

# egosphere-2026-510 - Some insights from the second principle of thermodynamics for snowpack modeling

## Answer to Referee 2

We thank the referee for their helpful review of our manuscript. Please find our point by point response to the review below. The text of the review is reported in blue with our corresponding response just below. Proposed addition to the manuscript text are given in green.

### 1. Possible added value of exergy analysis:

The manuscript would benefit from a brief discussion of whether an exergy-based framework could complement the present entropy analysis. Whereas entropy production is well suited to assessing thermodynamic consistency and identifying dissipation mechanisms, exergy could provide an additional and highly relevant physical insight by quantifying where the quality of energy is degraded, where the largest losses of useful energy occur, and which regions of the snowpack are the most dissipative.

This may be particularly valuable in zones characterized by strong thermal gradients. For instance, by associating each heat transfer term with its corresponding Carnot efficiency, it would be possible to identify the regions where exergy destruction is maximal. A similar approach is commonly adopted in thermodynamic cycle analysis, such as in the Rankine cycle, where the boiler is typically identified as the most critical component due to the large irreversibilities occurring at high temperature. By contrast, although the condenser often accounts for a larger energy dissipation from a first-law perspective, the quality of that energy is significantly lower because it is released at low temperature, and therefore the associated exergy remains relatively small. Applying the same concept in the present context could provide a more insightful assessment of the snowpack by identifying the most critical layers, namely those characterized by the highest-quality thermal energy and the largest thermodynamic losses. This could in turn guide the adoption of locally refined meshes or other targeted computational strategies, ultimately leading to a model that can more effectively exploit the second principle to achieve significant improvements in both physical insight and numerical performance.

Even if this extension lies beyond the scope of the present study, a brief discussion of this possible direction would significantly strengthen the manuscript and provide a valuable perspective for future model development.

We are not very familiar with exergy. From the best of our understanding, this corresponds to the work that can be extracted from a system in a surrounding environment maintained at temperature  $T^{\text{ext}}$ , pressure  $p^{\text{ext}}$ , and chemical potential  $\mu^{\text{ext}}$ . The exergy of a system with energy, volume, and matter  $U$ ,  $V$ , and  $N$  is given by  $F = U + p^{\text{ext}}V - T^{\text{ext}}S - \mu^{\text{ext}}N$ . Due to the second principle and the dissipation towards equilibrium, this quantity decreases over time and vanished when the system equilibrates with its surrounding. The rate of exergy destruction is directly linked to that of entropy production  $\psi$  through  $\partial_t F = -T^{\text{ext}}\psi$ .

We are however not sure that the concept of exergy can be used to further the entropy-analysis presented in the manuscript. First, exergy and its decrease are essentially a reformulation of the second principle and entropy production. They should both entail the same physics and relaxation towards equilibrium. Second, the concept of exergy lies on the idea that the surrounding environment is a bath of constant temperature, pressure, and chemical potential. However, snowpacks are driven by space and time varying environments. This is related to the problem discussed in Sect 2.3 of the manuscript: the presence of inhomogeneous boundary conditions restrains the use of classical thermodynamics potentials, such as Gibbs' free energy. From what we understand, the notion of local entropy production is the most general approach, and notions of exergy or Gibbs' free energy are reformulations that apply in specific conditions.

As pointed out by the referee, one can also wonder about the quantitative use the entropy production term. Large entropy sources can indeed be used as indicator of where physical processes are occurring (keeping in mind that temperature gradients, pressure gradients, and chemical potential differences could play this role as well). However, it is not clear that this criterion is the most suited to identify needs for spatial refinement, in comparison for instance to residual based a posteriori error estimators (see for instance [Cancès et al. \(2014\)](#)). Spatial discretization errors are more associated with the spatial patterns of the field to be solved and how irregular it is. Regions of large entropy production can be well represented with coarse grids, for instance when the temperature gradient field is strong but regular, while regions of low entropy production can be poorly captured, when the temperature gradients are weak but vary on a short spatial scale. In other words, while we think the entropy-criterion is valuable to analyze stability, it appears less suited for precision and mesh convergence.

