EC1: 'Comment on sp-2024-44', Kirsten Wilmer-Becker, 19 Feb 2025 reply

Dear Dr. Wilmer-Becker,

We would like to thank you for your careful reading of our manuscript and for your constructive comments. We appreciate the suggestions, which helped us improve the clarity and completeness of the text. Below, we provide a point-by-point response to your remarks. Revisions made in the manuscript are highlighted accordingly.

We thank you once again for your helpful feedback and for the opportunity to revise our manuscript.

Best regards,

Joanna Staneva

This paper highlights some important aspects that should be considered when applying a numerical model to the coastal ocean, particularly in an operational context. While this style of paper is not new, and there have been similar contributions to the literature in the past, the state-of-the-art in coastal ocean modelling methodologies does advance, and it is appropriate that the appraisal of those methodologies should similarly periodically advance. Additionally, there are a broad range of issues that require consideration in the coastal ocean from a modelling perspective, and most existing reviews of coastal modelling techniques focus on a subset of this range. This manuscript is no different, choosing to focus on spatial scales & processes, observations, nesting, unstructured approaches, and observing system experiments. These aspects are indeed relevant for coastal zone modelling, and consideration of this subset does not detract from its general relevance in my view. While the topics under consideration in the manuscript may not come as revelations to modellers well versed in coastal applications, they are central to producing good coastal models, and are a timely reminder that these aspects should receive close attention when building a coastal zone model. The speculation in the summary around how contemporary trends, driven by coastal necessities, may influence future applications is a good synthesis of where coastal modelling is heading. As such, I think this manuscript is a worthy addition to the literature and I recommend publication with some minor alterations.

Authors' response: Thank you. We appreciate the constructive evaluation and take note of the positive assessment regarding the scope and relevance of the manuscript. The intention here is indeed to present a focused review of selected elements central to high-resolution coastal modelling, particularly in operational frameworks. We have addressed the specific suggestions in the following responses.

In Section 3.1, novel observational platforms are considered, with HF radar and ADCPs in particular singled out for attention. I think it may also be worthwhile to make explicit mention of slocum gliders here (i.e., prior to its brief mention in Section 4). These autonomous underwater vehicles can host a wide array of instrumentation, and deliver high spatial and temporal resolution observations, especially if repeat transects are programmed. Similarly, while SWOT is a step forward in terms of remotely sensing the coastal ocean, the geostationary Himawari-8 satellite is similarly a step forward, delivering up to 500m and 10 minute resolution data, and maybe also worth a mention. I think any current review of observations should probably include these contemporary platforms.

Authors' response: Thank you. We agree that including additional observation platforms is relevant. The revised manuscript now explicitly refers to gliders and the Himawari-8 satellite, with a brief explanation of their application in the context of high-resolution coastal observations and provided additional references. We also updated the text about SWOT applications in the coastal ocean.

Some statements are made that would carry more weight if additional references or examples were given, particularly in Section 2; e.g., paragraph starting line 130 regarding small spatial scales, line 237-238, paragraph starting line 239, with perhaps additional examples outside the Baltic, line 249-250 for riverine input methods, and in general where qualitative statements are made throughout.

Authors' response: Thank you. We have reviewed the mentioned sections and added references where appropriate. These include examples from different regions, as well as studies that illustrate the methods discussed. Please also refer to our detailed responses to both reviewers for specific updates and newly cited literature. For the riverine input methods, we have added a dedicated subsection discussing the role of river discharge in coastal dynamics and modelling, supported by recent high-resolution studies.

Section 3.3. The type of open boundary applied to downscaled models is key to a good solution free from specification error. OBCs are generally not well transportable across applications, and require some application-specific tuning. An ocean model with a large suite of OBCs is advantageous when solving coastal ocean problems. I think the manuscript could be strengthened with some commentary around open boundary conditions, perhaps an elaboration of lines 151-154 with references.

Authors' response: Thank you. We have expanded the discussion in Section 3.3 to include a short paragraph on open boundary conditions. We refer to common challenges and the importance of case-specific configuration. Additional references have been included.

Line 250: 'Unstructured-grid models, with their ability to employ higher-order spatial discretizations' – this isn't strictly true as unstructured models more commonly employ lower order momentum and tracer advection owing to their irregular grid and awkward interpolations required to achieve higher order. They can, however, provide superior resolution placement and transition, allowing better dynamic representation in coastal and estuarine environments.

Authors' response: Thank you for this remark. Thank you for this constructive remark. We agree that unstructured-grid models typically employ lower-order discretization due to interpolation challenges on irregular meshes. The revised sentence now reflects this more accurately, while emphasizing the strength of such models in resolving complex dynamics through flexible resolution placement.

Paragraph starting line 260. Grid generators tailored for the specific requirements of the unstructured numerical core are starting to appear, e.g., JIGSAW (Engwirda, 2017, Geosci. Model Dev, 10 (6), p. 2117). This package creates high quality meshes that are an orthogonal, well centred centroidal Voronoi tessellation, that, for example, conform to the numerical requirements of TRiSK. This package is also seeing uptake in other cores, often with a front-end API attached (e.g., OCSMesh). These numerics-tailored mesh generators are in contrast to older meshing packages, e.g., John Shewchuk's TRIANGLE, which is a general-purpose triangulation package which has been used by modellers in the past, and is not specifically tailored to solving the Navier Stokes on a mesh. Perhaps the progress of JIGSAW style triangulators for more objective mesh generation could be mentioned.

Authors' response: This suggestion has been addressed by adding a reference to the JIGSAW mesh generator and a short note on its relevance for generating meshes tailored to specific numerical schemes.

Line 152: 'Unlike global models that can operate with open boundaries…' Should this be '…. without open boundaries'?

Authors' response: We agree and have corrected the sentence to: "*Unlike global models that can operate without open boundaries, regional and coastal models require well-defined lateral boundary conditions.*"

Section 3.2, Table 1: This is awkward – the list is good, but I think the coastal unstructured COMPAS model developed by CSIRO (Herzfeld et al, 2020, https://doi.org/10.1016/j.ocemod.2020.101599), or global MPAS developed by LANL (Ringler et al., 2013 10.1016/j.ocemod.2013.04.010) could also be a worthy addition. Awkward because I'm beating my own drum here with COMPAS. However, these models are based on the TRiSK numerics which is one of the few numerical cores that operates unstructured with finite volume on a C-grid (in this case Voronoi tessellations) without generating spurious modes that require suppression to control. The core also has other desirable properties that merit its inclusion. Ultimately the authors call though.

Authors' response: Thank you for the helpful suggestion. We agree with the relevance of both models. COMPAS and MPAS have now been added to Table 1 as unstructured finite-volume models, and the corresponding references (Herzfeld et al., 2020; Ringler et al., 2013) are included in the revised manuscript.