

Climate change affects future sea-bed mobility via storms and sea level rise.

Corresponding Author: Dr Julia Rulent

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Rulent,

Your manuscript titled "Impact of the climate change on hydrodynamic seabed stressors" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised substantial concerns that must be addressed. In light of these comments, extensive revisions will be required before we can further consider the manuscript for publication. We would, however, be interested in considering a revised version that fully addresses these concerns.

In particular, we require that you:

- (1) clearly articulate the study's novelty and global implications, ensuring key conclusions are robustly supported
- (2) fully justify your model approach and acknowledge limitations therein
- (3) enhance discussion of management implications for benthic species and offshore wind sites.

We hope you will find the reviewers' comments useful as you decide how to proceed. If additional work allows you to either incorporate or refute these criticisms, we will be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers.

When resubmitting, please provide a point-by-point response to the reviewers' comments. Please submit your responses as a separate file, distinct from your cover letter where you can add responses to the Editors' comments that you do not want to be made available to the reviewers. Word files are preferred. We recommend that any figures, tables or graphs that are included in the response to reviewers are also included in the main article or Supplementary Information.

Please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the reviewers' comments with a list of your changes to the manuscript text (which should be in a separate document to any cover letter), a tracked-changes version of the manuscript (as a PDF file) and any completed checklist:

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Shan Liu
External Editor
Communications Earth & Environment

Alice Drinkwater, PhD
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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

This paper presents a modeling study in which the authors couple the Wavewatch III and NEMO model to assess how seabed stresses will change with climate change on the Northwest European Shelf. The paper presents novel research with seemingly sound methods, it is well written, and the graphics are nicely designed and easy to interpret. Overall, I think this is a high quality manuscript that will be fit for publication with minor revisions.

Several minor questions occurred to me while reading it

1. As a reader, I want to know more about why this matters. It is very interesting from a theoretical perspective but I feel the discussion of management implications could be deeper.

- Any benthic species living in the area that are important for fisheries or other cultural reasons and might be impacted?
- "The rock grade chosen to keep infrastructure protected should not be mobilised in the next few decades, whilst modified mattresses to be nature-inclusive may not withstand intense currents [56]. Changes to sediment mobility can cause cage sinking and sediment movement in general calls for improved reef and housing designs to ensure long-term restoration success [57]." Some of these impacts are technical and niche. What are modified mattresses? And what is cage sinking? Would be nice to define these terms so the reader doesn't have to go look them up to understand why your work is important.
- More context on the offshore wind sites in the area would be helpful. How important are these sites for the energy supply in UK/Europe? Consider including the location of these sites on some of the maps presented in the paper for reference.
- This may be out of scope, but I am curious how the seabed stresses you describe here compare to the forces of fishing practices that disturb the seafloor?

2. Some questions about validation

- Based on the presented info, it seems like the model had good agreement without tuning any parameters (e.g. there is a "Validation" period but no "Calibration" period). Is that so? If another work developed a well-tuned model, that should be explained/cited. If tuning parameters were adjusted to maximize agreement, that should be explained. And if tuning parameters were adjusted to maximize agreement, I would think that should be done with one time series for calibration, and

then compared to a separate time series for validation.

- Curious about the "erroneous spikes" seen in the observations of seabed stress in fig A2. Do you believe the observations were made in error? If so, why? If not, then I think calling observations "erroneous" is misleading - the observations are not erroneous but rather your model is not capturing the observed spikes. If this is the case, I am curious if you have any ideas what processes your model might not be including that are causing the spikes.
- Consider adding location of the calibration/validation datasets to Fig 1.
- Why are all validation time series from the Irish sea? This is only a small part of the study region. Are there tide gauges available in other areas? If so, why were those not included?

3. Questions about the model

- The paper states "The simulation setup is consistent with SLR estimates derived from the same global climate model and scenario." But why is 2050/2100 SLR higher for the "hindcast + SLR" vs the projections?
-

3. Since you specifically mention German Bight in the text, consider adding a label pointing it out in fig 3f.

4. Did the authors develop equations 5 and 6? If not, cite. If so, consider adding an explanation on the theory of the equations.

5. You say in the manuscript that coupling the wave/flow models online is an area for future research, and you also say that doing so is important for assessing shallow water impacts which are higher than deep water impacts. Curious why you chose to couple the current and wave models offline instead of online in this study? Did coupling online add significant computation time or something? What is the order of magnitude of that increase? And could you clarify a bit on what "shallow water" means here, i.e. in what depth of water are the findings of your work accurate given this limitation?

Reviewer #2 (Remarks to the Author):

Dear Authors,

Thank you for the opportunity to review your manuscript. The study addresses with rising sea level an important topic, and features very interesting modeling results focused on the continental shelf of the UK and their potential implications for coastal dynamics. This is a sensitive topic that is widely discussed in the community, and every contribution is important. The breakdown of stress factors (waves, tides, and currents) affecting the seabed was fascinating to read. Linking stress and sediment classes is a challenging method for gaining new insights into various future scenarios. The figures are very nicely laid out and presented, and it is very clear that the data processing has been carried out correctly. The modeling framework has the potential to contribute valuable insights into the response of shelf environments and hydrodynamic forcing. The proposed objectives are also not supported by the study presented, which is unfortunate, as a great deal of effort and potential has gone into the work. It would be better if the existing illustrations were incorporated more strongly and the results section focused more on the findings and less on speculation and conjecture. The discussion section would also benefit greatly from referring more to the very good results and being less speculative.

However, the current version of the manuscript presents several inconsistencies in the data interpretation and scientific argumentation style that need to be addressed before the work can be properly evaluated. Also the proposed aims (addressed in the details section) are not supported by the presented study. Although it is unfortunate, after careful consideration, I have decided to recommend to reject the article because, on the one hand, essential comparisons and connections are missing to justify the specified objectives and, on the other hand, a large part of the text is based on speculation. Given the sensitivity and complexity of climate change related research topic, interpretations should be presented cautiously and clearly, with transparency of the conclusions. It is important that projections or scenarios are discussed within their uncertainty bounds and referenced against established assessments (e.g., IPCC reports) to avoid overstatement. I put as much effort as possible into the review to make my decision understandable and to show my respect for the work done by the authors.

Sincerely,
Reviewer

Reviewer #3 (Remarks to the Author):

Review of the manuscript "Impact of the climate change on hydrodynamic seabed stressors" by Rulent J. et al.

General comments:

The manuscript offers a significant examination of the impacts of climate change, particularly sea level rise (SLR) and increased storminess, on hydrodynamic seabed stresses and sediment transport capacity in the North-west European Shelf (NWES). This is the first study to find out how big these changes are likely to be in this important industrial area. This will help with marine spatial management in the future.

Major concerns:

Dependence on CMIP5 projections: The use of CMIP5 climate projections, particularly HadGEM3-GC3, under RCP8.5, could be considered a weakness. CMIP5 was the best available at the time, but CMIP6 is better since it has better models, higher resolution, new scenarios (SSPs), and more recent science. Using the most up-to-date climate projections would be the best way to make marine spatial management last. The writers implicitly recognise this by describing the scenarios as "extreme, yet possible."

Limited generalisation: The applicability and specific findings are restricted to the North-west European Shelf (NWES). Although the method is characterised as "globally applicable," the inferences regarding particular impacts (e.g., alterations in tidal amphidromes or wave predominance in the North Sea) are localised. The title says that the study will look at the "Impact of climate change on hydrodynamic seabed stressors." This could mean that the study will look at a wider range of situations than it really does. The argument for the NWES being representative is strong, but generalisations require caution.

Uncoupled modelling: The NEMO (ocean) and WW3 (wave) models are run stand-alone, and their stresses are combined offline. The authors recognise this as a weakness and propose that a fully integrated NEMO-WaveWatch3 system would represent a future enhancement, especially in shallow water areas where the most significant effects are noted and wave-ocean coupling is most pronounced.

Simplifying sediment dynamics: The estimation of sediment transport capacity assumes a non-cohesive sediment bed and inverts the problem to find the coarsest mobilisable grain size. This is a necessary simplification, but the authors note that a complete estimate would require high-resolution grain size data. Additionally, the intricacies of sediment habitat dynamics are examined but not modelled (for instance, whether mobilisation is adequate to modify bottom composition and its impact on benthic populations).

Intense storminess: A critical point of discussion concerns the confidence level associated with the projected increase in intense storminess. As the authors themselves note (Page 11, Lines 276-280), presenting the extreme storm scenario as an "end-member experiment" or a "bounding example" for risk management is appropriate, particularly given the acknowledged uncertainty in mid-latitude storm track projections, even within the newer CMIP6 ensemble. While the proposed mechanism—that higher SST fuels stronger storms through increased energy and moisture—is thermodynamically sound, robust confidence remains low in predicting the future frequency and magnitude of extra-tropical cyclones affecting the NWES. The manuscript should therefore more explicitly frame this section not as a prediction of what will occur, but as an essential hazard quantification based on the plausible extreme range of a single model realisation, ensuring the limitations of drawing statistically robust conclusions about future storminess from single model runs are foregrounded in the discussion.

In conclusion, the manuscript provides a rigorous and novel regional quantification of future seabed stressors, successfully isolating the predictable effects of sea level rise (SLR) from the potentially extreme, intermittent impacts of intensifying storms across the North-west European Shelf (NWES). This work is essential for future-proofing regional marine spatial management and is highly commendable. However, for a higher-level journal seeking global impact or the absolute cutting edge of methodology, the study faces hurdles related to scope and source material. Its strong regional focus on the NWES, while justifiable, limits its immediate generalisability, and its reliance on CMIP5/RCP8.5 climate projections, now superseded by CMIP6/SSPs, positions the foundational data a generation behind current standards. Furthermore, the acknowledged use of uncoupled hydrodynamic and wave models introduces a potentially questioned technical constraint. Therefore, while the paper represents an excellent contribution to a dedicated journal in shelf sea dynamics, oceanography, or coastal engineering, its regional focus and the use of antecedent climate model data may reduce its novelty and impact required for the most selective publications.

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Version 1:

Decision Letter:

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Dear Dr Rulent,

Your manuscript titled "Impact of the climate change on hydrodynamic seabed stressors" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers, which pertains to acknowledging the manuscript's regional focus. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Alice Drinkwater, PhD
Associate Editor
Communications Earth & Environment
Consulting Editor
Communications Sustainability

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have satisfactorily addressed my questions and concerns. The additional information on model calibration and model setup choices greatly improve the rigor of the work and I appreciate the extra context on offshore wind and bottom

stress from fishing. It is my opinion that this paper is ready for publication.

Reviewer #3 (Remarks to the Author):

I would like to thank you for the opportunity to review this manuscript once again. It is clear that a significant amount of effort has gone into this revision, and I appreciate the high level of technical rigour maintained throughout the document.

The revised manuscript and the accompanying response letter address several of my initial concerns with clarity. The depth of the hydrodynamic analysis, the detailed explanation of using CMIP5, and the estimate of the combined stress offline are particularly noteworthy. However, the study's scope has significantly narrowed to a regional focus on the North Sea or NWES. Your response suggests that this region serves as a global proxy because it includes diverse characteristics such as varying depths and tidal ranges; however, I find this justification to be overly brief and unconvincing in addressing the fundamental issue of scalability. While NWES is a complex system, its dynamics—driven by specific North Atlantic tidal resonance and ETCs, but it does not necessarily translate to other shelf systems. For instance, the hydrodynamic stressors in the NWES are fundamentally different from micro-tidal shelf systems like the Gulf of Mexico, Baltic Sea or tropical shelves where seasonal monsoons or TC activity are the primary drivers of seabed disturbance. Consequently, it remains unclear if these findings can be generalised.

While this allows for a very detailed and robust analysis of that specific area, it reinstates my original concern regarding the scalability of the conclusions. It remains unclear if the mechanisms and findings identified here can be generalised to other regions or global contexts. Given this shift in scope, I have deferred to the Editor to judge whether the manuscript's current regional focus aligns with the broad impact requirements of this journal.

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Reviewer Cover Letter to the Authors

General feedback:

The manuscript would benefit from a clearer delineation of its spatial and conceptual focus. While the study models a continental shelf area, its relevance to deeper ocean floor processes is not evident and should either be justified or explicitly excluded. Furthermore, the introduction does not clearly define which natural and anthropogenic factors are incorporated into the analysis, making it difficult to understand the scope and assumptions of the model.

The relationship between coastline evolution, sea-level rise, and the expansion of the effective fetch area is fundamental for transferring the model to other coastal regions, especially those with weak or negligible tidal forcing. This connection is currently not explained and should be explicitly discussed to strengthen the model's broader applicability.

The manuscript frequently alternates between regional findings for the UK shelf and claims of global relevance without providing a coherent framework that justifies this extrapolation. A clear distinction is needed between results that are region-specific and those that are intended to support generalizable insights (e.g., Chapter 2.1, Definition of Factors).

The consequences for the results by isolation of stress factors (waves, tides, and currents) is not sufficiently explained. A discussion is needed on how these components interact and how their coupling—or the lack thereof, affects the interpretation of results. Including a section that compares isolated versus coupled forcing scenarios would significantly enhance the robustness of the analysis.

In several parts of the results and discussion sections, statements are postulated without clear methodological backing or sufficient citation. At other points, findings are supported merely by references to colorbars or figures, rather than quantitative or statistical evidence. The scientific argumentation and referencing strategy should be reconsidered to ensure transparency and reproducibility.

Regarding the Temporal Resolution and Data Interpretation, the discussion of seasonal variability is based on only two observation points per year, which is insufficient to substantiate claims about seasonal dynamics. The manuscript should acknowledge this limitation explicitly.

The proposed decoupling effect between waves and the seabed is of conceptually interesting in the presented study but not substantiated by the presented results. If this process is to be claimed, supporting evidence or a sensitivity analysis should be added. Similarly, it remains unclear how the model accounts for coastline shifts under projected SLR scenarios. This should be clarified in the methods or model description.

Throughout the manuscript, there is a recurrent inconsistency between proposed mechanisms (‘‘what the authors suggest will happen’’) and the actual findings derived from the model. Clear differentiation between hypotheses, modeled outcomes, and speculative interpretation is essential

for scientific clarity.

Please note that the references mentioned throughout my comments are provided purely as examples to illustrate possible approaches or comparisons. I have no scientific or institutional connection to any of the authors or groups cited.

Reviewer Comments (Detailed Line-by-Line Suggestions)

Introduction

Line 47-48:

The introductory statement is accurate but potentially misleading. While it is true that the global ocean, (especially with respect to seabed processes) is not yet fully understood, the main focus of this study is clearly on the continental shelf, with a regional emphasis on the North Sea. The manuscript frequently alternates between regional findings and global applications without providing a conceptual framework that justifies the generalization.

