.

Line 63-4: I would suggest instead of “controlling marine invasions” -> “mitigating marine invasions”

Line 104: “costs” -> “losses”

Line 107: “early prevention strategies” – thank you for the examples in lines 208-212, this is great to have.

Lines 112–117, 248–251, 548–552: The authors have addressed my concern regarding overly optimistic invasion assumptions by restricting projections to basin-scale expansion. This is a substantial improvement and makes the results more defensible. However, the model still implicitly assumes rapid colonization of adjacent inundated habitats without accounting for dispersal limitations or ecological resistance within basins. This should be more clearly acknowledged as a remaining limitation.

Lines 112–114, 555–557: The issue regarding species selection and ecological realism has been well addressed. The restriction to intertidal and shallow-water species greatly improves the biological plausibility of the projections. However, it would still be helpful to briefly discuss how habitat loss (e.g., deepening of existing habitats) might offset gains in newly inundated areas for some taxa.

Lines 125-128, 128-130: I appreciated the addition of quantitative values, but the sentence structure is long. Consider splitting into two sentences for readability.

Lines 125–130, 142–145, 156–158: The manuscript now provides clear quantitative estimates rather than qualitative descriptions, which greatly improves interpretability and scientific rigor.

Data Availability section: The inclusion of code, model objects, and data in a public repository fully addresses concerns regarding reproducibility.

Lines 138–142: The authors have clarified the scope of the manuscript by focusing on inundated areas and moving broader ocean projections to the supplementary material. This improves clarity and alignment with the stated objectives.

Lines 156–158: “Reaches an average of...” -> consider specifying whether this refers to mean across species or regions for clarity.

Lines 173-174: “exhibited low levels of extrapolation” – is this in environmental niche space or geographically? I would just be explicit here.

Lines 179-182: this is a bit of a long sentence. I would recommend a period after globe, and then start the next sentence with "Here we..."

Lines 213-221: The manuscript acknowledges that newly inundated habitats may differ from existing marine systems; however, the modeling framework still assumes immediate suitability. Additional discussion of potential ecological constraints (e.g., sediment dynamics, nutrient fluxes, anoxia) would further strengthen the interpretation of results.

Lines 223-228: The estuary discussion is strong but could benefit from one sentence explicitly linking back to results.

Lines 249-251: replace "overlooks" with "excludes"?

Lines 548-552: While the basin-restricted approach partially addresses concerns about dispersal and invasion pathways, the manuscript still does not explicitly incorporate major invasion vectors such as shipping. Given the importance of these processes in marine invasions, this limitation should be more explicitly emphasized.

Lines 629-635, 712-720: The explanation of the BIO-ORACLE database and associated climate models is now clear and sufficient. The consistency between CMIP6 models used for both environmental predictors and sea-level rise projections is particularly appreciated.

Lines 690-693, 780-788, 812-818: The authors have done an excellent job addressing concerns about model robustness and overfitting. The inclusion of additional performance metrics (e.g., Boyce index), sensitivity analyses, and overfitting diagnostics substantially strengthens the manuscript.

Overall, the manuscript is significantly improved and now presents a much more robust and defensible analysis. The remaining concerns are largely conceptual rather than methodological and are appropriately acknowledged in the Discussion. I therefore would support publication with the suggested minor revisions to clarify the remaining points (noted above).

(Remarks on code availability)

Reviewer #2

(Remarks to the Author)

I am impressed with how the authors have addressed all the many constructive comments from all the reviewers.

I was initially much against the manuscript - to some extent because the biology included many wrong classification and taxonomies.

By redoing everything and inviting well-established marine invasion biologist and SLR experts onto the manuscript the science is now much stronger and more solid.

I also acknowledge that there are no similar global models over marine invasions onto future land being 'invaded' by the sea. Thus, the modelling effort is novel. However, only time will tell how precise those models are.

I still have reservations about the precision of the SDMs for some of the smaller and cryptic marine invasive species - but the analysis is way better and now based on correct taxonomy.

In short - the paper is now solid and well written and I have no further comments.

(Remarks on code availability)

Reviewer #4

(Remarks to the Author)

I have now gone through the responses to my comments and the updated methods section. The sea-level rise projections are now fully localized based on state-of-the-art AR6 projections. I have therefore no other concerns regarding publication (noting that the rest of the ms is outside of my field).

