We are thankful the Micheal Lehning for its 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.

General:
The paper presents a review on how to numerically implement the surface energy budget into a certain class of snow and ice models. The paper is very well written and in general presents the material in a clear manner. It is overall considered to be a useful contribution to the scientific community dealing with snow and ice modelling despite its rather theoretical setting, in which conclusions on existing snow and ice models are only possible in a limited way.

In this context, it is mandatory that existing snow and ice models that have schemes that come close to the solution presented here are discussed in sufficient detail. In particular, since for example SNOWPACK uses a finite element method (FEM), for which the nodal temperature is explicitly solved at the surface, it already achieves both aspects of the paper, an explicit surface and a tight coupling with internal heat transfer merely by construction of the FEM. This is true for the original version of SNOWPACK, which is now more than 20 years old. Moreover, the statement in l.81 is not a fair representation of the current state of snow models, since also efforts have been made to implement a coupled solver in SNOWPACK that does not generate temperature overshoots. This was crucial for sea ice simulations, where an additional complexity is created by the fact that the melting point of the snow and ice is a function of salinity, and that salinity in turn is impacted by the phase changes. This means that a simple approach of allowing overshoots to occur and then setting back the temperature to fusion value is not suitable any longer. This has been presented in Wever et al. (2020) and should be discussed in the current paper. The proper acknowledgment of the state of art is necessary and as a consequence limits the novelty of the proposed approach here. It is not acceptable to say "we don't discuss FEM models" as the authors do. This neglect is even more surprising since an overlapping group of authors proposes in another paper to use the FEM method for snow modelling (Brondex et al., 2023).

It is indeed true that FEM offers the advantage of naturally having a surface node, which facilitates the tightly-coupled modeling of the SEB, as done in SNOWPACK. This is now clearly mentioned in the article. We also specified that the choice of our article to focus on FVM is motivated by the fact that the FVM is broadly employed in snowpack/glacier 1D modelling. We also now include a short analysis of the FEM case (see Appendix C and modifications listed below).

P4 - L112
"Moreover, we focus on numerical schemes based on the FVM, as it is the method employed by most models (e.g. Anderson, 1976, Sauter et al., 2020, Westermann et al., 2023). We note that, contrary to the FVM, the use of the finite element method (FEM) naturally incorporates the presence of a surface temperature, which can be used for a fully-coupled treatment of the SEB, as done in SNOWPACK for instance (Bartelt and Lehning, 2002)."

We also clarified throughout the text that the classification that we propose is applies to FVM models only, for instance in the caption of Figure 1:

P6 - Fig 1

"Classification of FVM models with respect to their treatment of the SEB. Class 1: The surface energy and the internal temperatures are solved in a tightly-coupled manner but there is no explicit surface. Class 2: An explicit surface temperature (and surface melting) exists but it is solved in sequential manner with respect to the internal temperatures. Proposed scheme in this article: An explicit surface temperature is considered and is solved in a tightly-coupled manner with the internal temperatures. In the schematic, dots represent the prognostic variables of the schemes (with or without temperature at the surface) while the colors indicate which variables are solved simultaneously."

While the article is mainly focus on FVM, we wanted to include in the revised version a brief comparison with FEM, and explain how some of the points discussed in the paper (namely fictitious variable and linear elimination) can be directly translated in a FEM framework.

Doing so we stumble upon the issue of transforming element-wise energy and temperature (description required for the bucket-scheme for instance) into temperature-wise temperature (required for the FEM solving of the heat equation). This step is non-trivial as (i) it is non-unique and (ii) it can create oscillating node-wise temperature fields. While a solution to this problem has been proposed for SNOWPACK, it could not be directly translated into the sequential treatment adopted in our paper. Our different attempts to implement this elements to nodes transformation had an impact on the simulated surface temperature. Thus, the comparison between the FVM and FEM scheme in terms of accuracy and speed of convergence towards a common solution cannot be pursued in the article.

We propose to present the implementation of the FEM equivalent to the tightly-coupled scheme already discussed in the article. This is done in the new Appendix C (attached at the end of this response) and discussed in the 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, which affects the end-result of our 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."

