Review of Jovanovic et al. titled 'Simulating Greenland Ice Slabs and Firn Aquifers with a 1D Firn Model'

The manuscript uses a one-dimensional Lagrangian firn model, TFM, to simulate the formation of ice slabs and firn aquifers along Greenland glacier flowlines. The model solves Richards' equation for water flow and is compared with RetMIP results and observations. The topic is relevant, but it remains unclear what TFM improves upon relative to existing firn models. I also have concerns about the treatment of heat transport, saturated flow, impermeable ice layers, and numerical convergence. These issues need to be addressed before the results can be properly evaluated. I recommend major revision.

Major comments

1. The authors should clarify what TFM does better than existing firn models such as IMAU-FDM. Solving Richards' equation instead of using a bucket scheme is a methodological difference, but it does not by itself establish better predictions. The RetMIP comparisons mainly show qualitative agreement with other models. Does TFM improve predictions of infiltration depth, refreezing, ice-slab thickness, or firn-aquifer formation? A quantitative comparison would help establish the benefit of the additional complexity.

2. Lines 149-152: The authors state that "the Lagrangian formulation of the model results in convection phenomena being treated implicitly." I do not think this is generally correct. A Lagrangian formulation can account for the motion of the firn matrix, but it does not automatically include heat advected by water moving relative to the firn. Equation (17) contains only heat conduction. Where is the advective heat flux associated with liquid-water flow? The authors should give the full energy balance and explain how the water-flow and heat equations are coupled. They should also check the sign of the conduction term in Equation (17).

3. Lines 185-193: The freezing model limits refreezing by the available pore space. However, water expands when it freezes, and freezing in confined pores can generate stresses and possibly cracks. What happens to liquid water once the available pore space is exhausted? Also, does the model account for volumetric expansion after refreezing, e.g., a fully saturated region of liquid water freezes to expand ~ 1.1 times its volume as liquid water phase? Further, does the model allow any changes in permeability associated with freezing-induced damage? If these processes are excluded, the authors should discuss the implications for ice-layer formation.

4. The manuscript does not adequately discuss preferential flow in firn. Preferential infiltration can transport meltwater below a refreezing front and change the depth at which ice layers form (Humphrey et al., 2012; Moure et al., 2023). How might the exclusion of preferential flow affect the simulated infiltration depths and refreezing patterns? The authors do not need to implement preferential flow, but they should discuss this limitation.

5. Figures 3 and 6-8; lines 360 and 488-505: How does the model handle fully saturated regions, such as perched firn aquifers, and ice layers with negligible permeability? The authors acknowledge

that the Richards-equation solver has difficulties near saturation and that the Shimizu permeability parametrisation permits water to pass through dense ice layers. They also acknowledge that this can lead to excessive aquifer and ice-slab thickness. How sensitive are the main results to these limitations? In particular, the model should not allow downward flow through layers that have reached pore close-off. The treatment of unsaturated-saturated flow and impermeable layers should be discussed in the context of recent alternatives, such as Shadab et al. (2026).

6. Figure A2: The Dye-2 simulation appears to show meltwater reaching depths greater than 4 m in the 2016 experiment, whereas Heilig et al. (2018) report a maximum observed percolation depth of approximately 2.26 m in 2016. Why does the model predict deeper infiltration? The authors should compare the simulated and observed penetration depths for the same period and discuss possible reasons for the discrepancy.

7. Lines 488-497: The authors discuss difficulties in solving Richards' equation, including solver stagnation near saturation, but provide little information on the numerical performance of TFM. What regularisation and nonlinear solution methods are used? What are the convergence criteria? The authors should test sensitivity to vertical grid spacing, time step, and domain size. Benchmark problems for variably saturated flow, including those discussed by Shadab and Hesse (2024), may be useful. Without numerical convergence tests, it is difficult to determine whether some features are physical or numerical artifacts.

8. The RetMIP comparisons are useful, but comparison with other numerical models is not sufficient for verification. I suggest testing the model against theoretical solutions, such as those of Meyer and Hewitt (2017) and/or Shadab et al. (2025). These provide known solutions for idealized meltwater infiltration and refreezing problems. The authors could compare TFM against the appropriate limiting cases, taking account of the assumptions used to derive those solutions.

9. Lines 467-469: Tracking a firn column along a flowline reduces numerical cost, but it does not resolve multidimensional water and heat transport. Lateral drainage can be important above ice layers and within firn aquifers. The authors should make clearer which parts of the predicted ice-slab and aquifer evolution can be represented by a one-dimensional model and which depend on unresolved lateral processes.

Minor comments

1. The discussion of ice-layer formation around line 39 should include more relevant work, particularly Culberg et al. (2021) and Shadab et al. (2024). The latter discusses an ice-layer formation mechanism that does not require ponding on a pre-existing low-permeability layer.

2. Line 78, Equation (1): The constitutive relation is difficult to follow. Please improve the notation and formatting.

3. Line 105: Please state explicitly whether z is an elevation or a depth coordinate. The sign of the gravity term in Richards' equation depends on this convention.

4. Equation (10): There is a typo in the expression for n_{VG}.

5. Lines 115-116: Please explain the definition $\theta_s = (\rho_{\mathrm{ice}} - \rho)/\rho_w$. If ρ denotes dry firn density, the geometric porosity is $1 - \rho/\rho_{\mathrm{ice}}$. The distinction matters when converting pore volume into volumetric water content.

6. Line 135: Referring readers to Schultz (2024, p. 212 ff.) is not enough. The manuscript should briefly describe the discretisation and numerical solution of Richards' equation, including how it is coupled to freezing and heat transport.

7. Equation (20): The authors use a linear mixture approximation for thermal conductivity when water is present, although the conductivity of firn depends nonlinearly on density and microstructure. How sensitive are the results to this approximation? The authors could compare it with another density-dependent relationship, such as Yen (1981).

8. Line 185: The discussion of meltwater transport and refreezing would benefit from references to earlier work on infiltration fronts, such as Colbeck (1976).

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