Line 55:

“Larger storms increase energy transfer” : Although this is a plausible scenario, it has not been empirically confirmed and should be presented as a projection rather than a factual statement. Precision is essential when addressing climate change and sea-level rise (SLR) forecasts. Please cross-check with the latest IPCC report to ensure consistency with established climate scenarios.

Line 74-75:

This sentence is circular; it effectively states that “something determines how it acts.” The cause-and-effect relationship needs clarification.

Line 76-77:

“The need for maps [“|”] has been repeatedly flagged as a limitation”, The need for maps represents a research gap rather than a limiting factor. Please rephrase to clarify this distinction.

Line 85-88:

The term “future-proof management” requires definition. Which “climate-related drivers” are included in this study? Does the manuscript address general anthropogenic impacts, or are references limited to the wind farm case study?

Line 92-96:

The statement conflates global-scale assumptions with regionally variable processes, which reduces scientific precision. A clearer separation between global trends and regional dynamics is necessary. Even if the described outcome is the most likely scenario, it should be explicitly identified as a hypothesis rather than a confirmed result.

Line 102:

“Realistic mid- and end-century climate scenarios”, Please provide context for what qualifies as “realistic.” Why not consider high-end scenarios, such as those described by Dayan et al. (2021), “High-End Scenarios of Sea-Level Rise for Coastal Risk-Averse Stakeholders”? These could support long-term, risk-averse spatial planning. Clarify what distinguishes the proposed approach from such existing frameworks.

Line 103–104:

Clarify what is meant by “climate-related and dominant driver.” Specify how these drivers were selected and parameterized.

Lines 100–115 (Objectives):

The study’s objectives are not consistently addressed in the manuscript:

Lines 100–101: The objective to “quantify sediment transport capacity in space and time” is not fully achieved. Compare with line 291 (“we calculate which maximum median grain sizes would be mobile for a given bed shear stress”) and line 388 (“Sediment transport capacity does not easily translate to sediment composition”). The internal logic between these statements remains unclear.

Lines 104–107: Seabed infrastructure is discussed hypothetically in the context of a wind farm case study, yet no clear structural parameters or SLR-related stress interactions are provided. “Environmental health” is mentioned but not substantiated with data in the results section. Consequently, the findings cannot be directly linked to marine spatial planning recommendations.

Lines 107–111: The manuscript does not explain which results or methods have global applicability, or why. Tidal evolution under SLR is known to depend strongly on regional factors, yet the interaction between shifting coastlines and tidal-current dynamics is not addressed. This is essential for understanding potential amphidromic point migration, which the manuscript references but does not substantiate.

Section 2

Section 2.1

General feedback:

Figure 1: Move the figure labels (a–f) to the top right and the year/month or season to the bottom left for clarity. Also in the color bar label “(Nm²)”, a space is missing.

Although mentioned in the caption, please specify in the main text (around lines 130–132 and 146–147) which values are measured versus modeled.

The introductory sentence, “Any changes in bed stress brought about by climate change or seabed infrastructure must be put into the context of natural variability,” is valid, but no baseline context is provided on current sediment transport, anthropogenic impacts, or seabed conditions.

Line 120–121:

“Large but predictable variability in seabed stress is generated over the 28-day lunar cycle.”, This is region-specific and not globally applicable. The phrase “large but predictable” is vague and should be replaced with

a quantifiable or referenced statement.

Line 129-131:

For the Anglesey, St David's Head example, please clarify whether current speed values are modeled or measured. Also, explain why these locations are representative. Numerical values are provided without contextual comparison (e.g., no corresponding min/max values for storm-induced stress).

Line 137-139:

There is an inconsistency between text and Figure 1. The text reports spring tide bed stresses up to 8 N m^{-2} , while the storm-event color bar is truncated at 6 N m^{-2} . Please reconcile this discrepancy.

Line 140:

Show rather different spatial patterns, specify in what respect the patterns differ (magnitude, distribution, directionality, etc.).

Line 144:

What is meant by 'there are some commonalities'? The argument becomes difficult to follow. Please specify what commonalities are being referred to and between which variables.

Section 2.2

Figure 2: Panels 2b and 2d present annual trends as bar plots. Consider selecting representative regions from 2a and plotting mean stress versus month with error bars (min-max). This would make lines 175-184 easier to follow.

The section heading 'Drivers' is not reflected in the text. Clearly define what qualifies as a 'driver'.

Water depth (line 162) is mentioned but not discussed further.

Line 163-164:

Clarify whether this regional breakdown approach is transferrable to other continental shelf regions globally.

Summary (line 167-170):

The terminology 'processes', 'contributors', and 'drivers' appears interchangeable but should be explicitly defined. Consistency will improve clarity.

Section 2.3

Line 196-197:

The meaning of 'non-linear' is unclear, does it refer to spatial or temporal variability (or both)? Please specify.

Lines 198-204:

The mechanism of amphidromic behavior is addressed via reference [4], but here the discussion should focus on coastline shifts due to SLR, not solely on bathymetric change. Clarify which mechanism dominates.

Line 210:

The paragraph on tidal dissipation lacks quantitative support. Statements such as “SLR decouples currents from the seabed” require justification through an energy budget or comparable analysis. See Lu et al. (2024) for reference on tidal energy dissipation.

Lines 214–220:

For statements such as “of the order 0.3 N m^{-2} ,” please include a figure reference or data source to aid traceability.

Lines 232–247:

The discussion of SLR effects on long-period swell waves lacks mechanistic detail. Claims such as “swell waves cannot reach the seabed as easily” or “wave stresses are reduced almost everywhere” are speculative without quantitative validation. Provide theoretical or numerical evidence, and clarify which wave components are affected. As written, the cause–effect reasoning remains uncertain.

Line 250:

“The least uncertain facet of climate change” is an overgeneralization unsupported by quantitative context. Confidence and magnitude are distinct and should not be conflated.

Line 258:

Clarify the use of “end-member experiment.” If the scenario represents an upper bound, justify it and relate to comparable high-end studies (e.g., Dayan et al., 2021).

Lines 251–252:

“This is much more uncertain,” if uncertainty is recognized, quantify or discuss its range in the results or discussion section.

Lines 255–257:

Please refer to Section 5.5 for detailed information as suggested.

Section 2.5

Line 283:

“Using the new bed shear stress maps,” Reference the corresponding figure for clarity.

Line 288:

When discussing “grain size on the shelf,” consider also mentioning sediment composition, biological components, and particle sorting.

Line 293:

Assuming non-cohesive sediment has major implications for remobilization. This assumption should be discussed explicitly in the Discussion (see, e.g., your coauthor’s work “Classifying seabed sediment type using simulated tidal-induced bed shear stress”).

Line 323:

The claim that “tidal stresses on the shelf are expected to reduce” requires a supporting figure or citation. This appears inconsistent with line 227 (“SLR can also alter the wave stress”). Please reconcile these statements.

Lines 338-341:

The link between changes in sediment class and effects on anthropogenic structures (e.g., "East Anglia One") is unclear. Specify what quantitative or qualitative results support this connection.

Lines 341-342:

A 30 km scale bar indicates distance, not data resolution. The statement regarding dataset limitations inferred from the scale bar is confusing and should be revised. Additionally, references are unevenly distributed—some unsupported claims lack citations, while this minor statement lists three.

Discussion

Line 344:

The statement "future seabed mobility will be controlled by" simplifies the system; the seabed consists of mixed sediment classes with biological and compositional variability that should be acknowledged.

Lines 351-357:

The argument that "SLR should be included in MPA management" is valid but unsupported by empirical evidence from this study. No data are presented linking SLR-induced stress changes to ecological consequences such as species diversity, biomass, or sediment biogeochemistry.

Lines 369-377:

This paragraph lacks a clear outcome. Key questions remain unanswered: How much additional stress will structures experience under SLR? Claims about "local flow amplification" are not supported by model outputs.

Lines 387-388:

As noted, "Sediment transport capacity does not easily translate to sediment composition." Likewise, "D₅₀-based sediment classes cannot directly represent compositional heterogeneity; this should be acknowledged.

Lines 393-405:

Statements remain speculative ("might be" / "might not be"). These should be substantiated or rephrased as hypotheses.

Lines 406-427:

Regional examples alone cannot justify general conclusions. The discussion lacks evidence of SLR impacts on anthropogenic infrastructure within the results section, making these extrapolations unsupported

REPLY TO REVIEWER COMMENTS:

We sincerely thank all reviewers for their thoughtful and constructive feedback. The author team has carefully revised the manuscript in response to all comments and suggestions, and detailed replies are provided below. The original review comments are in black, the responses in purple italic. We hope that these revisions, together with the clarifications presented, fully address the reviewers' concerns and bring the manuscript to the standard required for publication in Nature Communications, Earth and Environment.

Reviewer #1 (Remarks to the Author):

This paper presents a modeling study in which the authors couple the Wavewatch III and NEMO model to assess how seabed stresses will change with climate change on the Northwest European Shelf. The paper presents novel research with seemingly sound methods, it is well written, and the graphics are nicely designed and easy to interpret. Overall, I think this is a high quality manuscript that will be fit for publication with minor revisions.

Thank you for this positive assessment, we are glad you liked our paper and our work. To ensure clarity, we would like to specify that the NEMO and WW3 models are run independently in this work, with the combined stresses calculated afterwards. From a numerical modelling perspective, this does not constitute a true coupling of the models. However, in some research contexts, combining the results in this way might be considered a form of coupling. We include this clarification to ensure that our methodology is clear.

Several minor questions occurred to me while reading it

1. As a reader, I want to know more about why this matters. It is very interesting from a theoretical perspective but I feel the discussion of management implications could be deeper.

- Any benthic species living in the area that are important for fisheries or other cultural reasons and might be impacted?

For this specific region, some important benthic species are sandeels, and their link to their wider food web, as seabirds. The work on the impact over benthic habitat and relationship to prey-predator relations will be explored as part of the ECOWind ACCELERATE program, some of it is already in progress (if interested, please see: <https://ecowind.uk/projects/ecowind-accelerate/>).

As this is out of scope for our modelling work we prefer to keep the discussion on possible impacts over the benthic habitat, fauna, or marine ecosystem as general. However, we have improved our discussion on the topic. Please see:

*lines (505– 539): ‘ **Another important broader implication in the context of this study is the way in which seabed stress may modify benthic habitats [...] need to be taken into account when carrying out future species distribution predictions.**’*

Please note that in response to other reviews, the discussion section has now been substantially restructured.

- "The rock grade chosen to keep infrastructure protected should not be mobilised in the next few decades, whilst modified mattresses to be nature-inclusive may not withstand intense currents [56]. Changes to sediment mobility can cause cage sinking and sediment movement in general calls for improved reef and housing designs to ensure long-term restoration success [57]." Some of these impacts are technical and niche. What are modified mattresses? And what is cage sinking? Would be nice to define these terms so the reader doesn't have to go look them up to understand why your work is important.

Thank you for pointing this out. We have added some definitions.

Lines 496-503: ‘The rock grade chosen to keep infrastructure protected should not be mobilised in the next few decades, whilst modified mattresses (a different type of rock armoring used in OWF foundations [87]) to be nature-inclusive may not withstand intense currents [88]. Changes to sediment mobility can cause cage sinking, where protective cages used to contain restoration substrates (e.g. oyster shells or other habitat material) become partially buried due to sediment movement. This, in general, calls for improved reef and housing designs to ensure long-term restoration success [89].’

*[87] Chambel, J. et al. A comprehensive review on scour and scour protections for complex bottom-fixed offshore and marine renewable energy foundations. Ocean Engineering **304**, 117829 (2024). URL <https://doi.org/10.1016/j.oceaneng.2024.117829>.*

[88] (SOWEC), S. O. W. E. C. Collaboration for environmental mitigation and nature inclusive design (cemnid). Tech. Rep. A-100906-S00-A-REPT-007, SOWEC (2024).

[89] Robertson, M. et al. Exploring the role of offshore wind in restoring priority marine habitats (2021).

- More context on the offshore wind sites in the area would be helpful. How important are these sites for the energy supply in UK/Europe? Consider including the location of these sites on some of the maps presented in the paper for reference.

Thank you for the suggestion; to provide better context for offshore windfarms we have developed the discussion on the topic (and expanded it also to offshore infrastructures in general, not just OWF), please see:

Lines (468 – 504): ‘**Firstly, changes in sediment mobilisation can impact the development of offshore infrastructures [...] areas where future changes in seabed mobility will need to be taken in consideration.**’

Within the new references of that section, please note that we cite **(Spanier and Kuenzer, 2024)**, which offers an exhaustive review of current and projected marine offshore infrastructure, including offshore windfarms.

REFERENCE:

[77] Spanier, R. & Kuenzer, C. Marine infrastructure detection with satellite data-a review. Remote Sensing **16**, 1675 (2024). <https://doi.org/10.3390/rs16101675>.

We also mention effect of monopiles in line (414-416): ‘**For example, infrastructures such as monopiles are known to influence seabed stress [70, 71], potentially doubling the seabed drag coefficient in their wake and thereby increasing shear stress**’.

[70] Unsworth, C. et al. Getting bed shear stresses right in the wake of an object in telemac-3d 1–7 (2024). Peer-reviewed conference paper, XXXth TELEMAC User Conference, Chambéry, France, 8 Oct 2024.

[71] Austin, M. J. et al. Enhanced bed shear stress and mixing in the tidal wake of an offshore wind turbine monopile. Ocean Science **21**, 81–91 (2025).

- This may be out of scope, but I am curious how the seabed stresses you describe here compare to the forces of fishing practices that disturb the seafloor?

Thanks, this raises a good point about the relative impacts of OWF development vs. fishing in the context of natural variability. We have added information on this topic to the discussion.

lines (412 – 421): ‘**These natural wave and current stresses are smaller in magnitude than the anthropogenic stresses generated by seabed structures and fish trawling. For example, infrastructures such as monopiles are known to influence seabed stress [70, 71], potentially doubling the seabed drag coefficient in their wake and thereby increasing shear stress [71]. Another example is bottom-contact fishing, which is one of the strongest human disturbances to the seabed. Each pass of a bottom trawl can penetrate 1–10 cm into soft sediments [72]. The most impactful gear (beam trawls) can produce short-lived spikes of up to 20–40 N m⁻². The most widespread approach**

(using Otter trawls) can generate trawl-induced bed shear stress values of $1\text{--}10\text{ N m}^{-2}$ [73].'

[70] Unsworth, C. et al. Getting bed shear stresses right in the wake of an object in telemac-3d 1–7 (2024). Peer-reviewed conference paper, XXXth TELEMAC User Conference, Chambery, France, 8 Oct 2024.