A second major point to address is the inconsistency and incompleteness with respect to the phase change (fusion) implementation as suggested. If I understand the set-up correctly, you explicitly implement the fusion process at the surface and keep the temperature solution at the phase change temperature with your variable switching formulation supported by the truncation method. But you don't do so below the surface, which generates an inconsistency for the sub-surface heat flux. For example, for the case of shortwave penetration into snow and ice, you would generate temperatures above the melt temperature below the surface, which would lead to an upwards heat flux towards the surface, which is at the melt temperature. But heat would flow downwards in reality. This inconsistency is not even mentioned in section 6.4 and probably has consequences for energy conservation. While the tight coupling and explicit surface are sufficiently investigated with sensitivity cases in the paper, the same needs to be done for this fusion treatment. The effect needs to be quantified and compared to the more classical “overshoot” solution.

While doing our study, we hesitated to include phase-change directly into the internal heat budget. As pointed out by the review, this treatment is closer to the actual physics at play (with phenomena such as the blocking of heat conduction fluxes in an isothermal snowpack). We nonetheless decided not to include this effect as (i) this strategy corresponds to a large portion of current snowpack and glacier models, and (ii) we foremost focus on the treatment of the SEB and a proper study/discussion on internal phase changes would be out the scope we aim for. We note that current models that do not take into the capping of internal temperatures still do include some capping of the surface temperature, since it has a large influence on the SEB (notably through the outgoing longwave radiation).
While neglecting internal phase change when solving the heat equation might lead to a deteriorated estimation of the heat conduction fluxes within the snowpack/glacier, this does not have consequences on the energy conservation of the models. As long as these heat fluxes are consistently distributed, the models remain strictly energy conservative.

To test the influence of including phase-change while solving the internal heat equation, we have implemented versions of the three FVM models used in the article that includes phase-changes directly in the heat equation, as suggested in the referee’s comment. Specifically, this was done using the enthalpy method (Meyer and Hewitt, 2017, Tubini et al., 2021). Comparison with the base versions of the models shows that this inclusion has no effect on the glacier test case (as melting occurs at the surface and not internally) and an effect of a couple of degrees on the surface temperature in the snowpack test-case. Nonetheless, the conclusions of the article on the accuracy and stability of the SEB strategies remain unchanged. This can be seen in the Figures below that compare the results of the convergence study with and without internal phase change. For each figure, the left panel corresponds to the convergence plot of the manuscript (no internal phase change in the heat equation), while the right panel corresponds to the convergence plot taking into account internal phase change in the heat equation.

