NEMO-GSL v1.0: An integrated hydrodynamic and ice-process model for a deep subarctic lake (Great Slave Lake, Canada)
Responses to Referee 2

November 20, 2026

We thank the referee for the positive assessment and the specific, technically detailed questions. The question on conservative temperature identified an error in our evaluation: model temperatures had not been converted to in-situ temperature. All temperature statistics have been recomputed. As described in our response to Referee 3, we also corrected errors in the evaporation evaluation and reran all simulations without an evaporation scaling factor. With both changes, the overall warm bias of the temperature profiles reported in the submitted manuscript has been removed. The referee's comments are in black and our responses in blue. Section, figure and table numbers refer to the revised manuscript, and all works cited here are listed in its reference list.

This is well-meant installation of NEMO5 in an area of scarce data. Not groundbreaking but interesting nonetheless in a setting so difficult. I am impressed by the speed at which the authors were able to set up a new model (NEMO5) in a new domain (Great Slave Lake). The mixed layer depth is quite accurately represented, and SST errors are within an acceptable range. The authors do note the high sensitivity of the model to atmospheric forcing, which is a good exercise and emphasize the need for accurate atmospheric dataset in northern Canada. Well-written manuscript. A few minor questions really remain though before publication.

We thank the referee for this positive assessment.

The choice of TEOS-10 for the equation of state rejoices me. Can you elaborate on the choice of absolute salinity? Was it based on some measurement?

As stated in Sect. 3.3, the absolute salinity of 0.2 g kg^{-1} is a lake-wide representative value derived from water quality observations in Great Slave Lake archived on Mackenzie DataStream. A single value does not capture the differences in dissolved solids between the West Arm, which receives the more mineralized inflows of the Slave and Hay rivers, and the more dilute East Arm. The same value is used for the river inflows, and ambient salt is removed at the outflow cells, so salinity does not contribute to horizontal density gradients (Sects. 3.3 and 3.4). A strictly positive salinity is also required by NEMO, particularly beneath the ice, where salt fluxes could otherwise drive salinity towards zero in shallow cells. We acknowledge that TEOS-10 assumes the ionic composition of seawater, which differs from that of lake water. At such low salinities, however, density is controlled by temperature: the temperature

of maximum density at $S_A = 0.2$ g kg^{-1} is 3.93 °C, compared with 3.97 °C for pure water.

Do you convert from conservative temperature to in-situ when comparing for SST? (if not please provide an estimate of the bias)

We thank the referee for this question, which identified an error. In the submitted manuscript, simulated conservative temperature was compared directly with the in-situ observations. For fresh water this is not a small correction: at $S_A = 0.2$ g kg^{-1}, conservative temperature exceeds in-situ temperature by about 5% of its Celsius value, that is, by 0.24, 0.55, 0.80 and 1.04 °C at in-situ temperatures of 4, 10, 15 and 20 °C, almost independently of pressure over the depth of the lake. Simulated conservative temperature is now converted to in-situ temperature with the TEOS-10 GSW toolbox (`gsw.t_from_CT`, $S_A = 0.2$ g kg^{-1}, pressure from depth) before comparison with the observations (Sect. 3.5), and all metrics and figures have been recomputed. Together with the rerun of the simulations without the evaporation scaling factor (see our response to Referee 3), this changed the overall ME of the temperature profiles from $+0.23$ to -0.01 °C and the RMSE from 2.08 to 2.01 °C. For LSWT, the ME changed from $+0.39$ to -0.39 °C and the RMSE from 1.68 to 1.66 °C.

k-omega is not often used in NEMO applications (though promising), could you elaborate on the reason for this choice? Was it proved better for lake applications?

The choice was based on sensitivity tests with the four closures available in the NEMO GLS framework. We have not shown that k–ω is generally better for lakes, and the revised manuscript does not claim this. In these tests, which were carried out without the evaporation scaling factor, k–ε and k–ω performed similarly. k–ε gave a slightly lower temperature-profile RMSE (1.85 vs. 2.01 °C), whereas k–ω gave lower RMSE for MLD (5.42 vs. 5.56 m) and LSWT (1.66 vs. 1.68 °C) and more accurate freeze-up dates with the calibrated parameter set (see the table in our response to Referee 1). We therefore retained k–ω as the best overall compromise. As now stated in Sect. 3.3, the k–ω formulation has been extended for stratified geophysical flows and wave-affected surface layers (Umlauf et al., 2003), and differences between two-equation closures are small compared with the influence of the stability functions and boundary conditions (Warner et al., 2005; Reffray et al., 2015).

The choice of ice embedding/loading is interesting. The option of `ln_ice_embd=T` is still usable with no ice dynamics (as long as the ice code provides total ice+snow mass locally). However, I understand that not everything can be tested in such a quick attempt for results and output SSH would still need to be corrected (for ice loading) for lake stage-discharge relationship. Thus, the simple choice of correcting the lake water level over the whole lake with the total mass of ice makes sense in that perspective. Was this diagnostic done inline or offline?

