We thank the referee for their constructive review. Please find below our point by point response to the review. The comment of the referee are shown in blue and our response in black below. Proposed modifications of the manuscript are shown in green with page and line numbering corresponding to the preprint version of the article.

The authors present an approach to numerical modeling of snowpack or glacier interface with atmosphere using a finite volume method discretization of thermodynamic relations. The novelty of the approach lies in coupled computation of heat transfer through the ice/snow and the thermodynamic balance at the surface. The authors provide sufficient numerical experiments to support the agreement of their implementation with previously published results.
The only critical comment I would like to make is the relatively vague mathematical description of their approach, or the problem at hand. The authors discus the Fourier's law for the heat transfer in ice (Equation 1) and the balance of energy fluxes at the ice surface (Equation 3). Then, they immediately follow on to numerical discretization, leaving the reader curious as to what assumptions and specific method choices they made. I would outline below a few of my concerns.

We revised the manuscript trying to be more precise on the mathematical framework and on the notations. We hope the following modifications clarified the text.

The authors start with the heat equation:

$\partial t h - \mathrm{div}(\lambda\, \mathrm{grad}(T)) = Q$
where
$h = c_p(T-T_0) + \rho_w L \theta$

which leads to

$c_p \partial_t T + \rho_w L \partial_t \theta - \mathrm{div}(\lambda\, \mathrm{grad}(T)) = Q. \qquad (1)$

In the subsequent paragraph they discuss issues with representing the effects of phase changes on the temperature, but I believe they mean that they neglect the $\rho_w L \partial_t \theta$ term in their model. Please state that clearly.

Yes, we meant that while solving the processes of heat conduction and shortwave absorption, we neglect the $\rho_w L \partial_t \theta$ term, and all accumulated energy is used to modify the temperature, even if the fusion point has been crossed. Note that this term is then used in a second step to re-establish thermal equilibrium between the ice and liquid water. In case of melt/refreeze, the sensible heat ($c_p \partial_t T$ term) and liquid water latent heat ($\rho_w L \partial_t \theta$ term) are both used to create/remove water while maintaining the energy conservation.

This is now rephrased more clearly in the revised manuscript:

P3 - L75
"Note that in Eq. (1) the time derivative of the internal energy content h cannot in principle be replaced by $c_p\, d_t T$, but should also include the term $\rho_w\, L_{fus}\, d_t\theta$. Indeed, once the temperature has reached the fusion point, a further increase in energy translates into an increase in the liquid water content ($d_t\theta$!= 0) and of the associated latent heat content, rather than a further increase in the temperature. Yet, as discussed below, snowpack and glacier models nonetheless usually consider that the temperature can increase past the fusion point when integrating Eq. (1) in time (Vionnet et al., 2012, Sauter et al., 2020). This

is equivalent to neglecting the effects of first-order phase changes (melting and refreezing) on the temperature field, and thus setting $\rho_w L_{fus} d_t\theta$ to zero while solving the heat equation."

Moving on, the authors jump to Equation 5, where they present the discretized version of (1) using finite volumes. It would be useful to state the implicit assumptions here, that the three dimensional equation (1) is now considered as one-dimensional equation

$cp\partial tT - \partial z (\lambda \partial zT) = Q,$

which is then integrated over each "volume", which in this case is segment of length Δzk. This integration, along with replacing the point variables with their volume averages (with abuse of notation: $Tk = 1/\Delta zk \int Tdz$), and using fundamental theorem of calculus (we are in one dimension now, no need for divergence theorem) gives
$\Delta zkcp\partial tT - (\lambda \partial zT)k+1/2 + (\lambda \partial zT)k-1/2 = \Delta zkQ$, where subscripts k+1/2 and k-1/2 refer to the (top and bottom) endpoints of the cell Δzk

We now state directly from Eq. (1) that we are working in a 1D setting.

P3 - L70
"In this article, we assume that the snowpack/glacier can be represented as 1D column, and therefore Eq. (1) should be understood as 1D equation."

For the introduction of Equation (5) we specify that the T_k in represent the average temperature of the k^{th} cell. Moreover, reading the reviewer comment we realized that a subscript k is missing for the temperature in Equation (5). This is now corrected.