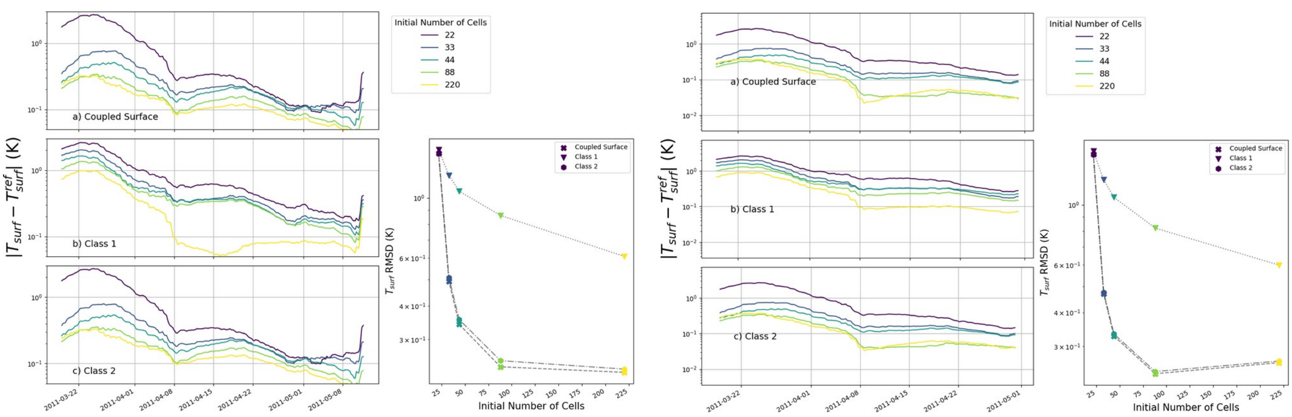

Fig. 1 – *Impact of internal phase-changes on the mesh converge analysis.*

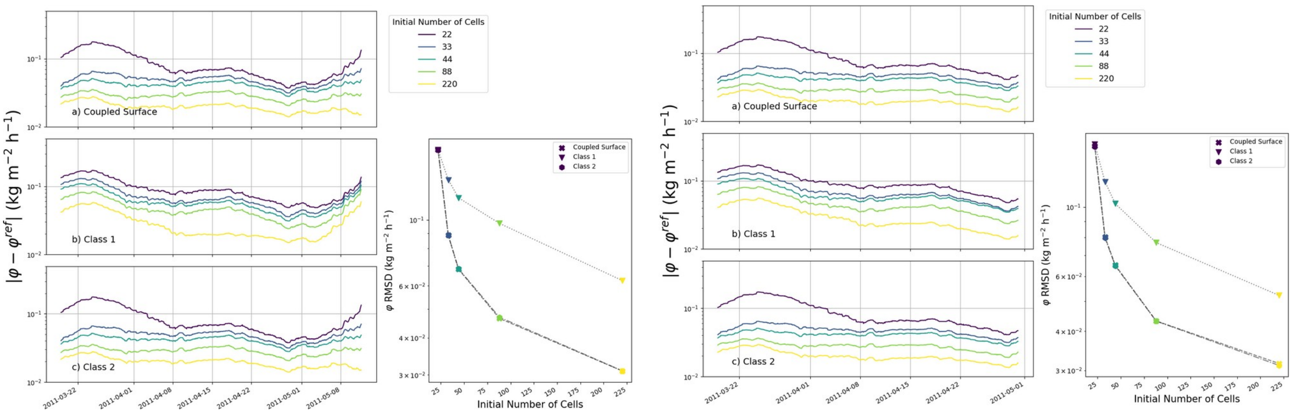

***Fig. 2 –** Impact of internal phase-changes on the mesh converge analysis.*

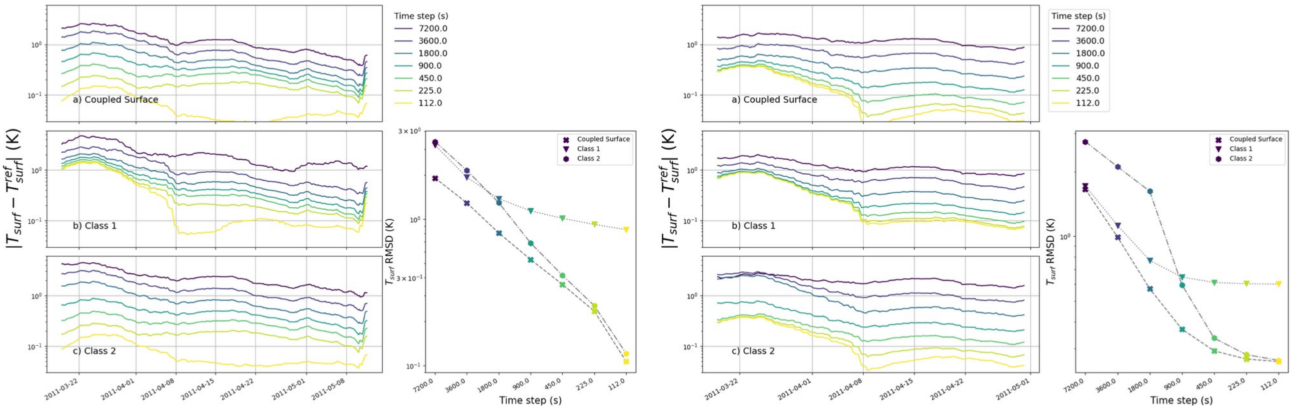

***Fig. 3 –** Impact of internal phase-changes on the time step converge analysis.*

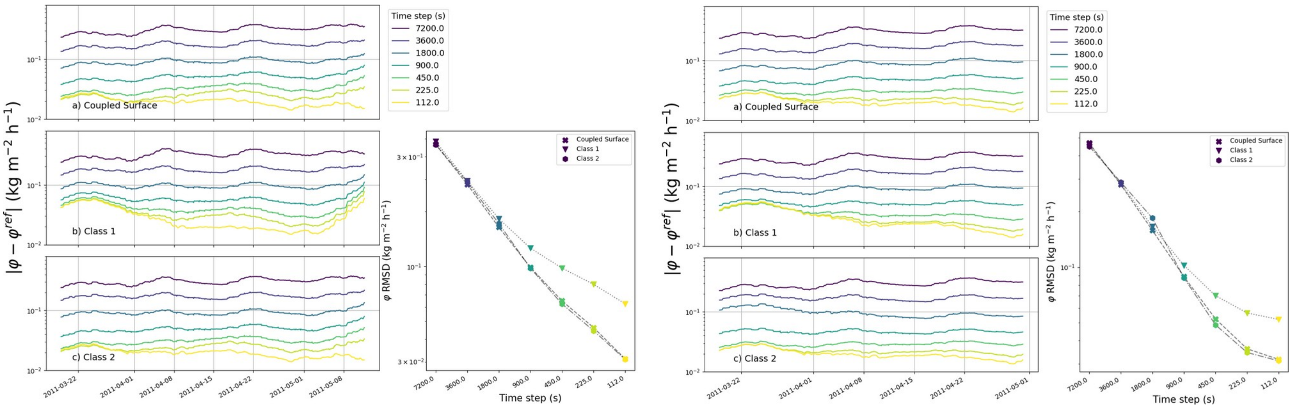

***Fig. 4 –** Impact of internal phase-changes on the time step converge analysis.*

We now mention in the revised manuscript that other strategies have been proposed in the literature, and we have corrected our mistake on the strategy employed by SNOWPACK.

P3 - L81

"This results in temperature overshoots that are then corrected in a second step by creating melt and setting back the temperature to the melt value (e.g. Vionnet et al., 2012, Sauter et al., 2020). In this article, we follow this simple scheme as it is commonly employed in snowpack and glacier models. That being said, other, more complex,

strategies have been proposed in the literature. This notably includes the use of a finite temperature-range over which melt/freezing occurs (e.g. Albert, 1983, Dutra et al., 2010), including melt/refreeze as an additional energy source term (e.g. Bartelt and Lehning, 2002, Wever et al., 2020), or the use of enthalpy as the prognostic variable (e.g. Meyer and Hewitt, 2017, Tubini et al., 2021)."

We also now mention that we have tested the sensitivity of our results to the implementation of phase-changes and that the conclusions of the article remain unchanged.