One of the idea we had in mind for the quantitative use of entropy production, was to see whether a local small sink of entropy could be compensated by a larger co-localized entropy source. What we have seen using a sequential treatment of the vapor model (further discussed in point 3), is that the entropy sink associated with phase changes (located in some cells near the crust) could lead to instability issues, despite the large entropy production due head and vapor fluxes (located at the cell interfaces). However, it remains to be analyzed whether this lack of compensation occurs because the entropy terms are not strictly co-located (directly in the cells for the phase changes and at the cell interfaces for the fluxes) and if such a problem would still occur with a strictly concave entropy. This will be mentioned **L792**:

Finally, one can wonder to what extent some entropy destruction might be compensated by even larger entropy production and still lead to a stable model. This could be beneficial to allow some flexibility and to design cheaper but still overall entropy-consistent models. However, in the case of a sequential implementation of the vapor equations, we found that small entropy destruction due to phase changes lead to clear instabilities, despite the presence of a heat flux entropy source several orders of magnitude larger. It remains to be found whether this absence of compensation is related to the non-concavity of the snow entropy or whether it due to the fact that the phase change entropy source and the heat flux entropy source are not strictly co-localized (the first one occurring within the cell and the latter at the interfaces between cells).

## 2. Entropy source in Figure 3:

I have some concerns regarding the interpretation of the entropy-related quantity shown in Figure 3. If this curve represents the internal entropy production (or entropy source) associated with the snowpack-canopy energy exchange, then it should remain non-negative according to the second principle of thermodynamics. In that case, negative values would indeed indicate either a thermodynamic inconsistency, a sign-convention issue, or a possible problem in the formulation. By contrast, if the plotted quantity instead represents the time variation of the system entropy or another term of the entropy balance, then negative values may be physically admissible, particularly in the case of an open system. I therefore suggest that the authors clearly

clarify what quantity is actually plotted in Figure 3 and explicitly explain how it relates to the entropy balance introduced earlier in the manuscript.

The green curves indeed represent the entropy source terms due to heat exchange between the snowpack and the canopy. The second principle would require this quantity to always be positive, but some numerical schemes (specifically here the sequential scheme) can break this law. This breaks the dissipative nature of the system, which no longer necessarily relax towards equilibrium, leading to potential numerical instabilities. To the contrary, entropy-compliant schemes preserve the dissipative nature of the system and the associated stability.

We will specify in the caption of the Figure that the entropy source term of the Figure corresponds to the first term of  $\mathcal{C}^{n+1}$ , i.e.  $F_{sc}^{n \rightarrow n+1} \left( \frac{1}{T_c^{n+1}} - \frac{1}{T_{surf}^{n+1}} \right)$ :

[...] The entropy source related to the snowpack-canopy air energy flux is displayed below in green, with the zero level indicated by a dotted horizontal line. This corresponds to the first term of  $\mathcal{C}^{n+1}$  in Eq. 20 and should be positive to respect the second principle. Note that it blows out of scale for the sequential scheme.

We will also mention it in the text of the manuscript :

These instabilities are associated with a negative entropy source (i.e. entropy destruction) after a few timesteps. This unphysical entropy destruction is due to the inconsistency between the orientation of the net snowpack-canopy air energy flux and the temperatures, that leads to a negative first term of  $\mathcal{C}^{n+1}$  in Eq. 20.

### 3. Need for a baseline comparison model

A quantitative comparison with a baseline first-law-only snowpack model would be particularly valuable. In addition to the general discussion introduced above, I encourage the authors to provide explicit benchmark results comparing the two approaches in terms of timestep sensitivity, computational cost, convergence behavior, and the resulting thermodynamic fields. This would allow the reader to more clearly assess whether the proposed second-law-based formulation provides a measurable practical advantage.

It is true that entropy-compliant models tend to be more complex, involving more operations to be performed, and thus increasing their numerical cost compared to more straight-forward implementations. It is essentially a trade-off of whether the stability provided by the entropy criterion is worth the increase in computational time compared to a simpler implementation.

For the snowpack/canopy, a "baseline" scheme would be the sequential treatment presented in Section 4. Even though a sequential treatment might appear less computationally costly (as it involves fewer operations), it can be difficult to use it in practice because of instability issues that develop unless a very large timestep is used, which greatly limits its applicability. Also, by adding a soil layer in the system and refining the grid at the bottom of the snowpack, we were able to create a model where the sequential scheme is always unstable: at small timesteps instabilities develop at the surface of the snowpack and at large timesteps they develop at its base. Moreover, as detailed in the response to the first review, the coupled snowpack/canopy model can be effectively cheapened with the use of a Schur complement, only marginally increasing the computational cost while maintaining the full stability of the scheme. But implementing such strategies requires a specific and more complex code architecture.