[71] Austin, M. et al. Enhanced bed shear stress and mixing in the tidal wake of an offshore wind turbine monopile. *Ocean Science* 21, 81–91 (2025).

[72] Bruns, I. et al. Physical impact of bottom trawling on seafloor sediments in the german north sea. *Frontiers in Earth Science* 11, 1233163 (2023). URL <https://www.frontiersin.org/articles/10.3389/feart.2023.1233163/full>.

[73] O'Neill, F. G. & Summerbell, K. The mobilisation of sediment by demersal otter trawls. *Marine Pollution Bulletin* 62, 1088–1097 (2011). URL <https://www.sciencedirect.com/science/article/pii/S0025326X11001316>.

2. Some questions about validation

- Based on the presented info, it seems like the model had good agreement without tuning any parameters (e.g. there is a "Validation" period but no "Calibration" period). Is that so? If another work developed a well-tuned model, that should be explained/cited. If tuning parameters were adjusted to maximize agreement, that should be explained. And if tuning parameters were adjusted to maximize agreement, I would think that should be done with one time series for calibration, and then compared to a separate time series for validation.

Thank you for pointing this out; the model hydrodynamics has been extensively validated by previous work on these configurations. The only 'new' parameter that we bring in here is the seabed stress, which is derived by previously validated and calibrated parameter (current and orbital velocities). Because of this we validate our seabed stress, but do not re-validate and calibrate most of the other model variables as this has already been done. We have now explicitly specified this in the method section.

Lines (603-605): **'The underpinning hydrodynamic and wave models have been tuned and run in this AMM15 configuration, with previous calibration and validation studies published by [101, 102, 105, 106].'**

[101] Valiente, N. G. et al. The impact of wave model source terms and coupling strategies on rapidly developing waves across the North-West European Shelf during extreme events. *Journal of Marine Science and Engineering* 9, 403 (2021). <https://doi.org/10.3390/jmse9040403>.

[102] Lewis, H. W. et al. The UKC3 regional coupled environmental prediction system. *Geoscientific Model Development* **12**, 2357–2400 (2019).

[105] Lewis, H. W. et al. The UKC2 regional coupled environmental prediction system. *Geoscientific Model Development* **11**, 1–42 (2018).

[106] Rulent, J. et al. Comparing water level estimation in coastal and shelf seas from satellite altimetry and numerical models. *Frontiers in Marine Science* **7**, 549467 (2020). <https://www.frontiersin.org/articles/10.3389/fmars.2020.549467/full>.

- Curious about the "erroneous spikes" seen in the observations of seabed stress in fig A2. Do you believe the observations were made in error? If so, why? If not, then I think calling observations "erroneous" is misleading - the observations are not erroneous but rather your model is not capturing the observed spikes. If this is the case, I am curious if you have any ideas what processes your model might not be including that are causing the spikes.

The observed “spikes” in seabed stress are likely artifacts in the field data rather than true physical signals. Bed shear stress is derived from multiple observations, which can occasionally produce unphysical values, including extreme peaks or negative values. While smoothing techniques, such as a LOWESS filter, can reduce such spikes, in this study we have retained the raw observations to remain consistent with the original data. The spikes are therefore interpreted as observational noise, and the model’s inability to reproduce them reflects these differences rather than errors in the modelling approach.

- Consider adding location of the calibration/validation datasets to Fig 1.

Now added in appendix: figure A1, in p.27.

- Why are all validation time series from the Irish sea? This is only a small part of the study region. Are there tide gauges available in other areas? If so, why were those not included?

During the development of the UKC4 modelling system, the model has been extensively validated for waves and hydrodynamics all around the UK. Observations from tide gauges are available over other regions and have been used to validate the model in previous work (now cited in method, as per previous reply). As the Irish Sea is of particular interest to some of our future work, we have done some further validation of this region as part of this work to give context to the validation of seabed stresses in this region.

3. Questions about the model

- The paper states "The simulation setup is consistent with SLR estimates derived from the same global climate model and scenario." But why is 2050/2100 SLR higher for the "hindcast + SLR" vs the projections?

The difference reflects the experimental design and how SLR is applied and reported. In the hindcast + SLR experiments, only sea level rise is imposed, applied exclusively at the open boundaries, while atmospheric and oceanic conditions are kept fixed. In the projection experiments, spatially variable SLR is combined with fully evolving future weather and ocean conditions. In the latter, the low-pressure system can locally affect the sea level values, even if the overall sea level rise is the same (RCP8.5) in both experiments.

The sea level rise experiments are a sensitivity testing to the system, where estimates of SLR are derived from the work of (Palmer et al 2018); these do not include ocean dynamics effects. The projection runs are forced from the further developed work of (Tinker et al 2014), which do contain regional dynamics and are a regional downscaled run. Though both relate to RCP8.5 there are important differences. In order to clarify this in the text we have now created a clear distinction between our sensitivity testing to the water level increase and the projections.

The estimates of SLR for the sensitivity test were removed from table 1 to not confuse the reader and discussed in text instead. In section 2.3 we added:

*Lines 221 – 225: **'This includes contributions from surface mass balance and ice dynamics of the Greenland and Antarctic ice sheets, as well as from land-based glaciers and land water storage 8 (e.g. groundwater extraction or reservoir impoundment), but does not include ocean dynamic contributions [51].'***

[51] Palmer, M. et al. UKCP18 marine report. Tech. Rep., NERC Archive (2018). URL <http://nora.nerc.ac.uk/id/eprint/522257/>.

Moreover, we have re-worded part the caption of Figure 3, and part of the methodology (5.4 and 5.5) to clarify this. We hope this will help to make the distinction between the SLR sensitivity tests and the results on climate change projections clearer to the reader.

3. Since you specifically mention German Bight in the text, consider adding a label pointing it out in fig 3f.

Now added in appendix: figure A1, in p.27.

4. Did the authors develop equations 5 and 6? If not, cite. If so, consider adding an explanation on the theory of the equations.

No, these equations were developed by Soulsby et. al (2005). We have now added the citation in line 661 and 664, when referring to equation 5 and 6.

5. You say in the manuscript that coupling the wave/flow models online is an area for future research, and you also say that doing so is important for assessing shallow water impacts which are higher than deep water impacts. Curious why you chose to couple the current and wave models offline instead of online in this study? Did coupling online add significant computation time or something? What is the order of magnitude of that increase? And could you clarify a bit on what "shallow water" means here, i.e. in what depth of water are the findings of your work accurate given this limitation?

Thank you for your question, and yes, this was an important consideration when developing the study. There are several reasons why we opted to estimate the combined stress offline.

Firstly, the direct coupling of the model roughly doubles the computational time (Polton et al., 2023, section 4.2). This is a considerable increase in computational cost, to develop a method that comes with a substantial level of technical development when suitable methods are already available. Estimating the total seabed stress offline, though more conventional, is an established method that has the advantage of being more transferable as coupling numerical models comes with a great deal of implementation complexities. This can more easily be re-applied to already existing model outputs from other published research.

The method for combined stress defined in Soulsby (1997) is an empirical relationship treats waves and currents as dynamically uncoupled in their generation, but interacting within the bottom boundary layer. So while the interactions which modify waves and currents through coupling are not represented, their stresses combine nonlinearly near the seabed, even though the wave and current fields are computed separately. This interaction is what our paper is focusing on.

We do believe however that coupling could be an interesting development of this study. The impact of coupling waves to currents was investigated by Lewis et al., (2019a and 2019b). This study shows that the impact of ocean–wave coupling on the wave field is of order ± 0.01 m in significant wave height and ± 0.04 s in peak wave period (figure Lewis et al 2019.a figures 5(h) and 5(i)), in that case it is clear that the effects do not relate to water

depth. By contrast, the coupling of waves to currents is more strongly dependent on water depth (Lewis et al., 2019b), and this study shows a reduction in bias against observations in the simulated current field. Based on these studies, it appears that feedback between NEMO-WaveWatchIII would better capture the momentum transfer, likely improving results especially in very coastal areas within a few km from land. With more time and resources this would be a hypothesis to investigate in future work.

We now explain our motivation to work with offline estimation of combined stresses in lines, (548-550): **‘In this pilot study, we ran the model uncoupled and combined stresses offline to simplify the methodology, reduce technical complexity, optimise computation time, and maintain applicability.’**

We also suggest the use of a coupled model in future study and potential effects of the model coupling in lines (541-548) : **‘Further advancement could be achieved running a coupled NEMO-WaveWatchIII system, as opposed to combining the stresses offline. Live coupling would better capture the feedback interaction between wave and ocean components [100–103], as in this configuration we are missing the transfer of momentum between ocean and wave models. This is beneficial to accurately represent shallow waters and should be implemented in future work. Many of the largest impacts we observe are taking place in shallow water regions, where the hydrodynamic-wave coupling will be most impactful.’**

As well as (562-566): **‘For example, the wave energy dissipation is reduced, allowing more wave energy to reach coastal regions in some areas, resulting in a slight increase in seabed stress as demonstrated in this study for the German Bight. This is an unexplored area which could stimulate future work, and may benefit from a coupled wave and hydrodynamic model.’**

Polton, J. et al. Reproducible and relocatable regional ocean modelling: fundamentals and practices. *Geoscientific Model Development* **16**(5), 1481–1510 (2023). URL <https://doi.org/10.5194/gmd-16-1481-2023>.

Lewis, H. W. et al. The UKC3 regional coupled environmental prediction system. *Geoscientific Model Development* **12**, 2357–2400 (2019.a).

Lewis, H. W. et al. Can wave coupling improve operational regional ocean forecasts for the north-west european shelf? *Ocean Science* **15**, 669–690 (2019.b).

Reviewer #2 (Remarks to the Author):

Dear Authors,

Thank you for the opportunity to review your manuscript. The study addresses with rising sea level an important topic, and features very interesting modeling results focused on the continental shelf of the UK and their potential implications for coastal dynamics. This is a sensitive topic that is widely discussed in the community, and every contribution is important. The breakdown of stress factors (waves, tides, and currents) affecting the seabed was fascinating to read. Linking stress and sediment classes is a challenging method for gaining new insights into various future scenarios. The figures are very nicely laid out and presented, and it is very clear that the data processing has been carried out correctly. The modeling framework has the potential to contribute valuable insights into the response of shelf environments and hydrodynamic forcing. The proposed objectives are also not supported by the study presented, which is unfortunate, as a great deal of effort and potential has gone into the work. It would be better if the existing illustrations were incorporated more strongly and the results section focused more on the findings and less on speculation and conjecture. The discussion section would also benefit greatly from referring more to the very good results and being less speculative.

However, the current version of the manuscript presents several inconsistencies in the data interpretation and scientific argumentation style that need to be addressed before the work can be properly evaluated. Also the proposed aims (addressed in the details section) are not supported by the presented study. Although it is unfortunate, after careful consideration, I have decided to recommend to reject the article because, on the one hand, essential comparisons and connections are missing to justify the specified objectives and, on the other hand, a large part of the text is based on speculation. Given the sensitivity and complexity of climate change related research topic, interpretations should be presented cautiously and clearly, with transparency of the conclusions. It is important that projections or scenarios are discussed within their uncertainty bounds and referenced against established assessments (e.g., IPCC reports) to avoid overstatement. I put as much effort as possible into the review to make my decision understandable and to show my respect for the work done by the authors.

Sincerely,
Reviewer

Dear Reviewer,

Thank you for taking the time to review our work, and the thorough evaluation of the manuscript. We do appreciate your concerns regarding the original manuscript and are glad that, despite your reservations, you considered the topic and the results valuable.

To address your comment that 'The proposed objectives are also not supported by the study presented' and that the results section should be 'focused more on the findings and less on speculation', we have drastically restructured our discussion and presentation of the results. Our work does answer its objective, but we recognize that our communication of the work can be improved, especially when it comes to ensuring that the reader does not interpret speculative statements as facts. Firstly, We have changed the end of our introduction section to clarify what the scope of our work is, and clearly indicate what conclusions can be drawn with confidence:

*Lines (122 – 126): **'Quantifying climate-driven impacts, assessed against present-day variability, this study examines how seabed stresses respond to the isolated effect of SLR, how future storm conditions under an extreme climate scenario may further modify seabed stress, and how the resulting changes in ocean currents and seabed stresses could influence future sediment transport capacity.'***

We then explicitly re-state our objectives at the start of the discussion:

*Lines (396-400): **'Three main questions are addressed: How will the seabed stress be affected by the isolated effect of SLR? How will climate change (future storms) impact on future seabed stress in a case study extreme scenario? How can these projected changes in ocean currents and seabed stresses influence future sediment transport capacity?'***

Moreover, we have restructured the entire discussion to separate all aspects of the discussion that are direct implication of our results from information on broader concepts and ideas that, though important to point out, are not direct outcomes of this work. The section dealing with our results can now be found in:

*Lines (406-465): **'Results show that [...] extreme storm events remain far less predictable and may continue to exert a dominant and potentially disruptive influence.'***

The sections that involve any speculation are separated. We explicitly state that these do not contain direct results of our work. We have strengthened this part of the discussion with previous literature and peer review research to support our claims. This section can be found in:

*lines (466-539): **'Beyond the direct results of this study, possible consequences and related concepts are considered, which remain uncertain but may be important for***

understanding the broader context, the significance of these findings, and guiding future research.[....] However, the outcomes of this work give indications of potential changes in seabed drivers, and identify regions of the NWES that are exposed and need to be taken into account when carrying out future species distribution predictions.'

The discussion ends with a paragraph on limitations of our work, and possible future work that we believe to be important:

*lines (540-566): **This study has given a new understanding of future seabed mobilisation over the NWES and changes to the near-bed hydrodynamics. Further advancement could be achieved [...] This is an unexplored area which could stimulate future work, and may benefit from a coupled wave and hydrodynamic model.'***

Moreover, we agree that 'It is important that projections or scenarios are discussed within their uncertainty bounds and referenced against established assessments (e.g., IPCC reports) to avoid overstatement'.

In this work we base our future climate forcing on UK Climate Predictions 2018 (UKCP18) based on the work of Palmer et al., (2018), and the further regional downscaling development by Tinker et al., (2024). Because natural variability is so large, and the climate change signal is weak, our experiments are designed to identify large but possible impacts. We consider only RCP8.5 to maximise the climate change signal and improve our understanding of an 'extreme but possible' case.

The UKCP18 21st century time-mean sea level projections are based upon the 5th to 95th percentiles of the underlying model distributions. The global mean sea-level rise in this simulation ranges between 0.56 - 1.12m by 2100. To drive our model, the central (mean) value is used, taking a spatially varying value to force the open ocean boundary. (the equivalent for CMIP6 is 0.63-1.02 m under a high emission (likely range, SSP5-8.5).