P12 - L329
"Also, as some of the current snowpack and glacier models include the effect of internal phase-change while solving the internal heat equation (e.g. Bartelt and Lehning, 2002, Meyer and Hewitt, 2017), we quantified the sensitivity of our results to this specific treatment of melt/freeze. For that, we have also implemented versions of our three models that include such internal phase-changes in the heat equation."

P16 - L441
"Finally, using the versions of the models including phase-changes in the heat equation, we quantified the sensitivity of these observations to the treatment of the melt/refreeze. While the simulated temperature sometimes differ from our basic implementations (especially in the snowpack test case where melt occurs internally), the general behavior of the models, including the potential presence of instabilities in the Class 2 models, remain unchanged."

P20 - L493
"Finally, using the versions of the models including phase-changes in the heat equation, we verified that the conclusions of this convergence analysis remain valid in the case of a different treatment of the internal phase-changes"

Minor comments:

1) At least I am more used to the terms "melt" temperature and "heat" capacity instead of "fusion" and "thermal".
We have reformulated "fusion" and "thermal capacity" into "melt" and "heat capacity", except for *"enthalpy of fusion"* as the formulation *"enthalpy of melt(ing)"* appears less common.

2) Eq. (3) does not contain heat advection by precipitation.
We have added a rain precipitation term in the SEB throughout the article.

