

Review of “Arctic-XBeach v1.0: A Python-Based Thermo-Morphodynamic Model for Arctic Permafrost Coastal Erosion” by K. Nederhoff et al.

The manuscript, “*Arctic-XBeach v1.0: A Python-Based Thermo-Morphodynamic Model for Arctic Permafrost Coastal Erosion*,” presents a computational framework coupling a one-dimensional thermal model with XBeach to simulate Arctic coastal erosion. An event-driven switching strategy reduces computational costs by activating XBeach primarily during storm events and at prescribed inter-storm intervals. The model is calibrated and evaluated using observations from Barter Island, Alaska, and demonstrates reasonable agreement with observed multi-year shoreline and bluff retreat. The work addresses an important problem and offers a promising approach to computationally efficient coastal erosion modeling. However, several aspects of the numerical methodology, verification, computational performance, and generalizability warrant further attention.

My main comments are as follows:

1. The manuscript would benefit from a more complete presentation of the governing equations, constitutive relationships, and numerical discretizations employed in the XBeach component. In particular, the implementation of the thermal erodibility mask and its effect on sediment transport and morphological evolution should be described more precisely.
2. The authors should clarify how mass and energy conservation are maintained during coupling, particularly when the bed morphology changes and thermal fields must be updated. The treatment of sediment transport across frozen–thawed boundaries and the numerical implications of the hourly coupling interval also warrant discussion.
3. Section 4.1.1 provides comparisons against analytical solutions for single-phase heat conduction. A spatial and temporal convergence study should be included to establish numerical accuracy. Furthermore, since phase change is central to the proposed formulation, verification against a problem involving a moving phase boundary, such as the classical Stefan problem, would substantially strengthen the manuscript.
4. The optimization procedure in Section 3.5 requires a more complete description, including the algorithm employed within Optuna, the sampling strategy, the objective function, and the convergence criteria. Convergence histories and some discussion of parameter identifiability would also be useful, given the relatively large number of calibrated parameters.
5. Since computational efficiency is a principal motivation for the event-driven coupling strategy, the authors should report actual CPU or wall-clock times, computational

hardware, and the number of XBeach activations during each simulation period, distinguishing storm-driven from scheduled activations. An estimate of the computational cost of continuous coupling, ideally supported by a short benchmark, would help quantify the benefits of the proposed approach.

6. The discussion of model performance in Section 4.2 would benefit from a table summarizing observed and predicted bluff and shoreline retreat, together with error metrics, for each calibration and validation period. Additional figures illustrating morphological evolution would also be informative. The substantial overprediction of bluff retreat during the low-erosion period (7.7 m predicted versus 0.4 m observed) deserves further investigation and discussion.
7. The current formulation does not explicitly represent soil mechanics, cohesive sediment behavior, or mechanical failure processes such as block collapse and thermal abrasion. Although some limitations are acknowledged, the authors should discuss more clearly how these omissions affect predictive accuracy, particularly for ice-rich bluffs and environments where mechanical failure is important. It might be useful to add a table with certain soil types/environments and say where the model is/is not applicable.
8. The model has been calibrated and evaluated at a single site. The authors should more clearly identify the coastal environments and material properties for which the current formulation is appropriate, as well as the site-specific data and recalibration required for application elsewhere. The distinction between software extensibility and demonstrated predictive generalizability should be emphasized.
9. The use of relatively coarse ERA5 wave and atmospheric forcing may affect the representation of localized storm conditions and, consequently, the activation of XBeach. The authors should discuss the sensitivity of their predictions to the forcing resolution and the user-defined switching thresholds.
10. The discussion of ice-wedge polygons and spatial heterogeneity on page 28 would benefit from citing the work of Frederick et al. and Bayat et al., which addresses related aspects of Arctic permafrost coastal erosion and its physical modeling.

Overall, the manuscript presents a potentially valuable modeling framework, but additional numerical verification, methodological detail, and quantitative computational benchmarks are needed to substantiate its principal claims and establish the reliability and applicability of the proposed approach. Therefore I recommend that the authors provide a major revision of the manuscript.

Minor comments:

- Arctic should be capitalized throughout.
- Where you introduce F_o , you have both F_o and F_0 . I think you should make it F_o throughout.
- P. 21, item 1: should be “determines”.