The tides and associated stresses are highly predictable – with errors of 0.01-0.15m. The way sea-level rise impacts the tide is mechanistically more well-bound, so the uncertainty from the SLR projections will map directly to the confidence levels in the tidal projections.

The impact of storminess (and its potential to alter under climate change) on the other hand, is a much larger source of uncertainty. There is uncertainty about the strength and path of any individual storm, which will be the major local impact.

*Because of the nature of this paper focusing on individual extreme winters, the storms represent single events. While these events will not represent a robust statistical picture of the future stresses, they can be used as an end-member to demonstrate the *impacts* of a*

possible, but rare event. To capture the rare but possible climate-change driven impacts therefore, we chose a single large storm, to explore a worst-case plausible outcome.

Waves and storminess are subject to large natural variability, which can mask decadal natural variability, and anthropogenically driven change. However, we can contextualise this study in more statistically robust work, looking over multiple decades. For example. In a previous study of trends in an RCP8.5 driven 100-year wave model of the UK (Bricheno & Wolf 2018). Trends were predicted of the up to 0.3m (10%) in a reduction on the mean wave height. The extremes were predicted to both increase and decrease with a magnitude of +/- 1m or 20%. But crucially, the spatially varying impacts were due to the position of individual storm tracks.

We have now added and modified several sections of the paper to clarify this. There is now a section in the discussion written to explain our work in the context of RCP8.5 and what the uncertainty bounds of this work are in lines (445-465):

‘It is important to consider all these results within the context of the CMIP5 RCP8.5 projection, [...] extreme storm events remain far less predictable and may continue to exert a dominant and potentially disruptive influence.’

Further updates were introduced based on the detailed comments that you provided in a separate appendix file. Please see below for a detailed response to your individual comments.

We hope that our revision of the manuscript addresses your concerns. This is indeed a sensitive topic, and it is important that the nuances and caveats are communicated clearly and not taken out of context.

Sincerely,
Authors

Reviewer #2 – response to appended file

Comments

Line 47-48: The introductory statement is accurate but potentially misleading. While it is true that the global ocean, (especially with respect to seabed processes) is not yet fully understood, the main focus of this study is clearly on the continental shelf, with a regional emphasis on the North Sea. The manuscript frequently alternates between regional findings and global applications without providing a conceptual framework that justifies the generalization.

We agree with the reviewer that this framing is potentially misleading. To avoid misunderstandings, we have explicitly stated that our focus is on the North Western EU shelf, which we use here as a case study for processes that are of global importance. Please see 48-53:

‘The changing climate is having a rapid impact on the global ocean [1], particularly in coastal and shelf seas, yet few studies have investigated effects at the seabed [2, 3]. To study this phenomenon we focus on the regional area of the North Western European Shelf (NWES) as a case study; this region includes areas with diverse characteristics such as deep and shallow zones, varying tidal ranges, and differing waves and surge dynamics. These conditions can be found in other shelf seas globally.’

Line 55: “Larger storms increase energy transfer”: Although this is a plausible scenario, it has not been empirically confirmed and should be presented as a projection rather than a factual statement. Precision is essential when addressing climate change and sea-level rise (SLR) forecasts. Please crosscheck with the latest IPCC report to ensure consistency with established *climate scenarios*.

We have now expanded this section to crosscheck and contextualise with the latest IPCC report and further relevant research. Please see lines 60 to 70:

‘Storms transfer energy to the seabed and increase sediment mobility (e.g. [8], [9]). Any increase in storminess will grow wave height and period, thus increasing wave energy, and associated the seabed exposure. The last two IPCC assessment reports [10–12] concluded that projections of future storminess remain uncertain. State of the art climate models (CMIP5, CMIP6) show a northward extension of the North Atlantic storm track, which could lead to increased frequency of windstorms over western

Europe (e.g., [13]), with larger increases at higher global warming levels (e.g., [14]). [15] predicts that UK storm severity could increase by around 30% by 2080, mainly due to storms covering larger areas. However, few studies focus on changes in storminess at mid-century [16] as large natural variability makes it difficult to detect clear trends in storminess.'

[8] Hequette, A. & Hill, P. R. Response of the seabed to storm-generated combined flows on a sandy Arctic shoreface, Canadian Beaufort Sea. *Journal of Sedimentary Research* 65, 461–471 (1995).

[9] Clukey, E. C. et al. Wave-seabed observations and a combined approach to determine required burial depth of offshore power cables under large storm waves and currents. *Ocean Engineering* 285, 115356 (2023).

[10] Christensen, J. H. et al. in *Climate phenomena and their relevance for future regional climate change* (eds Stocker, T. F. et al.) *Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change* Ch. 14, 1217–1308 (Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 2013). URL https://www.ipcc.ch/site/assets/uploads/2018/02/WG1AR5_Chapter14_FINAL.pdf.

[11] IPCC. in *Summary for policymakers* (eds Stocker, T. F. et al.) *Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change* (Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 2013). URL <https://www.ipcc.ch/report/ar5/wg1/>.

[12] Seneviratne, S. I. et al. in *Weather and climate extreme events in a changing climate* (eds Masson-Delmotte, V. et al.) *Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change* 1513–1766 (Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 2021).

[13] Harvey, B. J. et al. The response of the northern hemisphere storm tracks and jet streams to climate change in the cmip3, cmip5, and cmip6 climate models. *Journal of Geophysical Research: Atmospheres* 125, e2020JD032701 (2020).

[14] Feser, F. et al. Storminess over the North Atlantic and northwestern Europe—a review. *Quarterly Journal of the Royal Meteorological Society* 141, 350–382 (2015).

[15] Priestley, M. D. et al. Forced trends and internal variability in climate change projections of extreme European windstorm frequency and severity. Quarterly Journal of the Royal Meteorological Society 150, 4933–4950 (2024).

[16] Little, A. S. et al. Future increased risk from extratropical windstorms in northern Europe. Nature Communications 14, 4434 (2023).

Line 74-75: This sentence is circular; it effectively states that “something determines how it acts”. The cause-and-effect relationship needs clarification.

This sentence has been removed.

Line 76-77: “The need for maps [...] has been repeatedly flagged as a limitation”, The need for maps represents a research gap rather than a limiting factor. Please rephrase to clarify this distinction.

*The sentence was rephrased in line 88 to 91 : ‘**The need for detailed maps of seabed sediment types and seabed currents has been repeatedly flagged when investigating the suitability of sites for developing offshore renewable energy (e.g. windfarms), where accelerated development of infrastructure can occur to cut carbon dioxide emissions [34, 35].**’*

Line 85-88: The term “future-proof management” requires definition. Which “climate-related drivers” are included in this study? Does the manuscript address general anthropogenic impacts, or are references limited to the wind farm case study?

*This sentence has been adjusted to include a brief definition; however, this is still a general introduction giving a broad context to the work therefore we believe it is more suitable not to add too much information here that should be in the discussion. Please see line 97-101: ‘**To underpin marine spatial management that is future-proof, therefore able to withstand changes in storminess, seabed and the seabed environment, future changes from climate-related drivers need to be considered [44] [45], and the impact on the bed stress from the infrastructure needs to be contextualised with the impact from accelerating climate drivers of change.**’*

Line 92-96: The statement conflates global-scale assumptions with regionally variable processes, which reduces scientific precision. A clearer separation between global trends and regional dynamics is necessary. Even if the described outcome is the most likely scenario, it should be explicitly identified as a hypothesis rather than a confirmed result.

We have now explicitly reminded the reader that while these changes do occur globally, we focus on a regional analysis. Our hypothesis is also explicitly highlighted. Please see line

104 – 112:

*These global changes **will trigger regional impact**, and affect shallower regions exposed to northerly winds [47]. **Based on our hypothesis**, changes in surface wave height, fetch, period, and direction are thus expected to combine with SLR-modified seabed currents and alter seabed stresses, seabed mobility and the ecosystems that depend on it across all trophic levels. Changes like these from the accelerating climate crisis will occur globally, **with localised impact over regions like the NWES used in this study**; there is an urgent need to find a way to predict the consequences for energy transfer to the seabed and the consequences on wider ecosystems.'*

Line 102: "Realistic mid- and end-century climate scenarios", Please provide context for what qualifies as "realistic". Why not consider high-end scenarios, such as those described by Dayan et al. (2021), 'High-End Scenarios of Sea-Level Rise for Coastal Risk-Averse Stakeholders'? These could support long-term, risk-averse spatial planning. Clarify what distinguishes the proposed approach from such existing frameworks.

Thank you for flagging the Dayan et al 2021 research; we agree with the reviewer it would be extremely interesting to consider these high-end scenarios in future work, and this could be developed into a further proposal. However, there are several reasons why it was advantageous to refer to the RCP8.5 scenario for our work: The RCP8.5 scenario is already deeply embedded in the literature as high end-forced pathway, making it more accessible to a wider audience. It is a good scenario to use because it is a deliberately extreme, physically coherent stress test that maximizes detectability in a space with high natural variability, illuminates tail risks and bounds uncertainty (particularly when we use the single large storm approach). Because the RCP8.5 has been more broadly used, we also had downscaled forcing readily available to run model simulations based on previous work from Palmer et al., (2018); downscaling or generating suitable high-resolution forcing is a work on its own so this allowed us to generate a new simulation for a suitable scenario, that is more compatible with several studies available already referring to the RCP 8.5. Further developing the study into more complex setups is always possible but the basics need to be set, and our study is an important step forward to understand the effect of SLR and climate change onto seabed processes. We believe that the RCP8.5 scenario was in this case the most useful next step into developing the understanding of these processes.

Based on the reviewer's comment, we have clarified our thinking in the paper by removing the word 'realistic' and changing the text Lines 114 – 120:

'For the first time, the expected magnitude of future changes to seabed stresses and sediment transport capacity (i.e. the coarsest sediment the flow can mobilise, regardless of actual seabed composition) in space and in time is quantified from

Representative Concentration Pathways (RCP8.5) mid- and end-century climate scenarios. This is a widely used high-emission scenario that to our knowledge has not yet been used for this type of study.'

And explicitly adding (line 557 – 560): **'Moreover, we have focused here on the widely used RCP8.5 projections; this could be further developed by considering high-end sea-level scenarios that target regional, unlikely but not impossible, extreme sea-level changes even more explicitly (e.g. [104]).'**

Also we included a section in the discussion to reiterate that we refer to RCP8.5 and what the uncertainty bounds of this work are in (445-465):

'It is important to consider all these results within the context of the CMIP5 RCP8.5 projection, used here on a downscaled regional application [51, 74], and the associated uncertainties. This work provides an understanding of seabed stress and transport capacity changes in an extreme scenario case, therefore it improves our understanding of the tail risk of a specific physical projection; these results do not show a statistical characterisation of future conditions. There is high confidence on the effect of tides, that in present-day simulations can contribute up to 3 N m^{-2} to the total combined stress during spring tides in the NWES (tidal range of up to 12 m). There is less confidence in the effects of present-day storms, which, although generally well understood, involve short-timescale processes that remain complex to predict with high natural variability. In this work's simulation storms contribute up to 12 N m^{-2} to the total stress. When interpreting the projected results, there is high confidence within the scientific community that sea level will rise, with UKCP18 indicating an uncertainty range of 0.56–1.12 m [51]. This study shows that SLR can lead to a reduction of up to 1 N m^{-2} in the total combined tide and wave driven seabed stress under average conditions. However there is low confidence on the state of future climate and storms, which we show are expected to contribute up to 15 N m^{-2} to the seabed stress. Therefore, while SLR and associated background changes are expected to produce largely predictable, predominantly reduced seabed stresses, extreme storm events remain far less predictable and may continue to exert a dominant and potentially disruptive influence.'

We hope this addresses the reviewer's concern.

[51] Palmer, M. et al. UKCP18 marine report. Tech. Rep., NERC Archive (2018). URL <http://nora.nerc.ac.uk/id/eprint/522257/>.

[74] Tinker, J. et al. Twenty-first century marine climate projections for the nw european shelf seas based on a perturbed parameter ensemble. *Ocean Science* 20, 835–885 (2024).

[104] Dayan, H. et al, High-end scenarios of sea-level rise for coastal risk-averse stakeholders. *Frontiers in Marine Science* 8 (2021). URL <https://doi.org/10.3389/fmars.2021.569992>

Line 103-104: Clarify what is meant by "climate-related and dominant driver". Specify how these drivers were selected and parameterized.

Yes, we have now explicitly stated:

*(line 120-122): ‘we isolate physical controls on the seabed from **sea level rise and storms** to map the dominant drivers per region.’*

Lines 100-101: The objective to "quantify sediment transport capacity in space and time" is not fully achieved. Compare with line 291 ("we calculate which maximum median grain sizes would be mobile for a given bed shear stress") and line 388 ("Sediment transport capacity does not easily translate to sediment composition"). The internal logic between these statements remains unclear.

Here, sediment transport capacity is defined as the coarsest grain size that the flow can mobilise, whereas sediment composition refers to the actual sediment type present at the seabed. In this study, we quantify transport capacity only, that is, the maximum sediment size that can be moved based on the modelled seabed stress relative to the critical threshold for sediment motion. We do not aim to represent sediment composition, namely the sediment types that are actually present at the seabed in reality. Based on the reviewer’s comment, it appears this distinction has not been suitably clarified in the manuscript. To better explain this, we have added the following definition when first mentioning transport capacity:

*(line 116): ‘**i.e. the coarsest sediment the flow can mobilise, regardless of actual seabed composition.**’*

Lines 104-107: Seabed infrastructure is discussed hypothetically in the context of a wind farm case study, yet no clear structural parameters or SLR-related stress interactions are provided. "Environmental health" is mentioned but not substantiated with data in the results section. Consequently, the findings cannot be directly linked to marine spatial planning recommendations.

Yes, the reviewer is correct, and we do not claim that our finding demonstrates the effect of seabed infrastructure or show changes to the environmental health directly; our mention of these topics is in the introduction and discussion, not in the results section.

These topics are mentioned on a discussion basis, as we believe they are important concepts that relate to what is studied in this work. The ECOWind ACCELRATE program is broad, and the shelf-scale results presented here are part of a more complex network of work packages which will in address these concepts in detail. We discuss these topics as a way to introduce the work of our colleagues (in progress) that will examine the effect of windfarm structure on specific regions at much higher resolution, and investigate the effects on benthic habitat and environment, but also as a way to help the reader understand the context in which these result matter, and explore concepts and idea that can be developed from this base.