For the vapor model, there are multiple ways of building a non-strictly entropy producing numerical model. The way snowpack models are typically implemented is by solving one equation after the other (i.e. sequen-

tially) but computing the internal fluxes and phase changes using implicit time stepping whenever possible. We have implemented such type of model for the vapor system presented in Eq. 55. Specifically, we:

- 1- Solve the vapor budget computing vapor fluxes and phase changes with an implicit formulation, i.e. taking into account the feedbacks of the change in vapor pressure.
- 2- Solve the ice budget using the previously computed phase change term. Using this pre-computed term is necessary to ensure mass conservation.
- 3- Solve the energy equation using the previously computed vapor flux and with an implicit formulation for the heat flux.

This scheme is not necessarily strictly entropy-producing. Indeed, because the vapor fluxes and phase changes are computed before the final temperature is known in step 3, they might be incompatible with the final vapor pressure and chemical potentials (that are dependent on temperature). Running this model on our test case indicates that while the scheme is overall entropy-producing (notably with a large entropy production due to the heat fluxes), some entropy destruction can occur in the phase changes near the crust. Running simulation with large timestep (30 days here), instabilities develop after some time, yielding negative density values, and this even with downstreamed conductivities and diffusivities. To the contrary this instability does not develop with the strictly entropy-producing scheme. It is also interesting to note that these instabilities in the sequential model are exacerbated with a finer spatial grid.

In terms of computational cost, the sequential scheme essentially consists of two  $N \times N$  tridiagonal matrix problem to be solved. This can be done efficiently with the use of Thomas' algorithm. On the other hand, the coupled-scheme consists of a non-tridiagonal  $3N \times 3N$  matrix. Solving such a problem would be significantly more costly than the sequential scheme, especially for large  $N$ , and in that case a Schur complement approach does not cheapen the computation. We will mention this sequential model in the manuscript **L728**:

As a comparison, we have also implemented a more sequential treatment (and thus not fully-coupled) of the vapor equations. Specifically, the equations are solved in three steps. First, the vapor budget is solved, with the vapor flux and phase change term computed implicitly with respect to  $n_v$ . Then, the ice budget is closed by using the previously computed phase change. Finally, the energy budget is solved by using the previously computed vapor flux and computing the heat flux implicitly with respect to  $u$ . Note that this scheme is no longer strictly entropy-consistent. While it is faster to numerically solve, Fig. 6 shows that this model presents instabilities that develop over the course of the simulation. These instabilities cannot be mitigated with the use of downstreamed transport coefficients. It is notable that they appear in a location where some entropy destruction is associated with the phase change term of Eq. 64. They however disappear by lowering the timestep, such that the sequential model is brought back in its region of conditional stability.

All in all, as mentioned in Section 6.3, we do not think that strict adherence to the second-principle should be a requirement for physical models. This might be over-restrictive and ultimately detrimental when one can ensure that models are used within their region of stability. We will reinforce this point **L783**:

This is to be considered alongside the fact that implementing strictly entropy-consistent models might be cumbersome, as it requires specific material laws (as exemplified by the expression for the saturation pressure in Eq. 52) and tight-coupling between processes and subcomponents. Moreover, as they involve more complex numerical problems, strict entropy-consistent models usually present an overhead cost compared to simpler implementations. In the case of the snowpack/canopy model, the use of Schur complements to couple the subcomponents can strongly mitigate this increased cost (Zhang, 2006). However, such techniques cannot easily be employed for the vapor model of Sect. 5. At the same time, strict entropy-compliance favors the use of relatively large timesteps, without the need to implement adaptive time stepping or rewind strategies. Overall this can result in a significant speed-up compared to less robust models run in their instability regions.

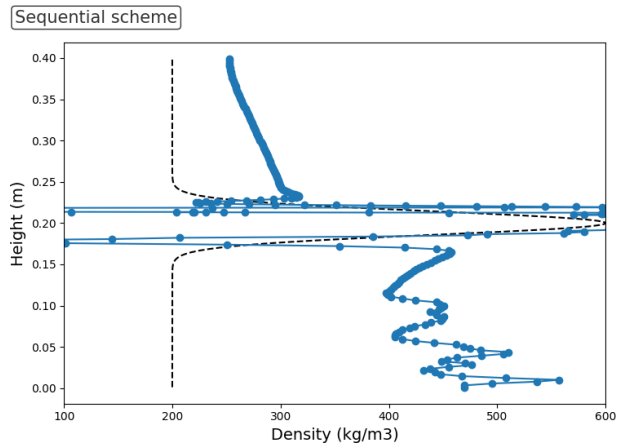


Figure 1: Results of snowpack with vapor model with a sequential resolution of the different equations of the system. Instabilities develop over time despite the use of downstreamed coefficients.