As the referee notes, the surface height output by NEMO reflects the water level beneath the ice, whereas the stage–discharge relationship requires the level measured at the gauges. We therefore add the lake-mean ice and snow mass, converted to a water-level equivalent, before computing the outflow (Sect. 3.4). The correction and the outflow computation are performed online, on a daily time step, in the modified river runoff routine, and the corrected level is written as an additional diagnostic alongside the standard NEMO surface height.

Lines 371,458: Semantic rabbit hole here: as I am working in a prediction center, “prediction”, “predicted” mean something very precise, that is, this is an initial value problem (we start with the best estimate of the ocean-atm-ice state) and let the model go. Your case is more of a boundary value problem (the atmospheric forcing, rivers, and given the significant spinup period used for the model). Therefore, expressions such as “overestimation” and “simulated” or “modelled” sounds more appropriate.

We agree and thank the referee for the clarification. The simulations are a boundary-value problem: the model is driven by atmospheric and river forcing after a five-year spin-up and is not initialized from an observed state. We have replaced "predict", "predicted" and "prediction" with "simulate", "simulated" or "modelled" throughout, including at the lines indicated; "prediction" now appears only where the manuscript refers to Environment and Climate Change Canada's prediction systems. For the same reason, the configuration is no longer described as an "operational tool" in the Conclusions.

Line 481: "northwestern winds", given the Ekman transport to the northeast being reported, I think the authors meant "southeasterly winds" (meteorologists report the wind direction from its source, not its destination). Or alternatively, "winds blowing in the NW direction" would work too. Please ensure that the caption describing the wind roses reflects which definition for the direction was used.

The referee is correct; the submitted text was wrong. In the meteorological convention, the 1998 wind rose (Fig. 16) shows two dominant directions, southeasterly winds, which are the most frequent, and less frequent but often stronger northwesterly winds, as now stated in Sect. 4.8. Sect. 5.4 no longer describes the 1998 winds as persistently northwesterly. It now explains the mean 1998 upper-layer circulation as a combination of the two wind regimes. The southward flow over the central West Arm is consistent with the northwesterly events. The eastward surface flow from the West Arm into the East Arm through the northern passages is consistent with the southeasterly winds, since wind blowing towards the northwest produces Ekman transport to its right, towards the northeast. As described in our response to the following comment, density differences between the arms also favour this eastward surface flow. Both wind-rose captions state that directions follow the meteorological convention, that is, the direction from which the wind blows.

Line 483-484: "A net loss of water from East Arm to the West Arm is expected due to minor river inflows into the East Arm, and the only outflow being the Mackenzie River in the western part of the lake". I am not sure which dynamic process is involved here. The river inflow and lake outlet in the West Arm creates a SSH gradient which via Coriolis can lead to the formation of an anticlockwise lake circulation, granted. However, how far this circulation can entrain the waters of the East Arm remains a bit speculative to me without deeper analysis. I speculate that the bathymetric constriction at the entrance of the East Arm may dissipate any large-scale circulation to take place there. Additionally, steric height differences may be in favor of a surface flow from west to east in summer, unless Coriolis deviates too much of it.

We agree that the statement was speculative and conflated two processes. Mass conservation requires a net transport out of the East Arm equal to its river inflow plus precipitation minus evaporation, but this net flux is small compared with the gross wind- and density-driven exchange through the connecting channel, and it does not determine the circulation pattern. We have revised Sect. 5.4 accordingly. We also agree with the referee's points on the bathymetric constriction and on steric height differences, and the revised text now includes both. Regarding the Coriolis deflection, the passages are only a few internal Rossby radii wide, so rotation may displace the eastward surface flow towards one side but does not prevent it. The new text reads: *"The opposing directions of upper- and lower-layer flow between the West and East Arms in 1998 indicate a two-layer exchange, in which warm surface water moves east through the northern passages and colder subsurface water returns west. Two processes favour this exchange. The southeasterly winds, blowing towards the northwest, drive upper-layer Ekman transport to their right, towards the northeast, supplying West Arm surface water to the entrances of the East Arm. In summer, the warmer, lower-density surface water of the West Arm also favours eastward surface flow, while the denser water of the East Arm favours a westward return flow at depth. Because the passages are only a few internal Rossby radii wide, rotation may displace this flow towards one side of the passages but does not prevent it. The bathymetric constriction at the entrance of the East Arm may also limit the*

penetration of the basin-scale counterclockwise circulation of the West Arm. Mass conservation requires a small net outflow from the East Arm, which receives river inflow while the only outflow, the Mackenzie River, is in the West Arm, but this net flux does not determine the exchange between the arms. The simulated exchange is therefore two-layered and driven by wind and density differences rather than by a uniform net flow. Quantifying it requires dedicated analysis, which is the subject of a companion study using passive tracers, currently in preparation."