We realise based on the reviewer's comments that our quote is misleading for the reader that is unfamiliar with our program. In order to clarify our manuscript and avoid misunderstanding, we removed this sentence and changed it to only specify what our research does answer. Please see:

*Lines (122 – 126): **'Quantifying climate-driven impacts, assessed against present-day variability, this study examines how seabed stresses respond to the isolated effect of SLR, how future storm conditions under an extreme climate scenario may further modify seabed stress, and how the resulting changes in ocean currents and seabed stresses could influence future sediment transport capacity.'***

*The references to offshore infrastructure and the broader program are now contained to parts of the discussion, in paragraphs that we explicitly state are speculative and not directly supported by our work or related to future work. Please see lines 466-504: **'Beyond the direct results of this study [...] Our results point towards hotspots of areas where future changes in seabed mobility will need to be taken in consideration.'***

*Our broader project is now only mentioned after all discussion as to not create confusion, please see lines (567-571): **'Part of the future work outlined above is already underway as part of the ECOWind ACCELERATE project (<https://ecowind.uk/projects/ecowind-accelerate>), as for example the inclusion of OWF infrastructure on top of the climate change impact discussed in this paper, or the effect of changes over benthic ecosystem and seafloor habitat.'***

Lines 107-111: The manuscript does not explain which results or methods have global applicability, or why. Tidal evolution under SLR is known to depend strongly on regional factors, yet the interaction between shifting coastlines and tidal-current dynamics is not addressed. This is essential for understanding potential amphidromic point migration, which the manuscript references but does not substantiate.

We show a globally applicable method – we apply it to a regional sea. Regional changes matter, and we look at the UK case study specifically because it has some of the most extreme tides as well as representative conditions for extreme waves and regions prone to extreme surges; this covers a number of important conditions. To address this we have adapted the text as below.

(lines 126 – 132): ‘We present a new, globally applicable method employed here in a NWES case study as it has the opportune combination of being exposed to a wide variety of conditions (intense storms, large magnitude waves and tides [46]) and being one of the most industrialized shelf seas in the world [48, 49]. This indicates that this region reflects a broad range of conditions present in other parts of the world and can provide insights for other rapidly developing shelf areas.’

Figure 1: Move the figure labels (a-f) to the top right and the year/month or season to the bottom left for clarity. Also in the color bar label " $(Nm\hat{A}^2)$ ", a space is missing.

We have now moved the labels (a-f) to the top left, added the year/month and max or mean to the bottom right, and corrected the label typo in Figure 1. We hope this improves readability.

Although mentioned in the caption, please specify in the main text (around lines 130-132 and 146-147) which values are measured versus modelled.

Yes, we have now explicitly added:

*Line 152 : ‘**modelled** current speeds’*

*Line 171: ‘The **modelled** wave generated seabed stresses’*

The introductory sentence, "Any changes in bed stress brought about by climate change or seabed infrastructure must be put into the context of natural variability" is valid, but no baseline context is provided on current sediment transport, anthropogenic impacts, or seabed conditions.

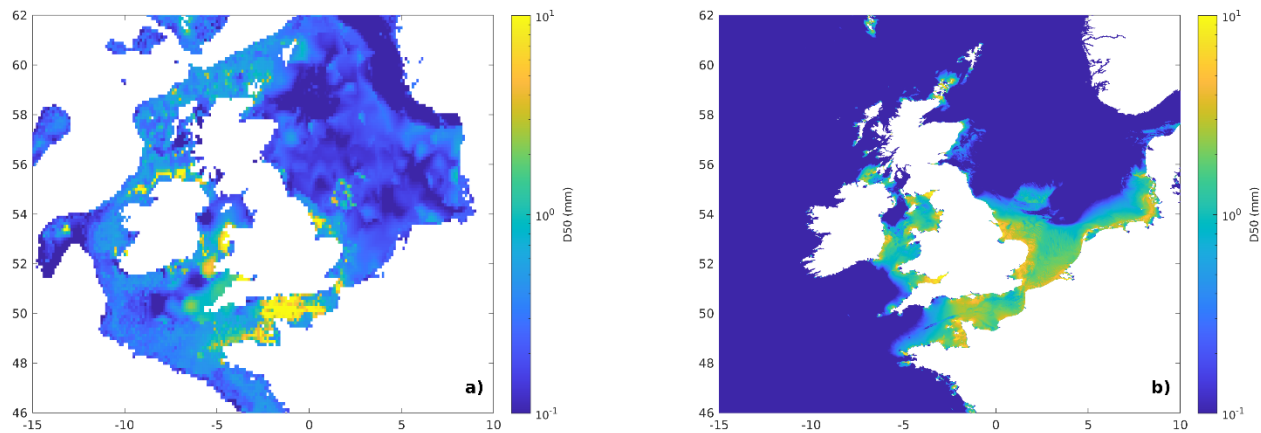
We have now added an appendix showing a comparison between observational dataset of D50 from Wilson et al.,(2018) and the D50 derived from our model. Please see appendix A.3. This is now referenced in :

*Line 761: ‘**Please note that further details on the validation of this method for estimating D50 from the modelled seabed stress is provided in appendix A.3’***

The Appendix can be found from line 813 and states:

'In order to validate our method of estimation of median grain size, D_{50} , from the model results based on the equations described in section 5.5, we here show the comparison between values of D_{50} derived from the work of [40] based on observation (figure A4a) the D_{50} values derived from our numerical modelling simulations (figure A4b). While these quantities are not directly comparable, the sediment present on the seafloor is expected to broadly correspond to the coarsest grain size that can be mobilised, since finer material below the threshold of motion is preferentially transported away, leaving behind the largest immobile fraction. This correspondence is not guaranteed, as the material present on the seafloor may be entirely immobile or composed of different sediment types. Nevertheless, the broad agreement between the two patterns provides a useful indication of consistency between observations and model derived estimates. Moreover, these plots illustrate how the model operates at relatively high spatial resolution compared to other available products, which supports its usefulness in capturing spatial variability in sediment mobilisation despite the inherent simplifications.'

Figure A4:



Line 120-121: "Large but predictable variability in seabed stress is generated over the 28-day lunar cycle.", This is region-specific and not globally applicable. The phrase "large but predictable" is vague and should be replaced with a quantifiable or referenced statement.

Yes, it is regional. The results are not global, and quantification refers to our case study, but the physics is true everywhere. The phrase 'large but predictable' is introductory and details on quantified values are presented in the paragraph below, lines 148-158. To clarify this we have added 'In our case study area, as discussed in more detail below,' to line 140-141, so it is clear to the reader that we do quantify this, and that its results are only referred to our case study, the NWES.

Line 129-131: For the "Anglesey, St David's Head" example, please clarify whether current speed values are modeled or measured. Also, explain why these locations are representative. Numerical values are provided without contextual comparison (e.g., no corresponding min/max values for storm-induced stress).

*Yes, it is modelled. Now specified in line 152: 'For example, at Anglesey, St David's Head, and Cherbourg **modelled** current speeds reach [...]'*

This location is chosen as an example of a high tidal range region with fast current as mentioned in the previous line. We also chose them based on the wider interest of the ecowind accelerate project. We'll be working on the Irish Sea in the rest of the project.

Line 137-139: There is an inconsistency between text and Figure 1. The text reports spring tide bed stresses up to 8 N m^{-2} , while the storm-event color bar is truncated at 6 N m^{-2} . Please reconcile this discrepancy.

*We have explicitly stated that the colourbar is saturated in the caption of Figure 1. To be even more explicit we have now updated the caption of Figure 1 to: '[...]NB the colour scale is saturated at 6 N m^{-2} for visualisation purposes, **though the maximum stress modelled reaches up to 8 N m^{-2} .**'*

Line 140: "Show rather different spatial patterns", specify in what respect the patterns differ (magnitude, distribution, directionality, etc.).

*Ok, we have specified this in line 162 – 165: ' [...] **show rather different spatial patterns; the mean shows dominant stress signals over high tidal range areas, whereas the maximum (that can only be reached during storm times, i.e. strong waves) becomes enhanced over areas exposed to the Atlantic as well as over the shallow regions of the North Sea**'.*

Please note that the lines following this paragraph also included some text regarding the different special distribution, however after the reviewer's comment we realise these may not have been explicit enough. We hope our changes improved the text.

Line 144: What is meant by "there are some commonalities"? The argument becomes difficult to follow. Please specify what commonalities are being referred to and between which variables.

*The commonalities are stated in the following sentence. To clarify this, we have now linked the sentences as follow (now in line 168-171): '**While the exact pattern of these wave generated stresses will be dictated by the path of an individual storm track, there are***

some commonalities; storm-generated wave stresses are largest on Atlantic-facing west coasts, and in areas of shallow water.'

Figure 2: Panels 2b and 2d present annual trends as bar plots. Consider selecting representative regions from 2a and plotting mean stress versus month with error bars (min/max). This would make lines 175-184 easier to follow.

This map does not show trends, this is not stated anywhere but based on the reviewer's comment we have now rephrased the caption of Figure 2. We have not altered the plot as we believe that adding information will make the content more confusing; the plot currently targets what we want to highlight in the paper. The clarified label now reads :

'Map showing areas selected for regional analysis (a), regional dominance of wave vs. current stresses during January 2017 (c), bar charts illustrating the mean stress contribution ($N\ m^{-2}$) for each region defined in panel (a), for winter (b) and summer (d). Each bar is decomposed into its stress components to highlight seasonal changes in mean and maximum stress derived from the WaveWatchIII model wave and NEMO currents constituents.'

The section heading "Drivers" is not reflected in the text. Clearly define what qualifies as a "driver".

Ok, we have now updated the title of 2.2 to 'Relative importance of storms and tide as drivers of sea bed stress'.

Water depth (line 162) is mentioned but not discussed further.

We have rephrased this to (line 187-189): 'Beyond the continental shelf, seabed stresses are relatively weak and predominantly current-driven, as increasing water depth prevents surface waves from transmitting momentum to the seabed.'

Line 163-164: Clarify whether this regional breakdown approach is transferrable to other continental shelf regions globally.

No, not directly as the regional decomposition has been based on previous publication from (Wakelin et al., (2020)), specifically for the North Western European shelf region. However, the method they used to divide the coast based on local circulation pattern could be re-used to split other shelf areas in relevant regions. This, however, is beyond the scope of our paper. To clarify, we have changed the text to:

*Lines 190-191 : '[...] the NWES domain is broken down into **areas** following the approach of [50] specific to this shelf region.'*

Summary (line 167-170): The terminology "processes", "contributors", and "drivers" appears interchangeable but should be explicitly defined. Consistency will improve clarity.

*We have rephrased this in line 195 - 198 : ‘**By compiling the regional contributions of each seabed-stress driver (i.e. tide and wave derived stresses), we can rank and map their relative importance, visualising, to first order, which drivers should be considered when estimating seabed stress.**’*

Line 196-197: The meaning of "non-linear" is unclear, does it refer to spatial or temporal variability (or both)? Please specify.

Changed ‘non linear’ to ‘spatially varying’ (line 231).

Lines 198-204: The mechanism of amphidromic behavior is addressed via reference [4], but here the discussion should focus on coastline shifts due to SLR, not solely on bathymetric change.

*The coastline is fixed throughout the simulation. SLR increases the water depth, it is our only driver on altering amphidrome / tide behaviour here. We have now explicitly specified this in the method, line 693: ‘The NEMO model bathymetry **and coastline** are held constant at the present day level’.*

Line 210: The paragraph on tidal dissipation lacks quantitative support. Statements such as "SLR decouples currents from the seabed" require justification through an energy budget or comparable analysis. See Lu et al. (2024) for reference on tidal energy dissipation.

We have now updated this in lines 246-251:

*‘**SLR decouples surface currents from the seabed, as discussed by [53–55]. Briefly, the increased water depth reduces the direct influence of bottom friction on surface flows so that surface currents respond more strongly to wind forcing and pressure gradients. As a result, momentum transfer to the seabed is weakened, partially decoupling surface currents from benthic dynamics.**’*

[53] Haigh, I. D. et al. The tides they are a-changin’: A comprehensive review of past and future nonastronomical changes in tides, their driving mechanisms, and future implications. *Reviews of Geophysics* **58**, e2018RG000636 (2020).

[54] Talke, S. A. & Jay, D. A. Changing tides: The role of natural and anthropogenic factors. *Annual Review of Marine Science* **12**, 121–151 (2020).

[55] Khojasteh, D. et al. A global assessment of estuarine tidal response to sea level rise. *Science of the Total Environment* **894**, 165011 (2023).

Lines 214-220: For statements such as "of the order 0.3 N m^{-2} ", please include a figure reference or data source to aid traceability.

Cited figure 3e in line 253.

Lines 232-247: The discussion of SLR effects on long-period swell waves lacks mechanistic detail. Claims such as "swell waves cannot reach the seabed as easily" or "wave stresses are reduced almost everywhere" are speculative without quantitative validation. Provide theoretical or numerical evidence, and clarify which wave components are affected. As written, the cause-effect reasoning remains uncertain.

The statement "wave stresses are reduced almost everywhere" is demonstrated by figure 3f; results of this experiment shows that the seabed stress is reduced when adding the SLR. We consider the differences between a hindcast simulation and the same run with increased water levels at the boundaries. The change in water level is the only forcing altered in the simulation and therefore is the mechanistic trigger of the effect seen in the two simulations differences.

The process in the spectral wave model, is that water depth modifies the dissipation source term by activating additional shallow-water loss mechanisms, therefore changing bottom friction dissipation and depth-induced breaking. The theoretical explanation of these mechanisms are further detailed in e.g. Komen et al (1994).

We have now added the following line to guide the reader looking for more information in line 277 - 279:

'In the spectral wave model, water depth alters dissipation by activating shallow-water loss mechanisms, consequently changing the bottom friction dissipation, as described in more detail in [56].'

[56] Komen, G.J. et al. Dynamics and modelling of ocean waves (Vol. 532). UK: Cambridge university press (1994).

Line 250: "The least uncertain facet of climate change" is an overgeneralization unsupported by quantitative context. Confidence and magnitude are distinct and should not be conflated.

We have clarified what we mean in line 291-293:

'SLR is the least uncertain facet of climate change as there is evidence that it will almost certainly keep rising at a rate that may accelerate [57-61].'