As a consequence, there is a trade-off between the increased stability of entropy-consistent models and their increased cost, which might be beneficial in some situations, but not always. Thus, we think that the compliance of models to the second principle of thermodynamics should rather be viewed as an overall guide and a tool, rather than a strict rule to adhere to.

We agree with the referee that an analysis of the timestep sensitivity / converge rate of the different schemes with vanishing timesteps is an interesting question, but we think it might be outside the scope of this manuscript. We focused on stability as we think it is the first criterion that a model should meet to be usable. Essentially, we want to know if a snowpack model, with an inhomogeneous grid, potentially containing coarse and fine layers, can be expected to yield a physically relevant output when run with an arbitrary timestep, imposed by the rest of the simulation chain or the forcing data. This is one of the main uses of entropy-methods in applied mathematics, where the entropy is linked to Lyapunov functionals.

Concerning the precision level of entropy-compliant schemes, the literature suggests that sequential schemes have so-called "operator-splitting errors" (Keyes et al., 2013). Thus, we can expect coupled schemes to generally yield lower errors. But we think this property is less important than that of unconditional stability and that entropy-criteria might not be the best tool to design models optimized for precision.

#### 4. Row 705:

Around line 705, the authors state that the use of a second principle-complying model allows simulations to be performed with very large time steps without numerical instabilities arising from the faster processes. I believe this statement requires a stronger justification and a more detailed discussion. In particular, I would like to better understand the numerical and physical reasons behind this claimed improvement in stability. Based on the results presented, especially in the first example, some instability or at least questionable behavior appears to be visible, which makes this conclusion less immediately convincing. For this reason, I encourage the authors to provide additional evidence, such as a comparison with a classical first-principle snowpack model, timestep sensitivity analyses, or a clearer explanation of how the second-law formulation specifically suppresses the instabilities associated with the faster processes.

The most common case of numerical instabilities is when fluxes/changes between/in the cells are overestimated and lead to overshoots in the thermodynamics fields. This leads to oscillations one timestep after the other. These overestimations of fluxes/changes can be mitigated by decreasing the timestep (specifically by ensuring that the timestep does not exceed the characteristic times at play in the discretized system). This

is the typical heuristic employed when a simulation fails: restarting it with a smaller timestep. The use of entropy-consistent scheme avoids such overestimations of fluxes/changes, because overshoots violate the second principle. A complementary point of view is that the production of entropy is usually associated with the existence of Lyapunov functionals (keeping in mind the caveat discussed in Sect. 2.3). This ensures that the system is unconditionally Lyapunov-stable, allowing the use of any timestep.

Not that while instabilities appearing for large timesteps is the most common case, problems can also manifest with short timesteps. This is the case for violations of the discrete maximum principle with non co-localized schemes (discussed **L235** of the manuscript) or with the three-way couplings of the snowpack/canopy system in Sect. 4.

The increased stability of entropy-consistent is observed for the different systems of equations presented in the manuscript. For the simple heat equation, schemes that do not follow the second principle are known to display some form of instabilities/oscillations, namely that forward Euler schemes can diverge, Crank-Nicolson schemes can have spurious oscillations with large timestep, and that FEM discretization can have spurious oscillations at small timesteps. To the contrary an entropy-consistent scheme (backward Euler discretization with FVM or lumped FEM) does not suffer from such instabilities or oscillations. For the snowpack/canopy system, the entropy-destructing sequential scheme diverges unless an impractically large timestep is used (and in that case, the model oscillates), while the entropy-producing scheme can be run with any timestep. Finally, for the vapor equations, instabilities that cannot be mitigated with the use of down-streaming develop in the case of an entropy-destructing sequential scheme. This is not the case with the use of a strictly entropy-consistent scheme. As mentioned in response to points 6 and 3 of the review, we will provide more details on the instabilities of non entropy-consistent schemes for the simple heat equation and the vapor system.