3) l. 108: Not true, SNOWPACK does not do a separate SEB, see above.
We now specify throughout the manuscript that the proposed classification only applies to FVM models.

4) l. 126: "result" not results.
We have corrected the typo.

5) l. 284: "equation" not equations.
We have replaced sentence with:

P11 - L284

"The system of Eqs.(13) is a 2x2 non-linear system where only A_s and B_s need to be re-assembled at each non-linear iteration and whose solution for U_s is the same as the large system of Eqs. (11)."

6) I don't understand the argument here: "Note that the method used to downscale the data does not guarantee physical consistency of the variables. This allows us to take into account shortwave, longwave and turbulent energy fluxes at the top of our domain".
We wanted to explain that we directly used the forcing data of Potocki et al. (2022), which provides all necessary inputs for the model. However, as briefly discussed in Brun et al. (2023) there are questions about the more appropriate method to downscale ERA5 data to South Col glacier.

As the goal of our article is solely focused on numerical methods and is not meant to address the quality of the forcings, we propose to simply rewrite the sentence to:

P13 - L341

"As such, our simulations are forced by the weather data provided by Potocki et al. (2022) that include all necessary information to take into account the shortwave, longwave and turbulent energy fluxes at the top of our domain."

7) Figures 3,4: These uncertainties should be discussed in light of typical snow/ice model errors.
We now compare the difference between the modeled snow surface temperature with bias observed during the inter-comparison exercise ESM-SnowMIP.

P15 - L432

"As with the glacier test case, the models exhibit surface temperature differences of about a couple of degrees. This is of the same order as the biases observed in the snow model inter-comparison exercise ESM-SnowMIP (Menard et al., 2021)."

Unfortunately, we are not aware of such an inter-comparison model exercise for glacier temperature surfaces. We therefore propose to include a mention of Sauter et al., (2020) which includes a comparison of COSIPY with measured glacier surface temperatures.

P15 - L421

"Concerning the glacier test-case, Fig. 3 shows that the class 1 model (no explicit surface) is systematically different compared to the two other models, with a slower decrease of the surface temperature at night, resulting in a surface temperature that is usually warmer of a couple of degrees for the represented period. For comparison, Sauter et al., (2020) report root mean square errors around 3K when comparing COSIPY simulations with observations of the Zhadang glacier surface temperature."

8) l. 438: why "model 2" now, not clear?
There was indeed a typo here, it the Class 1 model that produces less melt and thus that percolates less. This is now corrected in the text:

P16 - L 438

"This effect is due to the smaller melting predicted by the class 1 model."

9) l. 450 ff. should the reference not be a hundreds (900) of seconds consistent with typical time steps used?

The reference simulation is meant to replace the analytical solutions, that we cannot derive. It is meant to provide the reference toward which the numerical schemes should converge at high spatial and temporal resolutions, and should therefore be obtained with a quite small time step (30s here).

For the range of other tested time step, we decided to go above 900s as some models use larger time steps by default (3600s for COSIPY for instance) and we think it is interesting to analyze the behavior of models at large time step, as such a choice can be motivated to reduce the numerical cost of snowpack/glacier models in large simulation systems such as Earth system models.

P17 - L452

"The largest time step of 7200 s corresponds to twice the default value used for instance in COSIPY (Sauter et al., 2020) and is meant to represent the case of models used at quite large time steps for numerical cost considerations."

10) l. 460: should it be "worse" instead of better?

We wanted to state that sometimes the Class 2 yields smaller error than the scheme we proposed, but that in these cases the Class 2 is only slightly better. This was visibly not clearly enough stated in the manuscript as Richard Essery had the same comment. We revised the sentence to:

P17 - L458

"For almost all investigated time steps and in both test cases, the newly proposed scheme displays the lowest level of errors. Sometimes, the class 2 model yields the smallest error, but does so only by a small margin."

We have also re-formulated a similar sentence later in the manuscript.

P20 - L481

"Again, among the three implementations the tightly-coupled surface model yields the smaller errors for almost all investigated mesh refinements (as in the glacier test case, the class 2 model is however sometimes marginally better)."