(Remarks on code availability)

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Reviewer #1 (Remarks to the Author):

Comment 1)

I appreciated the opportunity to re-review a “Over three-quarters of sea-level rise induced inundated areas face future risks of alien marine invasions worldwide.” I am impressed with the sheer number of species explored here and it is addressing a pressing if not under-researched topic and system. There is an abundance of data available to this paper which provides a forward-looking approach towards potential invasive species out to 2100. The revised version shows clear improvements in structure, methodological transparency, and overall rigor. Many of my initial concerns have been carefully considered and addressed by the authors, particularly with respect to adopting a more conservative modeling framework, improving species selection, and expanding sensitivity analyses.

Our Response: We are sincerely grateful for your positive comments and finding the revised version robust and convincing.

Comment 2)

That said, while the manuscript is significantly improved, there remain a few conceptual and methodological issues that should be clarified further. Below I outline where the authors have adequately addressed prior concerns and where some issues remain, referring to relevant sections and line numbers in the revised manuscript.

Our Response: Thank you for carefully reviewing our manuscript and for providing constructive suggestions for further improvement. We have carefully revised our manuscript accordingly. **Please note that line number in the response letter refers to the clean revised version of the manuscript.**

Comment 3)

Line 63-4: I would suggest instead of “controlling marine invasions” -> “mitigating marine invasions”

Our Response: Greatly appreciated and done as suggested (Line 57).

Comment 4)

Line 104: “costs” -> “losses”

Our Response: Greatly appreciated and done as suggested (Line 100).

Comment 5)

Line 107: “early prevention strategies” – thank you for the examples in lines 208-212, this is great to have.

Our Response: Thank you for your positive evaluation.

Comment 6)

Lines 112–117, 248–251, 548–552: The authors have addressed my concern regarding overly optimistic invasion assumptions by restricting projections to basin-scale expansion. This is a substantial improvement and makes the results more defensible. However, the model still implicitly assumes rapid colonization of adjacent inundated habitats without accounting for dispersal limitations or ecological resistance within basins. This should be more clearly acknowledged as a remaining limitation.

Our Response: We agree with this suggestion. Accordingly, we have explicitly acknowledged this limitation as follows (Lines 210–213):

“In addition, hydrodynamic connectivity, sediment dynamics, and nutrient fluxes in these newly inundated lands may differ substantially from those in mature coastal marine ecosystems, potentially limiting or delaying the dispersal, colonization, and establishment of marine non-native species.”

Comment 7)

Lines 112–114, 555–557: The issue regarding species selection and ecological realism has been well addressed. The restriction to intertidal and shallow-water species greatly improves the biological plausibility of the projections. However, it would still be helpful to briefly discuss how habitat loss (e.g., deepening of existing habitats) might offset gains in newly inundated areas for some taxa.

Our Response: We appreciate this suggestion and clarify this issue in the text as follows (Lines 270–272):

“Although outside of the scope of our study, SLR may also cause range loss by deepening existing coastal marine habitats, which could partly offset projected gains in newly inundated areas.”

Comment 8)

Lines 125–128, 128–130: I appreciated the addition of quantitative values, but the sentence structure is long. Consider splitting into two sentences for readability.

Our Response: Following your suggestion, we have revised the text as follows (Lines 120–126):

“Based on these projections, 360,000 km² of coastal lands will be inundated by 2050, accounting for 0.24% of the current global surface. By 2100, the inundated area is projected to increase by about 65%, reaching 550,000 km² or 0.37% of global land surface (Fig. 2, Supplementary Table 3). Although inundated coastlands are projected across the latitudinal gradient, those most severely affected are concentrated at lower latitudes (Fig. 2). This concentration of inundated areas at lower latitudes becomes more pronounced from 2050 to 2100.”

Comment 9)

Lines 125–130, 142–145, 156–158: The manuscript now provides clear quantitative estimates rather than qualitative descriptions, which greatly improves interpretability and scientific rigor. Data Availability section: The inclusion of code, model objects, and data in a public repository fully addresses concerns regarding reproducibility.

Our Response: Thank you for your positive evaluation.

Comment 10)

Lines 138–142: The authors have clarified the scope of the manuscript by focusing on inundated areas and moving broader ocean projections to the supplementary material. This improves clarity and alignment with the stated objectives.

Our Response: Thank you for your positive evaluation.

Comment 11)

Lines 156–158: “Reaches an average of...” -> consider specifying whether this refers to mean across species or regions for clarity.

Our Response: We now clarified this issue in the text as follows (Line 152–154):

“The average projected expansion of suitable habitat within inundated areas across all study species reaches 28,000 km² by 2050 and increases to 37,000 or 42,000 km² by 2100 under SSP2-4.5 or SSP5-8.5, respectively (Supplementary Table 6).”

Comment 12)

Lines 173–174: “exhibited low levels of extrapolation” – is this in environmental niche space or geographically? I would just be explicit here.