We will also specify the point made in **L705** and move them to the end of the Section, after the sequential vapor model has been discussed:

Overall, this simulation highlights that the use of a second principle-complying model allows us to perform simulations with very large time steps, tailored to focus on the slow processes of interest (here the evolution of the density field), without numerical instabilities arising from the faster processes (for instance the relaxation of the temperature field towards equilibrium). In other words, the compliance to the second principle makes such a model robust against the numerical stiffness due to the large time scale differences between the various phenomena at play (Fazio, 2001). The unconditional dissipative nature of entropy-consistent scheme loosens constraints on the choice of the timestep and/or the spatial grid. This possibility of performing simulations with large time steps is notably valuable for applications involving ice sheet snow and firn, which require simulations lasting for decades to millennia (Hoang et al., 2025).

We are however not sure to understand what the referee means by "Based on the results presented, especially in the first example, some instability or at least questionable behavior appears to be visible, which makes this conclusion less immediately convincing". The snowpack/canopy example shows that a non entropy-consistent sequential scheme suffers from instabilities while the entropy-consistent coupled one does not.

## 5. Figures 4 and 5

I would encourage the authors to also display the entropy generation field in Figures 4 and 5. At present, only snow density-related results are presented. Displaying the entropy generation would significantly strengthen the argument of the paper, especially considering that the proposed model is specifically designed to account for the second principle of thermodynamics. If negative values of the entropy source also appear in these cases, this may suggest potential issues in the coupled formulation or in the numerical

implementation More generally, I also recommend including a baseline reference case based on a conventional first-law snowpack formulation, in order to provide a clearer comparison with the proposed approach.

We will add the entropy sources to Fig. 4. They are all positive, by construction of the model, with a dominant contribution from the heat fluxes. We will also mention in the manuscript **L703**:

Moreover, the right panel of Fig. 4 shows that all sources of entropy in the snowpack are positive, and thus that the model is indeed consistent with the second principle.

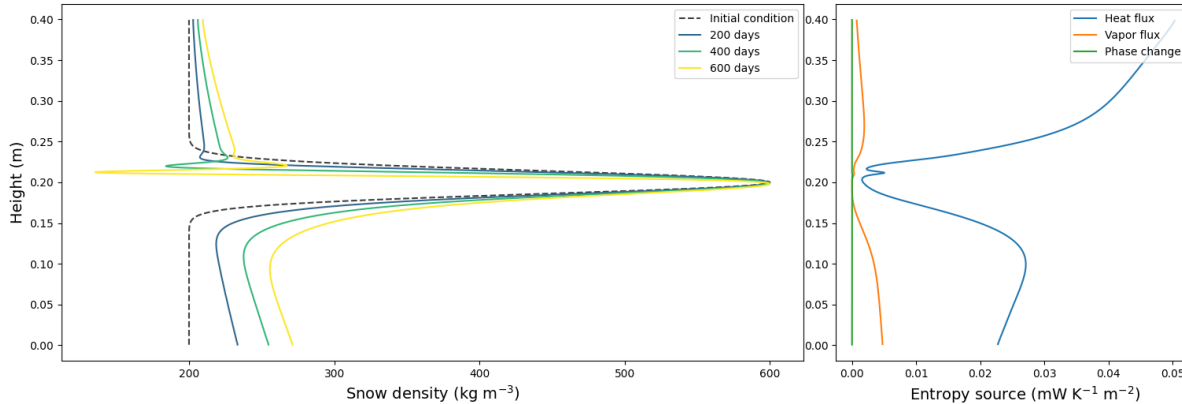


Figure 2: Left panel: Simulation results showing the density evolution of a 40 cm snowpack subjected to a constant temperature gradient of  $100 \text{ K m}^{-1}$  over 600 days. Dashed black line: initial density condition, with a density crust at mid-height. Solid color line: density profile over time, with a downward motion of the density crust and the apparition of density oscillations above. Right panel: associated entropy sources due to heat fluxes, vapor fluxes, and phase changes after 600 days of simulation.

As mentioned in point 3, we will also discuss that a sequential treatment of the vapor equations leads to a small entropy destruction, that nonetheless leads to instabilities appearing over time.

## 6. Third example missing (around line 730)

The manuscript refers to three different cases; however, only two numerical examples are explicitly presented. I would strongly encourage including the third case as well, particularly the simplest conduction-only example, as this would provide a useful validation benchmark. From my point of view, it is important to first present simpler and more accessible examples before moving to the more complex and lengthy formulations involving multiple coupled terms and governing equations. This would help the reader better understand the physical meaning of the proposed entropy-based approach and more clearly assess its added value.