11) l. 491: can you explain the deterioration?

This increase of error with smaller mesh size is a result of numerical instabilities, that develop with small mesh sizes. This is now mentioned in the text:

P20 – L 490

"Finally, Fig. (10) reveals that in the glacier test case, the phase change rate errors of the class 2 tend to deteriorate with further mesh refinement past a certain point (here for an initial cell number above 90). We interpret this deterioration as a result of the appearance of numerical instabilities that develop with small mesh sizes."

References:

Brondex, J., Fourteau, K., Dumont, M., Hagenmuller, P., Calonne, N., Tuzet, F., and Löwe, H.: A

finite-element framework to explore the numerical solution of the coupled problem of heat conduction, water vapor diffusion and settlement in dry snow (IvoriFEM v0.1.0), Geosci. Model Dev. Discuss., 2023, 1–50, https://doi.org/10.5194/gmd-2023-97, 2023.

Wever, N., Rossmann, L., Maaß, N., Leonard, K. C., Kaleschke, L., Nicolaus, M., and Lehning, M.: Version 1 of a sea ice module for the physics-based, detailed, multi-layer SNOWPACK model, Geosci. Model Dev., 13, 99–119, https://doi.org/10.5194/gmd-13-99-2020, 2020).

Appendix C: Finite Element Method scheme

In this paper, we focus on the FVM for spatial discretization. However, the heat budget equation could also be spatially discretized with the FEM. Indeed, the FEM naturally includes a node at the surface, and thus possesses a surface temperature, which helps to tightly couple the SEB to the interior of the snowpack/glacier. This strategy is for instance employed in the SNOWPACK model (Bartelt and Lehning, 2002; Wever et al., 2020). Specifically, in SNOWPACK, the coupled SEB is introduced as a top Robin boundary condition.

The goal of this appendix is to briefly present how the techniques presented in the main part of the manuscript (namely the use of fictitious variable and of a Schur-complement) can be used to implement a tightly-coupled FEM model.

C1 Expression of the heat equation in FEM

We consider the mesh of the domain to be discretized into N 1D elements (the direct equivalent of the cells in FVM) and thus of $N+1$ nodes (the end-points of the elements). As classically done with FEM (Pepper and Heinrich, 2005), we assume the temperature field to be a linear combination of basis functions φ_j, i.e. $T(z,t) = \sum_{k=1}^{N} T_j(t)\varphi_j(z)$. Here, we use basic linear elements. In this framework, $T_j(t)$ corresponds to the nodal value of the temperature field (which evolves over time) and the basis functions $\varphi_j(z)$ are piece-wise linear functions, valued 1 at node j and 0 at all other nodes. The standard Galerkin form (Pepper and Heinrich, 2005) of the internal heat budget (Eq. (1)) is:

$$\forall i \quad \sum_j \mathrm{d}_t T_j \int_\Omega c_\mathrm{p}\varphi_j\varphi_i \mathrm{dL} + \sum_j T_j \int_\Omega \lambda\nabla\varphi_j\cdot\nabla\varphi_i \mathrm{dL} = \int_\Omega Q\varphi_i \mathrm{dL} + F_\mathrm{s}\varphi_i(\mathrm{s}) \tag{C1}$$

where Ω represents the domain of simulation, F_s is the energy fluxes entering at the top of the domain (i.e. G), and $\varphi_i(\mathrm{s})$ is the basis function φ_i evaluated at top of the domain. We note that similarly to the FVM case, the temperature at the top of the domain presents a regime change whether the surface is melting or not. To handle this, we rely on the fictitious variable τ, i.e. $T_\mathrm{s} = T_\mathrm{s}(\tau)$. The vector of unknowns, denoted U, is thus composed of the internal temperatures and of the surface fictitious variable. Finally, we have not included any bottom energy flux to lighten the notation, but it could be included easily. Once temporally discretized with a Backward Euler scheme and linearized, the problem can be expressed in matrix form $AU^n = B$, with $A = (M + \Delta t K + \Delta t L)J_T$ and $B = MT^{n-1} + \Delta t Q + \Delta t F$ (T^{n-1} being the vector of temperature from the previous time step), and