[57] Nicholls, R. J., and Cazenave, A. (2010). Sea-level rise and its impact on coastal zones. Science, 328 (5985), 1517–1520. <https://doi.org/10.1126/science.1185782>

[58] Haigh, I. D., Wahl, T., Rohling, E. J., Price, R. M., Pattiaratchi, C. B., Calafat, F. M., and Dangendorf, S. (2014). Timescales for detecting a significant acceleration in sea level rise. *Nature Communications*, 5, 4635. <https://doi.org/10.1038/ncomms4635>

[59] Nerem, R. S., Beckley, B. D., Fasullo, J. T., Hamlington, B. D., Masters, D., and Mitchum, G. T. (2018). Climate-change–driven accelerated sea-level rise detected in the altimeter era. *Proceedings of the National Academy of Sciences of the United States of America*, 115(9), 2022–2025. <https://doi.org/10.1073/pnas.1717312115>

[60] Oppenheimer, M., Glavovic, B. C., Hinkel, J., van de Wal, R., Magnan, A. K., Abd-Elgawad, A., Cai, R., Cifuentes-Jara, M., DeConto, R. M., Ghosh, T., Hay, J., Isla, F., Marzeion, B., Meyssignac, B., and Sebesvari, Z. (2019). Sea level rise and implications for low-lying islands, coasts and communities. In H.-O. Pörtner et al. (Eds.), *IPCC Special Report on the Ocean and Cryosphere in a Changing Climate* (pp. 321–445). Cambridge University Press. <https://doi.org/10.1017/9781009157964.006>

[61] Fox-Kemper, B., Hewitt, H. T., Xiao, C., Aðalgeirsdóttir, G., Drijfhout, S. S., Edwards, T. L., Golledge, N. R., Hemer, M., Kopp, R. E., Krinner, G., Mix, A., Notz, D., Nowicki, S., Nurhati, I. S., Ruiz, L., Sallée, J.-B., Slangen, A. B. A., and Yu, Y. (2021). Ocean, cryosphere and sea level change. In V. Masson-Delmotte et al. (Eds.), *Climate Change 2021: The Physical Science Basis* (pp. 1211–1362). Cambridge University Press. <https://doi.org/10.1017/9781009157896.011>

Line 258: Clarify the use of "end-member experiment". If the scenario represents an upper bound, justify it and relate to comparable high-end studies (e.g., Dayan et al., 2021).

Please see changes to the introduction and methodology where we now clarify our use of end member experiments:

*(line 114 – 120): ‘For the first time, the expected magnitude of future changes to seabed stresses and sediment transport capacity (i.e. the coarsest sediment the flow can mobilise, regardless of actual seabed composition) in space and in **time is quantified from Representative Concentration Pathways (RCP8.5) mid- and end-century climate scenarios. This is a widely used high-emission scenario that to our knowledge has not yet been used for this type of study.**’*

*And explicitly adding (line 557 – 560): ‘**Moreover, we have focused here on the widely used RCP8.5 projections; this could be further developed by considering high-end sea-level scenarios that target regional, unlikely but not impossible, extreme sea-level changes even more explicitly (e.g. [104]).**’*

We also discuss the uncertainty bound of our research in lines (445-465):

‘It is important to consider all these results within the context of the CMIP5 RCP8.5 projection, used here on a downscaled regional application [51, 74], and the associated uncertainties. This work provides an understanding of seabed stress and transport capacity changes in an extreme scenario case, therefore it improves our understanding of the tail risk of a specific physical projection; these results do not show a statistical characterisation of future conditions. There is high confidence on the effect of tides, that in present-day simulations can contribute up to 3 N m^{-2} to the total combined stress during spring tides in the NWES (tidal range of up to 12 m). There is less confidence in the effects of present-day storms, which, although generally well understood, involve short-timescale processes that remain complex to predict with high natural variability. In this work’s simulation storms contribute up to 12 N m^{-2} to the total stress. When interpreting the projected results, there is high confidence within the scientific community that sea level will rise, with UKCP18 indicating an uncertainty range of 0.56–1.12 m [51]. This study shows that SLR can lead to a reduction of up to 1 N m^{-2} in the total combined tide and wave driven seabed stress under average conditions. However there is low confidence on the state of future climate and storms, which we show are expected to contribute up to 15 N m^{-2} to the seabed stress. Therefore, while SLR and associated background changes are expected to produce largely predictable, predominantly reduced seabed stresses, extreme storm events remain far less predictable and may continue to exert a dominant and potentially disruptive influence.’

[51] Palmer, M. et al. UKCP18 marine report. Tech. Rep., NERC Archive (2018). URL <http://nora.nerc.ac.uk/id/eprint/522257/>.

[74] Tinker, J. et al. Twenty-first century marine climate projections for the NW European Shelf Seas based on a perturbed parameter ensemble. *Ocean Science* **20**, 835–885 (2024).

[104]. Dayan, H., Le Cozannet, G., Speich, S., Thiéblemont, R. High-end scenarios of sea-level rise for coastal risk-averse stakeholders. *Front. Mar. Sci.* **8**, 569992 (2021).

Lines 251-252: "This is much more uncertain", if uncertainty is recognized, quantify or discuss its range in the results or discussion section.

We have now changed (and developed) this in lines 294-298:

‘However, there is less certainty with regards to the magnitude and speed of the sea level increase [57, 62, 63]. Similarly, there is limited confidence in projections of future storm intensity and frequency [64–67], making their impact on seabed stress uncertain, as it will depend on the characteristics of individual extreme events as we explore in section 2.4.’

[57] Nicholls, R. J. & Cazenave, A. Sea-level rise and its impact on coastal zones. *Science* 328, 1517–1520 (2010).

[62] World Meteorological Organization. Future sea-level rise is certain, but the amount and speed are uncertain. *WMO Bulletin* 74 (2025).

[63] Golledge, N. R. Long-term projections of sea-level rise from ice sheets. *WIREs Climate Change* 11, e634 (2020).

[64] Della-Marta, P. M. & Pinto, J. G. Statistical uncertainty of changes in winter storms over the north atlantic and europe in an ensemble of transient climate simulations. *Geophysical Research Letters* (2009). First published: 17 July 2009.

[65] Catto, J. L. et al. The future of midlatitude cyclones. *Current Climate Change Reports* 5, 407–420 (2019). Published online: 15 November 2019

[66] Severino, L. G. et al. Projections and uncertainties of winter windstorm damage in europe in a changing climate. *Natural Hazards and Earth System Sciences* 24, 1555–1578 (2024). Published: 03 May 2024.

[67] Priestley, M. D. K. & Catto, J. L. Future changes in the extratropical storm tracks and cyclone intensity, wind speed, and structure. *Wind Climate Dynamics* 3, 337–360 (2022). Published: 31 March 2022.

Lines 255-257: Please refer to Section 5.5 for detailed information as suggested.

Done, now in line 304.

Line 283: "Using the new bed shear stress maps", Reference the corresponding figure for clarity.

Done, now in line 331.

Line 288: When discussing "grain size on the shelf", consider also mentioning sediment composition, biological components, and particle sorting.

For simplicity this exercise assumes that sediment is non cohesive (see lines 293 in original manuscript) and without biological components

We have now added the information earlier and specified that we don't consider biological components in line 332-333 :

*'[...] it is possible to estimate the **sediment transport capacity** on shelf. **Here the seabed composition is considered non-cohesive and without biological components.**'*

Line 293: Assuming non-cohesive sediment has major implications for remobilization. This assumption should be discussed explicitly in the Discussion (see, e.g., your coauthor's work "Classifying seabed sediment type using simulated tidal-induced bed shear stress").

Please see reply to previous point.

Line 323: The claim that "tidal stresses on the shelf are expected to reduce" requires a supporting figure or citation. This appears inconsistent with line 227 ("SLR can also alter the wave stress"). Please reconcile these statements.

Thank you for pointing out that this may need clarification; these two statements are not inconsistent as both tidal waves and wind waves are directly dependent on water depth. The same physical process affects both: when considering SLR alone tides as well as waves are decoupled from the seabed. When also considering the changes in storms (i.e. stronger wind projections on top of the changes in water depth) then the stress related to the waves is increasing because of changes in the wind, not SLR. The effects of projected wind (storm) changes swamp the effects of projected SLR. We have added some clarification in the text.

*Line 372-377: 'In all our future projections, tidal stresses on the shelf are expected to reduce with an overall reduction in seabed currents, **as SLR deepens the water column, weakening momentum transfer between the surface and seabed. This is true for waves as well as tides (because wave orbital motions decay exponentially with depth). Conversely, there is amplification of wave-induced stress from storms, as events in this case study projection are more intense than that of the present-day storm simulation.**'*

Lines 338-41: The link between changes in sediment class and effects on anthropogenic structures (e.g., "East Anglia One") is unclear. Specify what quantitative or qualitative results support this connection.

There is no link, we simply wanted to give an example. It was picked because it's a very active area, of interest to windfarms. We have now removed this example to avoid confusion.

Lines 341-342: A 30 km scale bar indicates distance, not data resolution. The statement regarding dataset limitations inferred from the scale bar is confusing and should be revised. Additionally, references are unevenly distributed: some unsupported claims lack citations, while this minor statement lists three.

The point of that scale bar was supposed to be showing the resolution's improvement in this type of dataset (from 10 km to 1.5 km) the 30 km scale bar with 3x10 km ticks was

added to help visualise the scale. However, we agree with the reviewer that this is confusing and not the best way to show this. We have removed this section and added a section in the appendix showing these differences in more detail. The figure shown in appendix A.3 now mentions the improvement of our resolution instead, to keep the main manuscript clearer.

*line 824 - 827: **‘Moreover, these plots illustrate how the model operates at relatively high spatial resolution compared to other available products, which supports its usefulness in capturing spatial variability in sediment mobilisation despite the inherent simplifications.’***

Line 344: The statement ‘future seabed mobility will be controlled by’ simplifies the system; the seabed consists of mixed sediment classes with biological and compositional variability that should be acknowledged.

We have now clarified this in lines 400-403:

‘This study is purely modelling-based, and focuses on the effects of SLR and storms within a broader set of controlling factors which cannot all be considered here, such as mixed sediment classes and biological or compositional variability.’

Lines 351-357: The argument that “SLR should be included in MPA management” is valid but unsupported by empirical evidence from this study. No data are presented linking SLR-induced stress changes to ecological consequences such as species diversity, biomass, or sediment biogeochemistry.

We agree with the reviewer that we do not directly tackle the effects that SLR has on the MPA. We do however believe that it is an important consideration to make within the discussion section, to support future work and to understand possible implications of changes to seabed stress that were out of scope for our research in this case. To make sure that we do not mislead the reader in interpreting this, we have now restructured the discussion to include a separate section on speculative claims. We explicitly state that this is not based on our results. The section that the reviewer is referencing in this comment has now been substantially re-written, strengthened with peer-reviewed references and is explicitly referenced as ‘speculative’. The references to MPAs are now only in this part of the discussion, please see:

Lines (505-539): **‘Another important broader implication in the context of this study is the way in which seabed stress may modify benthic habitats, and how this can consequently affect the design and management of Marine Protected Areas (MPA) or measures to compensate for potential habitat losses. SLR and storms can both have a**

significant impact on these areas, and previous studies indicate that adaptive measures represent an effective approach for enhancing their environmental resilience [90–93]. Sediment transport, which can be induced by both SLR and storms [94], also can have an impact over these areas [95]. Impacts of winnowing and sand wave migration will alter the habitat suitability for different benthic communities [96]. Benthic habitats are also sensitive to periodic extreme events. The passage of a storm can lead to a step-change in benthic habitats and infaunal communities [97]. In areas of high flows and bed stress, benthic communities are exposed, more robust and opportunistic, so they can withstand a large range of natural variability [98]. Increased storminess may create more and bigger disturbance regimes for benthic communities. A better understanding of future transport capacity, as the one presented in this work, can be helpful to identify regions more vulnerable to changes. However, it is not yet clear whether sediment mobilisation will be sufficient to really alter the seabed composition and therefore the habitats. Sediment transport capacity does not easily translate to sediment composition. Moreover, D50 sediment classes do not represent sediment heterogeneity, and the changes in sediment also need to be modelled together with all the other drivers for benthic community distribution (e.g. temperature, primary production) to estimate what the combined effects will be. If the storms are only rare events, then the disturbance may not be enough to alter the benthic habitat (sediment composition) over a longer time. In that case they mainly will change (increase) the disturbance regime to which the current benthic communities are exposed, causing additional stress to the species present. The impact will also depend on the underlying sediment type and thickness. Increased shear stress might not immediately change the substratum type. If changes are regular and long-term, it might change the energy climate for benthic communities, and therefore cause changes in species presence/absence and community composition. These seabed composition results must therefore be evaluated together with other factors that determine species composition. For example, rapid warming and marine heatwaves [99]. However, the outcomes of this work give indications of potential changes in seabed drivers, and identify regions of the NWES that are exposed and need to be taken into account when carrying out future species distribution predictions.'

Lines 369-377: This paragraph lacks a clear outcome. Key questions remain unanswered: How much additional stress will structures experience under SLR? Claims about "local flow amplification" are not supported by model outputs.

We agree with the reviewer that this is not a direct outcome of our study, but we consider it appropriate to mention it in the Discussion as a point for consideration. Doing so helps

clarify the relevance of the processes examined here, supports the development of future work, and contributes to a broader understanding of the context in which this study is developed.

To address your concern, and not mislead the reader, we have drastically re-structured the discussion into separate sections that address the direct outcomes of our study, and speculative debate. The section you mention in this comment has now been strengthened with additional peer-reviewed research and was merged in:

Lines (466-504) : **‘Beyond the direct results of this study [...] areas where future changes in seabed mobility will need to be taken in consideration.’**

Lines 387-388: As noted, "Sediment transport capacity does not easily translate to sediment composition." Likewise, D50-based sediment classes cannot directly represent compositional heterogeneity; this should be acknowledged.

Now specified in line 523-526:

‘Moreover, D50 sediment classes do not represent sediment heterogeneity, and the changes in sediment also need to be modelled together with all the other drivers for benthic community distribution (e.g. temperature, primary production) to estimate what the combined effects will be.’

Lines 393-405: Statements remain speculative (“might be” / “might not be”). These should be substantiated or rephrased as hypotheses.

We acknowledge the reviewer’s concern regarding speculative language in the Discussion; this wording was indeed intended only for speculative or hypothetical statements. We believe that the Discussion can also be used to raise broader concepts that go beyond the direct implications of the results, as points for consideration. However, based on the reviewer’s comment, we recognise that this intention was not communicated clearly enough. We have therefore revised the Discussion to provide a much clearer distinction between implications that are directly supported by our results and broader concepts that are related to our ideas but not directly demonstrated by this work.