We will present a brief numerical example for the simple conduction problem in Sect 3. We build a test case for a homogeneous snowpack with a fixed top temperature. The numerical discretization is done using FVM and three different timestepping schemes: Forward Euler, Crank-Nicolson, and Backward Euler, only the last one being strictly entropy-producing. The results show that the non entropy-consistent schemes have instabilities leading to unphysical results, while the entropy consistent scheme is stable. We will add a new "Numerical example" section **L266**:

To exemplify these results, we implemented three numerical models based on different time stepping methods, namely a Forward Euler, a Crank-Nicolson, and a Backward Euler scheme. Of the three, only the latter strictly respects the second principle. We used them to solve the simple problem of a homogeneous snow-

pack with a fixed top temperature relaxing towards thermal equilibrium. Results of the simulations are displayed in Fig. 2 below. This indicates that the non entropy-consistent schemes have numerical instabilities, specifically a divergence for the Forward Euler scheme and damped oscillations for the Crank-Nicolson scheme. In both cases, these instabilities are associated with entropy destruction. On the contrary, the entropy-consistent scheme does not show any sign of instability and relaxes towards equilibrium without overshoots. This behavior is associated with a strict production of entropy within the system.

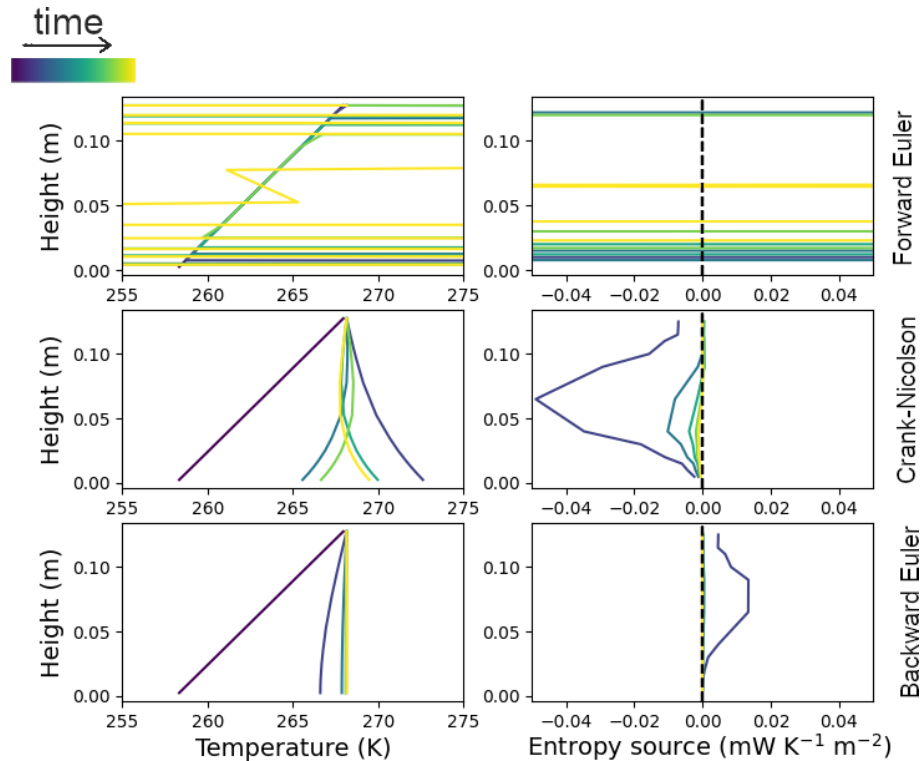


Figure 3: Solution of the simple heat equation for three different time stepping schemes. The temperature field is displayed on the left panel and the associated entropy source on the right panel. Each color represents a given time in the simulation. Note that the Forward Euler results blow out of scale.

## 7. Justification of complexity

At present, the manuscript introduces a large number of additional equations and thermodynamic terms. However, the numerical examples do not yet clearly demonstrate a substantial improvement over more classical modelling approaches. For this reason, I encourage the authors to better justify why this additional level of complexity is necessary and to clearly highlight the measurable advantages it provides, for instance in terms of accuracy, physical consistency, or numerical robustness.

