Dear Dr. Matthieu Lafaysse,

Thank you very much for your review and constructive comments. The entire text of your referee comment is shown (ML) together with our authors' responses (AR).

Kind regards, Niccolò Tubini – on behalf of all authors

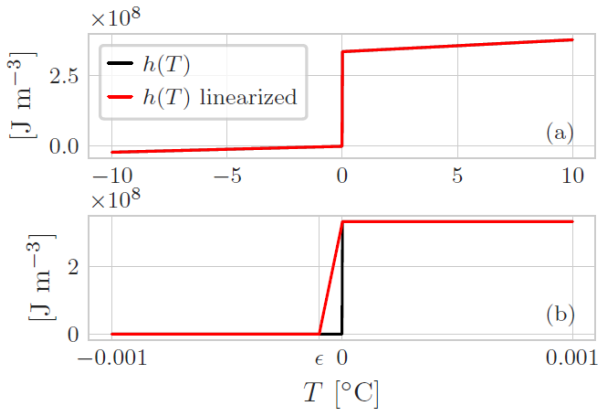
Main comments

1. **ML**: My main concern is the fact that it is rather difficult to understand the algorithm only with the material provided in this paper. The introduction of Section 3.2 and the sentences Lines 220-221 remained really obscure to me even after checking Appendix B. Combining the reading with an attentive analysis of Casulli and Zanolli, 2010 finally allowed me to perfectly understand the process, but it asked me significant efforts because (1) the notations differ between both papers, (2) I could not understand why Fig. 1 and Fig. D1 looked inconsistent with the formulations of enthalpy in Eq. 8 and Eq. D1, and (3) there is room for improvement in the presentation of the algorithm in Appendix B.
    - Therefore, I would first strongly suggest to reproduce Eq. 13-19 of Casulli and Zanolli after Eq. 20 of this paper by using the own notations of this paper. Even if the details of the linearization of the two functions in this
      iterative procedure is already published, I think it would really help the cryosphere modellers to provide again the detailed equations for their understanding of the numerical approach. I actually had to write all the equations myself to finally understand.

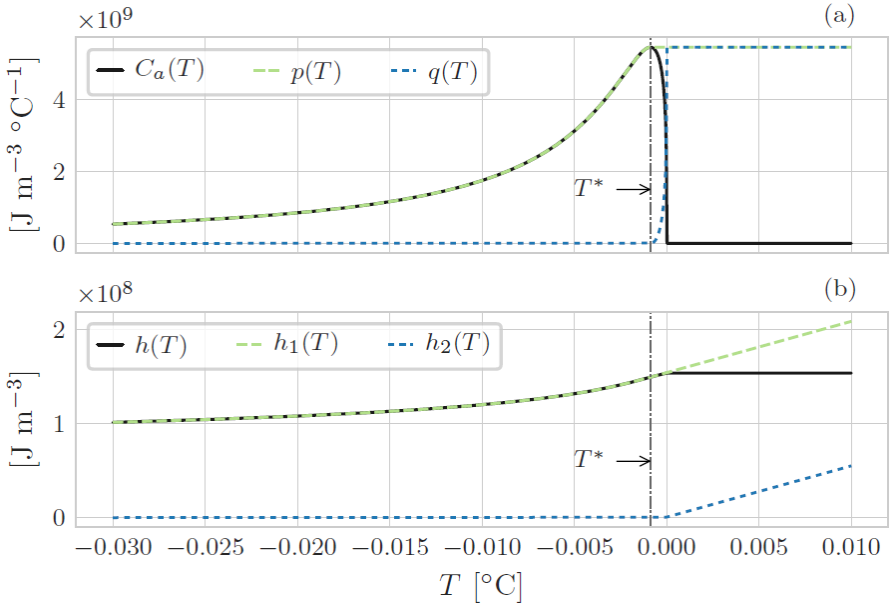
AR: In the resubmitted manuscript we have reproduced Eq. 13-19 of Casulli and Zanolli, 2010 as you suggested. The notation is slightly different since the quantities in the Richards' equation are different from those of the heat equation. But we tried try to keep the same notation when it was possible.

