

Response to RC3 Comments for
Placing constraints on submarine permafrost extent along the U.S. Beaufort Shelf using thermodynamic modeling

Ms. No: EGUSPHERE-2025-5529

By E.W. Conley, J.M. Frederick, A.C. Stanciu, R.E. Abbott, M.G. Baker, D. Fukuyama, & M.A. Nole

RC3: This manuscript presents thermodynamic simulations to constrain the extent of submarine permafrost along the U.S. Beaufort Shelf near Oliktok Point. The study explores 18 model configurations that vary lithology, pore-water salinity, geothermal heat flux, and simplified borehole-based heterogeneity, and then compares the resulting ice-saturation patterns with previous seismic, resistivity, and DTS-based interpretations. The topic is potentially valuable, especially given the growing interest in combining process-based modeling with emerging fiber-optic observations in Arctic shelf environments.

However, in its current form, I do not find that the manuscript supports several of its strongest claims. My main concern is that the study is framed in places as if it had robustly constrained the actual present-day conditions along the transect. In contrast, the evidence presented is more consistent with a scenario-bounding or sensitivity-analysis study. The manuscript shows that some parameter combinations appear more consistent with previous geophysical interpretations than others, but it does not yet provide a sufficiently direct observational evaluation to justify the stronger inferences currently made in the Abstract, Discussion, and Conclusions. In my view, the study has clear potential, but substantial revision is needed before it is ready for publication.

Author Response: We thank the reviewer for the thoughtful and constructive assessment of our manuscript. We agree that the study is more appropriately framed as a scenario-bounding and sensitivity-analysis investigation rather than a definitive reconstruction of present-day conditions along the transect. Our intent was to evaluate thermodynamically permissible submarine permafrost extents under varying lithologic, salinity, and geothermal conditions and assess their consistency with available geophysical observations. In the revised manuscript, we will revise the Abstract, Discussion, and Conclusions to more clearly distinguish between modeled scenarios, observational consistency, and direct observations.

Reviewer Comment: In its current form, the study is best understood as a set of thermodynamic sensitivity experiments exploring how simulated submarine permafrost extent changes under different assumptions about lithology, pore-water salinity, geothermal heat flux, and simplified subsurface heterogeneity. That is a useful contribution. However, the manuscript often frames the results more strongly, as if the study had robustly constrained the actual site conditions. I suggest that the authors define the contribution more clearly and consistently from the outset. This is primarily a scenario-bounding or sensitivity-analysis study, and the framing of the abstract, discussion, and conclusions should match that scope.

Author Response: We agree that the framing of the manuscript should more consistently reflect the scope of the study. Our intent was to use thermodynamic simulations to establish physically permissible bounds on submarine permafrost extent under varying lithologic, salinity, geothermal, and heterogeneity assumptions while evaluating consistency with available geophysical observations. In the revised manuscript, we will revise the Abstract, Discussion, and Conclusions to more clearly frame the work as a scenario-bounding and sensitivity-analysis study rather than a definitive reconstruction of present-day site conditions.

Reviewer Comment: A central weakness of the manuscript is that the simulations are not evaluated against observations in a way that clearly demonstrates model capability for this specific site. The discussion refers to seismic, resistivity, and DTS-related information, but these are used mainly for indirect or qualitative comparison rather than for clear calibration or validation. As a result, the study does not yet show how well the model reproduces the

observed system; rather, it shows that some tested scenarios appear more plausible than others. This distinction should be made much more clearly throughout the manuscript.

Author Response: We appreciate the reviewer's perspective and agree that the distinction between observational validation and consistency with geophysical interpretations should be stated more clearly throughout the manuscript. However, we respectfully disagree that the absence of direct calibration to the existing geophysical observations represents a central weakness of the study. A primary motivation of this work is that current seismic and resistivity methodologies may provide conservative or potentially ambiguous estimates of submarine permafrost extent, particularly in partially frozen or lithologically heterogeneous systems. Accordingly, the purpose of the thermodynamic simulations is not to directly reproduce the geophysical interpretations, but rather to evaluate physically permissible permafrost distributions and assess which scenarios are most consistent with the available observational constraints. In the revised manuscript, we will clarify this distinction more explicitly to avoid implying direct validation of the modeled results.

Reviewer Comment: The borehole-based heterogeneous simulations are presented as more realistic, but they remain highly idealized. In particular, the extension of lithologic units laterally across the shelf from a small number of boreholes is a major assumption, not a minor simplification. Likewise, the treatment of bottom-water temperature, the use of only two geothermal heat-flux values, and the adoption of a fixed 40% ice-saturation threshold all strongly influence the simulated distribution and inferred extent of submarine permafrost. These assumptions are acceptable as part of a conceptual sensitivity study, but they currently carry too much weight in the interpretation. The manuscript should either test their influence more explicitly or discuss their consequences more critically.

Author Response: Our intent was not to present these models as definitive reconstructions, but rather as more geologically informed end-members relative to the fully homogeneous cases. We agree that assumptions regarding lateral lithologic continuity, bottom-water temperature, geothermal heat flux, and the 40% ice saturation threshold substantially influence the modeled distributions and should be discussed more critically. In the revised manuscript, we will expand the discussion of these assumptions, their implications, and the associated uncertainties to better reflect the conceptual and sensitivity-based nature of the study.

Reviewer Comment: I encourage the authors to moderate several of the stronger inferences. In particular, the suggestion that the preferred scenarios imply pore-water salinity near seawater values and higher geothermal heat flux, the interpretation of deeper high-resistivity signals as hydrocarbons rather than deeper permafrost, and the suggestion that localized DTS anomalies may indicate methane seepage all seem more speculative than the manuscript currently acknowledges. These are interesting hypotheses, but they should be presented as such. At present, the wording in several places makes them sound more firmly established than the results justify.

Author Response: Yes, several interpretations within the manuscript should be framed more cautiously. We wanted to present these observations as plausible hypotheses and scenario-based interpretations rather than definitive conclusions. In the revised manuscript, we will moderate the wording associated with potential hydrocarbon-related resistivity signals to more clearly reflect the uncertainty and exploratory nature of these findings.

Reviewer Comment: Please reconsider the use of "eighteen models." This is likely to be interpreted by many readers as eighteen distinct models, whereas this is one simulator/framework explored across eighteen configurations.

Author Response: This is a solid point. We agree that the phrasing could be interpreted ambiguously. In the revised manuscript, we will revise the wording throughout to more clearly distinguish between the numerical modeling framework itself and the eighteen individual simulation scenarios/configurations explored within the study.

Reviewer Comment: There are several language issues and small editorial problems that should be corrected in revision. Examples include:

- “ResFrac Coporation” in the author affiliation.
- Line 199: “These were chosed”.
- Line 392: “~450 m..” contains a double period.
- Line 413: “? - a companion study. . .”.

Author Response: Thank you for pointing out these small editorial problems. We will make sure to address them in the revised manuscript.

Reviewer Comment: The data availability statement still includes a “doi:TBD,” and several cited outputs are listed as “in prep.” This may be acceptable at the discussion stage, but the authors should ensure these are updated before final publication.

Author Response: We have noted with the editor that based on institutional controls, we cannot publicly release the data until the manuscript is formally accepted. At that time, we will correct this and provide the appropriate DOI and any other important/identifying information.