I also think the discussion of numerical stability could be strengthened. While the manuscript suggests that second-law-based consistency may improve robustness, tighter coupling and additional source terms can also make the numerical system more difficult to solve in practice if not handled carefully. It would be valuable to discuss whether alternative numerical strategies for improving stability have been considered, such as under-relaxation techniques, iterative damping methods, adaptive timestep control, or other numerical stabilization approaches. From the perspective of numerical stability, it may therefore be worth discussing whether other approaches could provide similar or improved robustness while introducing a lower level of model complexity.

It is true that adopting a strictly entropy-consistent scheme is usually more complex to implement and to

solve numerically. That is why we mention Sect. 6.3 that we do not think that the unconditional compliance with the second principle should be mandatory. If one can ensure that the model is run in its region of stability, having a strictly entropy-compliant model might be an overhead with little benefits. As mentioned in response to point 3, we will strengthen this point in the revised version of Section 6.3.

Concerning the alternatives, entropy-methods are a tool of analysis for the stability of the sequence of solution for a time-discretized problem. It is not a discretization scheme per se, but provides information on them. An alternative would be linear stability analysis, based on the eigenvalues of the matrix form of the discretized problem. However, entropy methods provide a more general framework, as they can directly be applied to non-linear problems and systems of equations.

To build entropy-consistent models in our manuscript, we rely on fully coupled and fully implicit schemes. But there might indeed be less costly/complex schemes that preserve the entropy-structure of the problem. This would be highly beneficial and will dedicate time to this problem in the future. We will reinforce this point **L788**:

Also, thermodynamics considerations can be used to design efficient coupling strategies, that are less complex than direct tight-coupling, but still ensure entropy non-destruction (e.g. Both et al., 2019; Liu et al., 2021). Future work will be dedicated to search whether this might be harnessed to design simple but robust models and/or coupling strategies between pre-existing models. Finally, one can wonder to what extent some entropy destruction might be compensated by even larger entropy production in another process and still lead to a stable model. This could be beneficial to allow some flexibility and to design cheaper but still overall entropy-consistent models.

Unstable models are indeed usually stabilized using adaptive time stepping. As mentioned in point 4 of this review, this usually brings the model back to its stability region once the timestep falls below the characteristic times at play in the discretized equations. Adaptive timesteps can however be very detrimental in terms of computation time. First they require to check for potential problems and to rewind the simulation to a saved previous state. Moreover, the required time step for stability might be very low (especially for snowpack models with fine layers in their grid) and significantly increase the number of operations for a given simulation without any benefits. Moreover, there are equations (such as the snowpack/canopy of Sect. 4) where there are no regions of stability for vanishing timesteps. In this case, an adaptive strategy would lead to an infinite loop of decreasing timesteps.

Finally, if we understand correctly iterative damping/under-relaxation are techniques employed for iteratively solving non-linear problems. They cannot be applied to stabilize the sequence of solutions timestep after timestep. We note that attempts to employ entropy-criteria to help the convergence of iterative methods have been made, without any clear benefits so far in general nonlinear settings (see Both et al. (2019) for steps in this direction, which cannot be applied to our problem however).

The manuscript is scientifically substantial and generally well structured, but in its current form it is rather long and at times difficult to read. The progression from the heat-equation example to the coupled snow-canopy case and finally to the vapor-transport formulation is logical, but the presentation becomes increasingly dense, especially in the later sections where many equations, variables, and thermodynamic relations are introduced in a short space. I therefore encourage the authors to improve readability by shortening or streamlining some derivations, clarifying the main take-home messages at the beginning and end of each section, and carefully revising the language throughout. At present, the paper reads more like an extended theoretical development than a focused journal article, and a tighter presentation would make its main contributions easier to appreciate.

We indeed wondered what was the appropriate level of details that should be provided in the manuscript while writing it. We could have directly started from the state of the art in applied mathematics and only have provided the entropy-analysis of the snowpack/canopy and vapor systems. But then the manuscript might have been less accessible for people not familiar with this framework. Instead, we tried to provide a self-contained manuscript, but to the risk of being lengthy and re-stating results that are already well-discussed in the literature (i.e. Sects. 2 and 3).

We will try to emphasize the main message of each section in order to ease the reading of the article.

### Minor language revisions (some)

We thank the referee for the language corrections, and will modify the manuscript accordingly.

The authors of egusphere-2026-510

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