$$M(i,j) = \int_\Omega c_\mathrm{p}\varphi_j\varphi_i \mathrm{dL} \tag{C2}$$

$$K(i,j) = \int_\Omega \lambda\nabla\varphi_j\cdot\nabla\varphi_i \mathrm{dL} \tag{C3}$$

$$L(N+1,N+1) = -d_\tau SEB + L_{\text{fus}} d_\tau \dot{m} \tag{C4}$$

$$J_T(i,i) = \begin{cases} 1 & \text{if } i \leq N \\ d_\tau T_\text{s} & \text{else} \end{cases} \tag{C5}$$

$$Q(i) = \int\limits_\Omega Q \varphi_i \text{dL} \tag{C6}$$

and

$$F(N+1) = SEB(\tau^i) - d_\tau SEB \tau^i - \dot{m} + L_{\text{fus}} \left(d_\tau \dot{m} \tau^i \right) \tag{C7}$$

where SEB and $d_\tau SEB$ corresponds to the atmospheric fluxes in the SEB and their derivatives with respect to τ at the current iteration, and $\dot{m}$ and $d_\tau \dot{m}$ are the melting rate and its derivative at the current iteration. In the equations above, only the non-zero terms have been given.

As in the FVM case, this system is composed of a linear-part (the interior, corresponding to the first $N-1$ equations) and a non-linear part (the surface, corresponding to the last two equations). Its solving can thus be accelerated using a Schur-complement technique (Section 4.1.1) by breaking the matrix A into four blocks: a constant $(N-1)\times(N-1)$ diagonal A_{diag} block, a constant $(N-1)\times 2$ vertical A_{up} block, a constant $2\times(N-1)$ horizontal A_{low} block, and a 2×2 diagonal block A_s to be re-computed at each non-linear iteration.

C2 The rest of the model

After solving the coupled heat budgets with FEM, we obtain a nodal temperature field. Since conserved quantities, such as energy or mass, are defined element-wise in snowpack/glacier FEM models (Bartelt and Lehning, 2002), the nodal temperature field needs to be converted into an element-wise energy field. We note that this also defines an element-wise temperature field, where the temperature of an element is simply the average of the nodal temperatures at its end. This element-wise energy field can then be used to simulate melt/refreeze, liquid water percolation, and to remesh the domain using the same routines as in FVM models.

Once all routines for a given time step have been performed, we are left with an element-wise temperature field that needs to be converted back to a nodal temperature field, as required for the FEM. However, this conversion is not straightforward.

First, as we have N element-wise temperatures to transform into $N+1$ nodal temperatures, the problem is not properly closed and an extra (arbitrary) constraint needs to be added. This could, for instance, be setting the surface temperature to the value computed in the SEB. Furthermore, even after choosing an extra constraint to close the problem, the element-wise to node-wise transformation can produce spurious oscillations in the nodal field even if the element-wise field is monotonous (in other words, the transformation does not respect a form of discrete maximum principle; Ciarlet and Raviart, 1973). It is therefore not possible to derive an optimal scheme for this transformation that would (i) not modify the element-wise temperature field and (ii) not create spurious oscillations in the node-wise temperature field.

As spurious oscillations in the temperature field would affect the estimation of the temperature gradients that are used in snow-pack models to estimate metamorphism (e.g. Bartelt and Lehning, 2002; Vionnet et al., 2012), it seems preferable to rather allow the modification of the element-wise temperature field. That being said, such a strategy implies a spatial re-distribution of energy between elements that is not motivated by any underlying physical mechanism. We note that the SNOWPACK model handles this element to node transformation during a phase change step after the liquid percolation scheme, and does so without creating large spurious temperature oscillations.

Unfortunately, it is not possible to directly implement the SNOWPACK scheme in our toy-model, as the sequential treatment is not the same. Moreover, we did not manage to derive a scheme that performs this element to node transformation without affecting the surface temperature. Thus, in our numerical simulations, the FVM and FEM models yield different results. In the absence of an analytical solution, a direct comparison of the FEM and FVM implementations remains impossible.