- **ML**: Then, I think that Fig. 1 and D1 should be modified (either the curves, either the axis legends) because H cannot be constant with temperature. Ideally, the plots could also be a bit larger with some trick to better distinguish overlapping curves.

AR: About Fig. D1 the problem is that the temperature range is too small to appreciate the variation of h(T) with T because of the magnitude of latent heat. To help the understanding we modified the Fig. D1 as follows: in (a) we have the enthalpy function, and (b) is a detail on the linearization for the latent heat.



As regards Fig. 1, we have reduced the temperature range and increased the size, it is now a two-columns figure. However, the variation of h with T for T > 0 [°C] is not evident because of the magnitude of latent heat and the small temperature range for T > 0 [°C]



- **ML**: Then, I am sorry to say that Algorithm 1 in Appendix B is really confusing and that the version detailed by Casulli and Zanolli was much clearer for me. First, a key point is missing i.e. the initialization of the guess of the
inner iteration (Ti,0k). The comments "linearize h1" and "linearize h2" are ambiguous because they would suggest that some code instruction is required here whereas it is not the case. rhs is not defined. Is it the b vector of Eq. 20 ? The use of the layer index i in superscript next to the inner iteration index is confusing and putting both iteration indexes at the same level as in Casulli and Zanolli seems a better choice. Furthermore, the layer index i is in subscript in the main text. The meaning of superscript n (previous time step ?) might also be remind. I am not sure if dk is equivalent
to fn,m−1 combined with dn−1 in Casulli and Zanolli ? If yes (I guess so), I think that separating the terms which do not depend on iteration m would help understand the

role of both iterations. Furthermore, it is cheaper to compute them outside the inner loop. Therefore, why not keeping the presentation of Casulli and Zanolli on that point with the computation of d in the outer loop and f in in the inner loop ? Finally, it's a detail but the condition on the residuals to exit the loops is not explicitly formalized: is the threshold applied on each element of the vector or on a norm of the vector?

AR: About the Algorithm 1 in Appendix B, I will report the version by Casulli and Zanolli specifying the index and apex. The threshold is applied on the norm of the vector of the residuals.

2. **ML**: The comparison of numerical results with analytical results is very convincing (Section 4). However, I believe that the comparisons with other numerical approaches is essential if the authors want to convince the cryosphere modelers to change their algorithms. Therefore, I think that the comparison with other numerical models might have found its place in the main text rather than in Appendix G. In the same idea, the comparison presented by the authors with the Newton-Raphson algorithm is not fully representative of the shortcomings of the existing models as the literature review presented by the authors show that a number of models use even worse representations (DECP is the standard for snow models). Of course, I understand that DECP can not be seen as a state-of-the-art reference for the authors, but would it be possible to include it in the comparisons so that modelers using this approach feel more concerned?

   AR: Appendix G has been moved in Section 4.1.
   We understand that a comparison with other model would be of interest. However, running other models over long periods as the one we used in this paper would tremendously delay our paper resubmission. For our assessment we rely on literature and communications with the Authors of some of the paper we cite. Besides a comparison is always tricky because of the insufficient knowledge one researcher has of codes of others. This task is better left to some intercomparison effort where every Author run their code on a common set of benchmarks.

