This manuscript addresses a timely and important problem: the thermo-hydro-saline coupling that governs seawater-intrusion-driven thaw of coastal permafrost. The authors present a fully coupled numerical model and use it to conduct sensitivity analyses on seawater temperature, permeability, and porosity. The identification of a transition from single to double circulation and the introduction of the salt contribution degree are interesting contributions. However, methodological, and interpretive issues should be addressed before the manuscript can be considered for publication.

1. Permafrost is defined as regions where ice saturation exceeds 30% of pore space (Section 2.4), but no justification or citation is given for this threshold. Please state the basis for the 30% criterion. In addition, PV is reported in m², i.e., a two-dimensional equivalent thaw area; this should be stated explicitly at first use so that *PV* values are not misread as volumetric quantities.
2. The Discussion states that "the modeled ice-bearing permafrost table depth after 1400 years of inundation (16.7 m with salt diffusion) matches their field data only when advection is included, whereas a purely diffusive scenario underpredicts that by ~27%". This is potentially confusing because the present simulations extend only to 60,000 days (~164 years). Please rewrite to make clear that the 1400-year figure and the 27% underprediction come from Angelopoulos et al. (2019), and explain how the present advection-inclusive mechanism is consistent with their finding, rather than implying that this study ran 1400-year simulations.
3. The manuscript argues that viscosity u can be treated as constant because subsurface temperature differences are within 5 °C. This is reasonable, but a short quantitative statement would strengthen it: over 0-5 °C the dynamic viscosity of water decreases by roughly 15% (from ~1.79×10^{-3} to ~1.52×10^{-3} Pa·s), which is small relative to the temperature- and salinity-induced density contrasts driving flow. More sentences suffice to clarify.
4. The sensitivity analysis currently covers seawater temperature Ts, intrinsic permeability *k*, and porosity *n*. However, the salinity- and temperature-dependent soil freezing characteristic (SFC) curve in Eq. (6) is controlled by the residual water saturation Swres and the fitting parameter W(c), and only a single value (Swres=0.05) is adopted. Because Swres directly sets the unfrozen water content that persists under fully frozen conditions, it controls the latent-heat release during phase change, the width of the phase-change interval, and hence the sharpness of the thaw front and the timing of the single-to-double circulation transition td*c*. A dedicated sensitivity analysis of Swres is therefore warranted.
5. In addition, several parameters (geothermal heat flux, top boundary temperature, and dispersivity) that are likely to contribute substantially to model uncertainty are currently fixed without justification or discussion. To strengthen confidence in the reported results, the following should be addressed.
6. This distinction between the current permafrost modeling and models from previous study from Xiayang Yu et al., (2026) can be stated clearly, and its implications for the modeled thaw-front morphology.

7. The emergence of the counterclockwise cell is an interesting finding of the study, yet its driving force is described somewhat loosely as "thermal convection." The authors should decompose the buoyancy budget explicitly: how much of the counterclockwise circulation is driven by the temperature contrast between seawater and the cold interior, how much by the salinity contrast, and how much by the interaction between the two (double-diffusive effects)?