At this point the authors introduce the approximation of the $(\lambda \partial zT)k+1/2$ term with Equation 6. I am curious, however, whether it is not better to leave the term $(\lambda \partial zT)z=surf$ at the top of the first layer as is, and replace it with the term G from the surface energy balance equation (3)? I am not sure whether this is the way the authors achieve coupling, or whether they still discretize the temperature gradient at the ice surface using the surface temperature and half of the top layer size?

We were indeed sloppy in the description of the fluxes at the cell boundaries. We believe that the issue arises from the fact the top (and bottom) cell is a special case, which was not reflected in our article. For the top cell, the top flux is not computed using Eq. (6), but rather with the subsurface conduction flux G.
We propose to modify the text to clearly state that Eqs (5) and (6) only applies to interior cells, and that cells touching the top and bottom boundaries needs to include the boundary fluxes (which is G for the top most-cell).

P7 - L183
"where Δz_k is the thickness of the k^{th} cell, c_{pk} its volumetric thermal capacity, Q_k the average volumetric energy source in the cell, and $F_{k+1/2}$ and $F_{k-1/2}$ are the heat conduction fluxes at the top and bottom interfaces of the cell. For internal cells, $F_{k+1/2}$ and $F_{k-1/2}$ correspond to the fluxes between the k^{th} and the $k+1^{th}$ cells and the $k-1^{th}$ and k^{th} cells, respectively. For the top cell $F_{k+1/2}$ corresponds the heat flux leaving towards the surface (i.e. -G) and for the bottom cell $F_{k-1/2}$ corresponds to the flux from the ground."

P7 - L189
"The heat conduction fluxes between cells need to be estimated from the temperatures and thermal conductivities of adjacent cells. The flux $F_{k+1/2}$ between cells k and k+1 is computed as:

Eq. (6)

where $\lambda^{harm}_{k+1/2}$ is the weighted harmonic average of the thermal conductivity of the two adjacent cells. The use of an harmonic average provides better results in the case of layered media such as snow (Kadioglu et al., 2008) and ensures that no heat conduction occurs in case one of the cells is a perfect thermal insulator.
Note that Eq. (6) only applies to fluxes between cells and must be replaced for the two boundary cells, at the top and bottom of the domain. For the bottom cell, a flux between the domain and the ground below must be used as a bottom boundary condition. For the top cell, the heat flux coming from the surface must be used. This flux corresponds to the discretized version of the term G in the SEB, provided in Eq. (10) below."

The authors discuss in lines 103-105 that term G depends on surface temperature and temperature within ice, which indicates that this term is indeed discretized.

This term is discretized using Eq(10), and used instead of F_{k+1/2} for the energy budget of the top-most cell. This is now clearly put in the text:

P7 - L195
"This flux corresponds to the discretized version of the term G in the SEB, provided in Eq. (10) below."

I would urge the authors to provide a more detailed and careful mathematical description of their work, as it would improve the reproducibility of their result, not only for the finite volume method community, but also researchers working with other numerical discretizations.

Following the review of Michael Lehning, we have also added an Appendix describing how to implement an equivalent model using FEM (attached at the end of this response). However, in the FEM framework appears the problem of converting element-wise temperatures into node-wise temperatures. This transformation has no straight-forward answer and requires some additional assumptions that affects the end-result of the simulations. As such, we were not able to integrate a FEM model in comparisons to the FVM ones.

This is now is explained in the new Appendix C, as well as in the main part of the revised manuscript:

P11 - L292
"Finally, a translation of this numerical strategy (including the fictitious variable and the Schur-complement technique) in a FEM framework is presented in Appendix C."

P12 - L329
"Finally, note that we do not include the FEM in this comparison. As detailed in Appendix C, a specificity of FEM models is to rely on a temperature field that can be defined element-wise or node-wise. It is thus required to convert back and forth between these two

representations. However, the relation between the two is not bijective. This prevents an unambiguous transformation from element-wise to node-wise temperatures, affecting the end-result of the simulations. Because of this problem, the FEM is not further explored in this article, as a direct comparison to the FVM models is not possible."