Our Response: We now made this issue more explicit as follows (Line 166–167):

“...low levels of extrapolation in environmental niche space”.

Comment 13)

Lines 179–182: this is a bit of a long sentence. I would recommend a period after globe, and then start the next sentence with “Here we...”

Our Response: Greatly appreciated and done as suggested (Lines 174).

Comment 14)

Lines 213–221: The manuscript acknowledges that newly inundated habitats may differ from existing marine systems; however, the modeling framework still assumes immediate suitability.

Additional discussion of potential ecological constraints (e.g., sediment dynamics, nutrient fluxes, anoxia) would further strengthen the interpretation of results.

Our Response: Greatly appreciated and done as suggested. Please see our response to your #6 comment above for details.

Comment 15)

Lines 223–228: The estuary discussion is strong but could benefit from one sentence explicitly linking back to results.

Our Response: We appreciate this suggestion and revise the text as follows (Lines 220–223): *“Our results show that future ranges expansions into inundated lands are projected to happen preferentially in areas close to estuaries, which are projected to offer some of the most suitable habitats for marine non-native species (Figs. 3 and 4, Supplementary Figs. 7-12).”*

Comment 16)

Lines 249-251: replace “overlooks” with “excludes”?

Our Response: Greatly appreciated. In this round of revision, we just deleted the sentence. (Line 251).

Comment 17)

Lines 548–552: While the basin-restricted approach partially addresses concerns about dispersal and invasion pathways, the manuscript still does not explicitly incorporate major invasion vectors such as shipping. Given the importance of these processes in marine invasions, this limitation should be more explicitly emphasized.

Our Response: We completely agree with your comments on the importance of invasion vectors in biological invasions. Indeed, this was already highlighted in the manuscript as follows (Lines 109–112):

“Given the large uncertainties associated with projecting the effects of future transoceanic shipping on the global expansion of marine non-native species, we conservatively restricted our analyses to those marine ecoregions⁴¹ where each species is currently established as non-native (i.e., demonstrably historically absent¹⁹; see Methods).”

Nonetheless, as suggested, we now further explicitly strengthened this caveat to the discussion as follows (Lines 246–253):

“Finally, introduction vectors, such as shipping, play a vital role in facilitating the spread of marine species beyond their native ranges^{71,72}. Although future invasions facilitated by these vectors do not compromise our results because we conservatively focused only on inundated ecoregions that currently have resident established non-native species, we acknowledge that by not incorporating these potential vectors of introduction via anthropogenic activities, our projections likely underestimate the extent and magnitude of future expansion of marine non-native species into inundated areas.”

Comment 18)

Lines 629–635, 712–720: The explanation of the BIO-ORACLE database and associated climate models is now clear and sufficient. The consistency between CMIP6 models used for both environmental predictors and sea-level rise projections is particularly appreciated.

Our Response: Thank you for your positive evaluation.

Comment 19)

Lines 690–693, 780–788, 812–818: The authors have done an excellent job addressing concerns about model robustness and overfitting. The inclusion of additional performance metrics (e.g., Boyce index), sensitivity analyses, and overfitting diagnostics substantially strengthens the manuscript.

Our Response: Thank you for your positive evaluation.

Comment 20)

Overall, the manuscript is significantly improved and now presents a much more robust and defensible analysis. The remaining concerns are largely conceptual rather than methodological and are appropriately acknowledged in the Discussion. I therefore would support publication with the suggested minor revisions to clarify the remaining points (noted above).

Our Response: Thank you again for your positive evaluation and supporting publication upon some minor revisions what we have done above.

Reviewer #2 (Remarks to the Author):

I am impressed with how the authors have addressed all the many constructive comments from all the reviewers.

I was initially much against the manuscript - to some extent because the biology included many wrong classification and taxonomies.

By redoing everything and inviting well-established marine invasion biologist and SLR experts onto the manuscript the science is now much stronger and more solid.

I also acknowledge that there are no similar global models over marine invasions onto future land being 'invaded' by the sea. Thus, there the modelling effort is novel. However, only time will tell how precise those models are.

I still have reservations about the precision of the SDMs for some of the smaller and cryptic marine invasive species - but the analysis is way better and now based on correct taxonomy.

In short - the paper is now solid and well written and I have no further comments.

Our Response: Thank you very much for carefully evaluating our revised manuscript and for your supportive comments.

Reviewer #4 (Remarks to the Author):

I have now gone through the responses to my comments and the updated methods section. The sea-level rise projections are now fully localized based on state-of-the-art AR6 projections. I have therefore no other concerns regarding publication (noting that the rest of the ms is outside of my field).

Our Response: We are very grateful for your positive comments.