To address this, we have re-structured (and re-written part of) the discussion. Firstly we have started the discussion resuming clearly what questions are directly answered by our work, and pre-warned the reader that the discussion will deal with implications of our results first, followed by some paragraphs on speculative concepts and general ideas not directly demonstrated by our work but related to it. For this introductory paragraph, please see:

lines 393-405: ***‘This study highlights the influence of SLR and storms on future seabed mobility, based on the projections of the RCP8.5 scenario. We focus on the NWES region, as it is a well studied area, exhibiting a wide variety of conditions found across continental shelves globally, increasing the broader applicability of the results. Three main questions are addressed: How will the seabed stress be affected by the isolated effect of SLR? How will climate change (future storms) impact on future seabed stress in a case study extreme scenario? How can these projected changes in ocean currents and seabed stresses influence future sediment transport capacity? This study is purely modelling-based, and focuses on the effects of SLR and storms within a broader set of controlling factors which cannot all be considered here, such as mixed sediment classes and biological or compositional variability. Below, we first discuss direct implications of our results, followed by a broader speculative assessment of important concepts related to these findings. Limitations and future work are also discussed.’***

We then have separated all aspects of the discussion that are direct implication of our results. This can now be found in:

Lines (406 - 465): ***‘Results show that [...] dominant and potentially disruptive influence.’***

Finally, we have separated all speculation into separate paragraphs, explicitly stating that these do not contain direct results of our work, but important concepts to consider. We have strengthened this part of the discussion with previous literature from peer review research to support our claims. This section can be found in:

lines (466 – 539): ***‘Beyond the direct results of this study, possible consequences and related concepts are considered, which remain uncertain but may be important for understanding the broader context, the significance of these findings, and guiding future research. [...] However, the outcomes of this work give indications of potential changes in seabed drivers, and identify regions of the NWES that are exposed and need to be taken into account when carrying out future species distribution predictions.’***

The discussion ends with a paragraph on limitations of our work, and possible future work that we believe to be important:

lines (540-566): ***This study has given a new understanding of future seabed mobilisation over the NWES and changes to the near-bed hydrodynamics. Further advancement could be achieved [...] This is an unexplored area which could stimulate future work, and may benefit from a coupled wave and hydrodynamic model.’***

We hope that this addresses the reviewer’s concern and clarifies the discussion.

Lines 406-427: Regional examples alone cannot justify general conclusions. The discussion lacks evidence of SLR impacts on anthropogenic infrastructure within the results section, making these extrapolations unsupported

Please see reply to previous point. We have now drastically re-structured the discussion; the paragraph cited has now been rearranged. The parts regarding sediment mobility impact to offshore infrastructures are now part of our speculative paragraph and have been partly rephrased:

line (483-504): **‘Enhanced bed shear stress driven by climate change modifications to waves and tides will also change sediment mobility around offshore infrastructure. Enhanced seabed stresses can force a live-bed regime to transition to one that supports bedform development and migration [85] or drive change towards an upper-stage plane bed by flattening bedforms; seabed scour can result from both, exposing foundations and seabed cables to potential damage. These changes will also enhance the energetics of the turbulent wakes that form in the lee of seabed infrastructure such as offshore windfarm monopiles. At the scale of an offshore windfarm, the co-occurrence of enhanced stress resulting from climate change and the turbulent wakes from seabed infrastructure can combine. These enhanced stresses have the potential to increase seabed heterogeneity (the spatial variability of seabed morphology) and modify grain size mixtures [71]; all of which are critical for the functioning of the benthic ecosystems [86]. Sediment transport capabilities also impact the success of nature-inclusive designs of marine infrastructure. The rock grade chosen to keep infrastructure protected should not be mobilised in the next few decades, whilst modified mattresses (a different type of rock armoring used in OWF foundations [87]) to be nature-inclusive may not withstand intense currents [88]. Changes to sediment mobility can cause cage sinking, where protective cages used to contain restoration substrates (e.g. oyster shells or other habitat material) become partially buried due to sediment movement. This, in general, calls for improved reef and housing designs to ensure long-term restoration success [89]. Our results point towards hotspots of areas where future changes in seabed mobility will need to be taken in consideration.’**

Reference to the ECOWind ACCELERATE project and regional case study has been added to the future work paragraph:

Lines (567-571): **‘Part of the future work outlined above is already underway as part of the ECOWind ACCELERATE project (<https://ecowind.uk/projects/ecowind-accelerate/>), as for example the inclusion of OWF infrastructure on top of the climate**

change impact discussed in this paper, or the effect of changes over benthic ecosystem and seafloor habitat.’

[71] Austin, M. J., Unsworth, C. A., Van Landeghem, K. J. J. & Lincoln, B. J. Enhanced bed shear stress and mixing in the tidal wake of an offshore wind turbine monopile. *Ocean Science* 21, 81–91 (2025).

[85] Couldrey, A. J., Benson, T., Knaapen, M. A., Marten, K. V. & Whitehouse, R. J. Morphological evolution of a barchan dune migrating past an offshore wind farm foundation. *Earth Surface Processes and Landforms* 45, 2884–2896 (2020).

[86] Gates, A. R. et al. Ecological role of an offshore industry artificial structure. *Frontiers in Marine Science* 6, 675 (2019).

[87] Chambel, J. et al. A comprehensive review on scour and scour protections for complex bottom-fixed offshore and marine renewable energy foundations. *Ocean Engineering* 304, 117829 (2024). URL <https://doi.org/10.1016/j.oceaneng.2024.117829>.

[88] (SOWEC), S. O. W. E. C. Collaboration for environmental mitigation and nature inclusive design (cemnid). Tech. Rep. A-100906-S00-A-REPT-007, SOWEC (2024).

[89] Robertson, M., Locke, S., Uttley, M., Helmer, L. & Kean-Hammerson, J. Exploring the role of offshore wind in restoring priority marine habitats (2021).

Reviewer #3 (Remarks to the Author):

Review of the manuscript “Impact of the climate change on hydrodynamic seabed stressors” by Rulent J. et al.

General comments:

The manuscript offers a significant examination of the impacts of climate change, particularly sea level rise (SLR) and increased storminess, on hydrodynamic seabed stresses and sediment transport capacity in the North-west European Shelf (NWES). This is the first study to find out how big these changes are likely to be in this important industrial area. This will help with marine spatial management in the future.

Thank you for this positive assessment, we are glad you find our work relevant.

Major concerns:

Dependence on CMIP5 projections: The use of CMIP5 climate projections, particularly HadGEM3-GC3, under RCP8.5, could be considered a weakness. CMIP5 was the best available at the time, but CMIP6 is better since it has better models, higher resolution, new scenarios (SSPs), and more recent science. Using the most up-to-date climate projections would be the best way to make marine spatial management last. The writers implicitly recognise this by describing the scenarios as "extreme, yet possible."

We acknowledge this point, however, high resolution regional forcing (atmosphere and boundary conditions) is required to drive the model in this study. At present, a down-scaled CMIP6 projection suitable for this type of study is not yet available; when this work was implemented however, the forcing did exist for CMIP5 derived forcing (Tinker et al., 2024). From a regional climate modelling point of view this is state of the art.

Downscaling CMIP6 is a whole project of its own and was not feasible withing the timescale of the project, moreover although CMIP5 is less recent, it is widely used, making our study more comparable and relatable to existing work.

Additionally, it was advantageous to refer to the RCP8.5 scenario for our work: The RCP8.5 scenario is already deeply embedded in the literature as high end-forced pathway, making it more accessible to a wider audience. It is a good scenario to use because it is a deliberately extreme, physically coherent stress test that maximizes detectability in a space with high natural variability, illuminates tail risks and bounds uncertainty (particularly when we use the single large storm approach). We believe that the RCP8.5

scenario was in this case not the most likely high-end configuration, but the most useful next step into developing the understanding of these processes.

Based on the reviewer's comment, we have clarified our thinking in the manuscript, and acknowledge that other scenarios could be considered for future studies of this type.

Please see changes to (line 114 – 120): ‘ For the first time, the expected magnitude of future changes to seabed stresses and sediment transport capacity (i.e. the coarsest sediment the flow can mobilise, regardless of actual seabed composition) in space and in time is **quantified from Representative Concentration Pathways (RCP8.5) mid- and end-century climate scenarios. This is a widely used high-emission scenario that to our knowledge has not yet been used for this type of study.**’

And (line 557 – 560): ‘**Moreover, we have focused here on the widely used RCP8.5 projections; this could be further developed by considering high-end sea-level scenarios that target regional, unlikely but not impossible, extreme sea-level changes even more explicitly (e.g. [104]).**’

Also we included a section in the discussion to reiterate that we refer to RCP8.5 and what the uncertainty bounds of this work are in lines (445-465):

‘It is important to consider all these results within the context of the CMIP5 RCP8.5 projection, used here on a downscaled regional application [51, 74], and the associated uncertainties. This work provides an understanding of seabed stress and transport capacity changes in an extreme scenario case, therefore it improves our understanding of the tail risk of a specific physical projection; these results do not show a statistical characterisation of future conditions. There is high confidence on the effect of tides, that in present-day simulations can contribute up to 3 N m^{-2} to the total combined stress during spring tides in the NWES (tidal range of up to 12 m). There is less confidence in the effects of present-day storms, which, although generally well understood, involve short-timescale processes that remain complex to predict with high natural variability. In this work's simulation storms contribute up to 12 N m^{-2} to the total stress. When interpreting the projected results, there is high confidence within the scientific community that sea level will rise, with UKCP18 indicating an uncertainty range of 0.56–1.12 m [51]. This study shows that SLR can lead to a reduction of up to 1 N m^{-2} in the total combined tide and wave driven seabed stress under average conditions. However there is low confidence on the state of future climate and storms, which we show are expected to contribute up to 15 N m^{-2} to the seabed stress. Therefore, while SLR and associated background changes are expected to produce largely predictable, predominantly reduced seabed stresses,

extreme storm events remain far less predictable and may continue to exert a dominant and potentially disruptive influence.'

We hope this addresses the reviewer's concern.

*Tinker, J. et al. Twenty-first century marine climate projections for the NW European Shelf Seas based on a perturbed parameter ensemble. Ocean Science **20**, 835–885 (2024).*

Limited generalisation: The applicability and specific findings are restricted to the North-west European Shelf (NWES). Although the method is characterised as "globally applicable," the inferences regarding particular impacts (e.g., alterations in tidal amphidromes or wave predominance in the North Sea) are localised. The title says that the study will look at the "Impact of climate change on hydrodynamic seabed stressors." This could mean that the study will look at a wider range of situations than it really does. The argument for the NWES being representative is strong, but generalisations require caution.

We agree with the reviewer that the manuscript's framing is potentially misleading. To avoid misunderstandings, we have explicitly stated that our focus is on the North Western EU shelf, which we use here as a case study for processes that are of global importance.

Please see lines 48-53:

'The changing climate is having a rapid impact on the global ocean [1], particularly in coastal and shelf seas, yet few studies have investigated effects at the seabed [2, 3]. To study this phenomenon we focus on the regional area of the North Western European Shelf (NWES) as a case study; this region includes areas with diverse characteristics such as deep and shallow zones, varying tidal ranges, and differing waves and surge dynamics. These conditions can be found in other shelf seas globally.'

We also have drastically restructured the discussion to neatly separate direct outcome of our study, related to the specific condition of the NWES from general ideas that relate to the study but are also of global concern.

*discussion that are direct implication of our results. This can now be found in:
Lines (406 - 465): **'Results show that [...] dominant and potentially disruptive influence.'***

*Wider general considerations are now in lines (466 – 539): **'Beyond the direct results of this study, possible consequences and related concepts are considered, which***

remain uncertain but may be important for understanding the broader context, the significance of these findings, and guiding future research. [...] However, the outcomes of this work give indications of potential changes in seabed drivers, and identify regions of the NWES that are exposed and need to be taken into account when carrying out future species distribution predictions.'

Uncoupled modelling: The NEMO (ocean) and WW3 (wave) models are run stand-alone, and their stresses are combined offline. The authors recognise this as a weakness and propose that a fully integrated NEMO-WaveWatch3 system would represent a future enhancement, especially in shallow water areas where the most significant effects are noted and wave-ocean coupling is most pronounced.

Yes, this was an important consideration when developing the study. There are several reasons why we opted to estimate the combined stress offline.

Firstly, the direct coupling of the model roughly doubles the computational time (Polton et al. 2023, section 4.2). This is a considerable increase in computational cost to develop a method that comes with a substantial level of technical development when suitable methods are already available. Estimating the total seabed stress offline, though more conventional, is an established method that has the advantage of being more transferable as coupling numerical models come with a great deal of implementation complexities. This can more easily be re-applied to already existing model outputs from other published research.

The method for combined stress defined in Soulsby (1997) is an empirical relationship treats waves and currents as dynamically uncoupled in their generation, but interacting within the bottom boundary layer. So while the interactions which modify waves and currents through coupling are not represented, their stresses combine nonlinearly near the seabed, even though the wave and current fields are computed separately. This interaction is what our paper is focusing on.

We do believe however that coupling could be an interesting development of this study. The impact of coupling waves to currents was investigated by Lewis et al. (2019a and 2019b). This study shows that the impact of ocean-wave coupling on the wave field is of order ± 0.01 m in significant wave height and ± 0.04 s in peak wave period (figure Lewis et al 2019.a figures 5(h) and 5(i)), in that case it is clear that the effects do not relate to water depth. By contrast, the coupling of waves to currents is more strongly dependent on water depth (Lewis et al., 2019b), and this study shows a reduction in bias against observations in the simulated current field. Based on these studies, it appears that feedback between

NEMO-WaveWatchIII would better capture the momentum transfer, likely improving results especially in very coastal areas within a few km from land. With more time and resources this would be a hypothesis to investigate in future work.

We now explain our motivation to work with offline estimation of combined stresses in lines, (548-550): **‘In this pilot study, we ran the model uncoupled and combined stresses offline to simplify the methodology, reduce technical complexity, optimise computation time, and maintain applicability.**

We also suggest the use of coupled model in future study and potential effects of the model coupling in lines (541-548): **‘Further advancement could be achieved running a coupled NEMO-WaveWatchIII system, as opposed to combining the stresses offline. Live coupling would better capture the feedback interaction between wave and ocean components [100–103], as in this configuration we are missing the transfer of momentum between ocean and wave models. This is beneficial to accurately represent shallow waters and should be implemented in future work. Many of the largest impacts we observe are taking place in shallow water regions, where the hydrodynamic-wave coupling will be most impactful.’**

As well as (562-566): **‘For example, the wave energy dissipation is reduced, allowing more wave energy to reach coastal regions in some areas, resulting in a slight increase in seabed stress as demonstrated in this study for the German Bight. This is an unexplored area which could stimulate future work, and may benefit from a coupled wave and hydrodynamic model.’**

Polton, J. et al. Reproducible and relocatable regional ocean modelling: fundamentals and practices. *Geoscientific Model Development* 16(5), 1481–1510 (2023). URL <https://doi.org/10.5194/gmd-16-1481-2023>.

Lewis, H. W. et al. The UKC3 regional coupled environmental prediction system. *Geoscientific Model Development* 12, 2357–2400 (2019.a).

Lewis, H. W. et al. Can wave coupling improve operational regional ocean forecasts for the north-west european shelf? *Ocean Science* 15, 669–690 (2019.b).

Simplifying sediment dynamics: The estimation of sediment transport capacity assumes a non-cohesive sediment bed and inverts the problem to find the coarsest mobilisable grain size. This is a necessary simplification, but the authors note that a complete estimate would require high-resolution grain size data. Additionally, the intricacies of sediment

habitat dynamics are examined but not modelled (for instance, whether mobilisation is adequate to modify bottom composition and its impact on benthic populations).

Yes, these assumptions represent a necessary simplification, appropriate for this resolution and allow for a consistent approach and coverage of the entire shelf area. This is suitable for the purpose of this study, which is to estimate the changes to the seabed stress, and potential changes to transport capacity on a specific case scenario, not to give a projection of future sediment composition. A high-resolution grain size assessment is feasible only over smaller regions (e.g., a windfarm area or a surveyed site) and is being implemented in further developments of the ECOWind ACCELERATE project, alongside studies of impacts on benthic populations and higher-level predators. The simplification implemented here allows for a better understanding of the physical dynamics, and conditions that can mobilise each sediment type.

To address your concern we have now clarified our assumption further in line 331-336 :

*‘Using the new bed shear stress maps we have generated (figure 5), it is possible to estimate **the sediment transport capacity** on shelf. **Here the seabed composition is considered non-cohesive and without biological components. This is a practical way of standardising the forces experienced by the seabed, while being agnostic of existing seabed type. A more complete estimate of seabed mobility would require shelf-wide measurements of the existing sediment grain size.**’*

We also mention the development in progress in our project, which will work more in detail with impacts on sediment dynamics and over the habitat, in lines (567-571):

*‘ **Part of the future work outlined above is already underway as part of the ECOWind ACCELERATE project (<https://ecowind.uk/projects/ecowind-accelerate>), as for example the inclusion of OWF infrastructure on top of the climate change impact discussed in this paper, or the effect of changes over benthic ecosystem and seafloor habitat.**’*

We also have clarified the objective of our work more explicitly to avoid miscommunication, and split our results (dealing with seabed stresses and transport capacity), from wider implication related but not directly related to this study (eg. Sediment dynamics and habitat, which as the reviewer says, are not modelled). Our scope is now clarified in:

*Lines (122 – 126): ‘**Quantifying climate-driven impacts, assessed against present-day variability, this study examines how seabed stresses respond to the isolated effect of SLR, how future storm conditions under an extreme climate scenario may further***

modify seabed stress, and how the resulting changes in ocean currents and seabed stresses could influence future sediment transport capacity.'

We then explicitly re-state our objectives at the start of the discussion

Lines (396-400): Three main questions are addressed: How will the seabed stress be affected by the isolated effect of SLR? How will climate change (future storms) impact on future seabed stress in a case study extreme scenario? How can these projected changes in ocean currents and seabed stresses influence future sediment transport capacity?'

Moreover, we have restructured the entire discussion to separate all aspects of the discussion that are direct implication of our results from information on broader concepts and idea that, though important to point out, are not direct outcomes of this work. The section dealing with our results can now be found in:

Lines (406-465): 'Results show that [...] extreme storm events remain far less predictable and may continue to exert a dominant and potentially disruptive influence.'

And have restructured the discussion and improved the discussion, separating any speculation regarding impact over habitat into a specific section.

lines (466-539): Beyond the direct results of this study, possible consequences and related concepts are considered, which remain uncertain but may be important for understanding the broader context, the significance of these findings, and guiding future research. [...] However, the outcomes of this work give indications of potential changes in seabed drivers, and identify regions of the NWES that are exposed and need to be taken into account when carrying out future species distribution predictions.'

We hope that this addresses your point.

Intense storminess: A critical point of discussion concerns the confidence level associated with the projected increase in intense storminess. As the authors themselves note (Page 11, Lines 276-280), presenting the extreme storm scenario as an "end-member experiment" or a "bounding example" for risk management is appropriate, particularly given the acknowledged uncertainty in mid-latitude storm track projections, even within the newer CMIP6 ensemble. While the proposed mechanism—that higher SST fuels stronger storms through increased energy and moisture—is thermodynamically sound, robust confidence remains low in predicting the future frequency and magnitude of extra-tropical cyclones affecting the NWES. The manuscript should therefore more explicitly frame this section not as a prediction of what will occur, but as an essential hazard

quantification based on the plausible extreme range of a single model realisation, ensuring the limitations of drawing statistically robust conclusions about future storminess from single model runs are foregrounded in the discussion.

Yes, we completely agree with the reviewer that this point needs to be clear and framed in a way that leaves no space to misinterpretation. Particularly as the storms generate the most extreme seabed impacts. To make sure it is clear that we are looking at a one case scenario dealing with a plausible extreme range, and not a statistical projection we have done as follows;

Firstly, we have amended the introduction and restructured the discussion. We now begin by reminding the reader that we are explicitly dealing with RCP8.5. Specifically in lines 114 – 120:

‘For the first time, the expected magnitude of future changes to seabed stresses and sediment transport capacity (i.e. the coarsest sediment the flow can mobilise, regardless of actual seabed composition) in space and in time is quantified from Representative Concentration Pathways (RCP8.5) mid- and end-century climate scenarios. This is a widely used high-emission scenario that to our knowledge has not yet been used for this type of study.’

Another key finding of our work is the relative order of magnitudes of the storm and tidal controls, versus the impacts of gradual sea-level rise and climate change. We have added:

Line 327-328: ***"[...] any climate change signal will be dwarfed by natural seasonal variability."***

lines 393-405: ***This study highlights the influence of SLR and storms on future seabed mobility, based on the projections of the RCP8.5 scenario. We focus on the NWES region, as it is a well studied area, exhibiting a wide variety of conditions found across continental shelves globally, increasing the broader applicability of the results. Three main questions are addressed: How will the seabed stress be affected by the isolated effect of SLR? How will climate change (future storms) impact on future seabed stress in a case study extreme scenario? How can these projected changes in ocean currents and seabed stresses influence future sediment transport capacity? This study is purely modelling-based, and focuses on the effects of SLR and storms within a broader set of controlling factors which cannot all be considered here, such as mixed sediment classes and biological or compositional variability. Below, we first discuss direct implications of our results, followed by a broader speculative***

assessment of important concepts related to these findings. Limitations and future work are also discussed.'

We have also included a section in the discussion to put our work in the context of CMIP5, reiterate that we refer to RCP8.5 and better explain what the uncertainty bounds of this work are in lines (445-465):

'It is important to consider all these results within the context of the CMIP5 RCP8.5 projection, used here on a downscaled regional application [51, 74], and the associated uncertainties. This work provides an understanding of seabed stress and transport capacity changes in an extreme scenario case, therefore it improves our understanding of the tail risk of a specific physical projection; these results do not show a statistical characterisation of future conditions. There is high confidence on the effect of tides, that in present-day simulations can contribute up to 3 N m^{-2} to the total combined stress during spring tides in the NWES (tidal range of up to 12 m). There is less confidence in the effects of present-day storms, which, although generally well understood, involve short-timescale processes that remain complex to predict with high natural variability. In this work's simulation storms contribute up to 12 N m^{-2} to the total stress. When interpreting the projected results, there is high confidence within the scientific community that sea level will rise, with UKCP18 indicating an uncertainty range of 0.56–1.12 m [51]. This study shows that SLR can lead to a reduction of up to 1 N m^{-2} in the total combined tide and wave driven seabed stress under average conditions. However there is low confidence on the state of future climate and storms, which we show are expected to contribute up to 15 N m^{-2} to the seabed stress. Therefore, while SLR and associated background changes are expected to produce largely predictable, predominantly reduced seabed stresses, extreme storm events remain far less predictable and may continue to exert a dominant and potentially disruptive influence.'

Finally, we also have mentioned that different types of projection scenarios should be considered in the future work section, explicitly adding in lines (557 – 560): **'Moreover, we have focused here on the widely used RCP8.5 projections; this could be further developed by considering high-end sea-level scenarios that target regional, unlikely but not impossible, extreme sea-level changes even more explicitly (e.g. [104]).'**

[104]. Dayan, H. et al. High-end scenarios of sea-level rise for coastal risk-averse stakeholders. *Front. Mar. Sci.* **8**, 569992 (2021).

<https://doi.org/10.3389/fmars.2021.569992>

In conclusion, the manuscript provides a rigorous and novel regional quantification of future seabed stressors, successfully isolating the predictable effects of sea level rise (SLR) from the potentially extreme, intermittent impacts of intensifying storms across the North-west European Shelf (NWES). This work is essential for future-proofing regional marine spatial management and is highly commendable. However, for a higher-level journal seeking global impact or the absolute cutting edge of methodology, the study faces hurdles related to scope and source material. Its strong regional focus on the NWES, while justifiable, limits its immediate generalisability, and its reliance on CMIP5/RCP8.5 climate projections, now superseded by CMIP6/SSPs, positions the foundational data a generation behind current standards. Furthermore, the acknowledged use of uncoupled hydrodynamic and wave models introduces a potentially questioned technical constraint. Therefore, while the paper represents an excellent contribution to a dedicated journal in shelf sea dynamics, oceanography, or coastal engineering, its regional focus and the use of antecedent climate model data may reduce its novelty and impact required for the most selective publications.

Thank you for taking the time to review our work. We have addressed your concerns with the above responses and made changes to our manuscript to bring it up to the journal's high standards. The NWES is the focus of this study and was deliberately selected because it encompasses a wide range of conditions representative of diverse regions worldwide. We have worked at making our method as transferable as possible, partly by keeping our model un-coupled, gaining in computational efficiency and facilitating reproducibility. The offline calculation of seabed stresses is a sound method, suitable for the purpose of our work. From a regional modelling perspective the CMIP5/RCP8.5 remain the state-of-the-art, and have not previously been used in this type of study to our knowledge. We hope we have convinced you that, while our study focusses on the NWES region, the wider implications of this study are relevant to a much wider audience.

Thanks for your time helping improve our manuscript,

Julia Rulent

We thank the reviewers for taking the time to reassess the manuscript and to evaluate our revisions. We have made additional modifications to address the remaining concerns raised in the latest round of review. We hope these changes have improved the manuscript and clarified the points discussed. We appreciate the time and effort invested in reviewing this work and are pleased that the manuscript has reached this stage of the publication process.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors have satisfactorily addressed my questions and concerns. The additional information on model calibration and model setup choices greatly improve the rigor of the work and I appreciate the extra context on offshore wind and bottom stress from fishing. It is my opinion that this paper is ready for publication.

Thank you for taking the time to read our manuscript, and for the positive assessment. We are glad to hear you think the publication is ready.

Reviewer #3 (Remarks to the Author):

I would like to thank you for the opportunity to review this manuscript once again. It is clear that a significant amount of effort has gone into this revision, and I appreciate the high level of technical rigour maintained throughout the document.

The revised manuscript and the accompanying response letter address several of my initial concerns with clarity. The depth of the hydrodynamic analysis, the detailed explanation of using CMIP5, and the estimate of the combined stress offline are particularly noteworthy. However, the study's scope has significantly narrowed to a regional focus on the North Sea or NWES. Your response suggests that this region serves as a global proxy because it includes diverse characteristics such as varying depths and tidal ranges; however, I find this justification to be overly brief and unconvincing in addressing the fundamental issue of scalability. While NWES is a complex system, its dynamics—driven by specific North Atlantic tidal resonance and ETCs, but it does not necessarily translate to other shelf systems. For instance, the hydrodynamic stressors in the NWES are fundamentally different from micro-tidal shelf systems like the Gulf of Mexico, Baltic Sea or tropical shelves where seasonal monsoons or TC activity are the primary drivers of seabed disturbance. Consequently, it remains unclear if these findings can be generalised.

While this allows for a very detailed and robust analysis of that specific area, it reinstates my original concern regarding the scalability of the conclusions. It remains unclear if the

mechanisms and findings identified here can be generalised to other regions or global contexts. Given this shift in scope, I have deferred to the Editor to judge whether the manuscript's current regional focus aligns with the broad impact requirements of this journal.

Thank you for your time reviewing this manuscript once more. We are happy to hear that most of your concerns were addressed.

We appreciate your point with regard to the regional scope of this work. While we do believe that the results are scalable to different regions, in order to address this and being transparent with the reader we have changed the abstract clarifying right away that our analysis is set in the NWES as a case study. We have also rephrased part of the introduction to explain more in detail why we believe these results are of interest to a wider audience. We think that, though our case study is regional, the methods are globally applicable and the physics driving the processes that we investigate here, is true everywhere. While there will be local differences in wave and tidal conditions, the impact of sea-level rise underpinning the change will be experienced everywhere. Also, the magnitude of the tidal response to increasing water levels is likely to be informative for other macrotidal regions. Similarly, the resulting changes in seabed stress provide insights that extend beyond the study area. While the storm-driven increase in stress quantified here reflects conditions specific to the NWES, the demonstration of a substantial storm impact is relevant for any region experiencing changes in the frequency or intensity of extreme events. To clarify our thought in the manuscript we now have:

Mentioned the NWES region directly in the abstract:

*'How will climate change affect the composition of the seabed? Using three-dimensional hydrodynamic and wave models combined with climate projections from the Coupled Model Intercomparison Project phase 5, **we investigate and quantify the drivers of seafloor stresses, using a North Western European Shelf case study.** This study maps the potential for grain size mobilisation in present-day and future conditions, highlighting areas where transport capacity is likely to change. Sea level rise alters both absolute water depth and tidal dynamics, changing the magnitude and timing of seabed stresses while simultaneously reducing exposure to waves and currents. In contrast, increasing storm intensity in a warming climate is expected to cause more frequent episodic disturbance of an otherwise increasingly stable seabed. These predicted changes and their potential impacts on the benthic environment are discussed in the context of growing anthropogenic pressures and the associated challenges for marine spatial management.'*

Added further explanation to the introduction (lines 49-55):

'Though the quantitative estimates presented here reflect local processes, the underlying hydrodynamic processes are universal, and our findings are applicable to other regions. The direction of the response of seabed stress and currents to changes in sea level and storms is not a local issue. Sea level rise is occurring globally, and will affect shelf-seas worldwide. As such, other systems can draw insight from results obtained in this well-studied region, which is representative of a wide range of environmental conditions.'

We hope this addresses your point, confirms the scope of this work, and makes our idea transparent to the readers.