3. **ML**: Finally, my last main remark is that modelling in geoscience is often a compromise between physical accuracy and numerical cost. This paper pays attention to the accuracy and stability of the solution. The numerical cost is considered through the possibility to extend the time step (section 5). However, in a number of surface models, the time step is often constrained by the need to represent the diurnal cycle and the time resolution necessary for the other processes.
   Therefore, I would be interested by a discussion about the numerical cost of the different approaches for a fixed time step (typically 10 to 60 minutes for soil or snow models resolving the diurnal cycle). Indeed, the algorithm proposed by the authors requires to solve $k \times m$ linear systems. It means that the improved accuracy (guaranteed convergence and stability) comes with a potentially much more expensive cost than the approaches published by Voller for ice (iterative but without any linear system to solve), or than the DECP approach commonly used in snow models and requiring the solving of only one linear system. In particular, would it be possible to estimate the number of iterations required in the examples provided in the two test cases? How fast is the convergence in the simple case of heat diffusion without any phase change? Is it possible to estimate an average number of iterations in long simulations based on real forcing conditions?

   AR: The numerics we use is robust, reliable, and realistic (Prentice et al. 2015). It never breaks and it give the required results. In our opinion when using linear methods for the problems under scrutiny, solutions do not match with what is expected for known solutions and mass error are

amplified. But obviously we cannot say for sure of models by other Authors, for the same reasons we have written in the previous answer. For what regards the number of operations performed by the non-linear we did some computation to assess this and our result is reported in the table below. We performed a simulation of 1 year with an hourly time step for the numerical test case reported in Section 5, with different spatial discretizations. The tolerance 10e-11 has been rescaled with the water latent heat of fusion and the water density. The maximum number of iterations for each time step is 40.

# control volumes	500	1000	2000	5000	10000
mean number of iterations NCZ	12	13	14	16	18
mean number of iterations N. R.	40	40	40	40	40
mean number of iterations g. c. N	40	40	40	40	40

As you can see the mean number of iterations is higher compared to the results reported in Casulli and Zanolli 2010 and this is because the derivative of the internal energy function is steeper that that the derivative of the water content function that appears in the Richards equation.

We have compared the performance of our code with a simple Newton-Raphson method (N. R., in the second row) and the so called globally convergent Newton (g. c. N., third row). The problem is that they are not faster, simple Newton does not converge so it always reaches the maximum number of iterations allowed, and the solution as presented in Fig. G2 does not reproduce the analytical solution. If it fails to converge, no time saving is obtained.

Minor comments

1. **ML**: Lines 49-55: It might also be interesting to mention that the notion of interface is also often meaningless in ice and snow where thick isotherm layers of a mixed solid-liquid medium are very common.

   AR: Thank you, we have added it in line 55 of the revised manuscript.

2. **ML**: Line 74: Applications with long time steps also include the surface components of climate models and Numerical Weather Prediction models, and also models dedicated to avalanche hazard forecasting.

   AR: Thank you, added in the revised manuscript in line 75.

3. **ML**: Line 115: "SFCC have an inflection point". With just a quick look at Bao et al., 2016, I could not find to which figure or comment this statement refers. Could you provide the details?

   AR: From Bao et al 2016, Section 1, paragraph 3 “Currently, the most common problems in frozen soil modeling are unstable simulation and heavy computational cost due to the highly nonlinear relationship in soil temperature, soil moisture, and ice content caused by the substantial latent heat associated with the phase change. Thus, how to deal with this highly nonlinear relationship between these three variables in frozen soil is the key focus of frozen soil model development.” They use the term nonlinear relationship.

   We have also added a reference to Hansonn et al. (2004), where Figure 1 shows the apparent heat capacity function with a maximum that correspond to an inflection point in the SFCC function.

4. **ML**: Section 2 is really nice and interesting to read, but a number of symbols definitions come a bit too late after their first use in an equation. For instance, enthalpy already appears in Eq. 1 but is only defined from Eq. 5 to Eq.8. Similarly, latent heat of fusion and liquid water fraction are used in Eq. 3 but are only defined after Eq . 7. It is not critical for the understanding but maybe some reordering could manage to avoid these late definitions.

   AR: Eq. 1 – 4 hold in general, independent of the material considered. This part is meant to present the three different formulations and to highlight that Eq.1 best represents the physical system because it expresses the conservation of enthalpy. By contrast, Eq.2 and Eq.3 are derived from Eq.1 by just applying the chain rule of derivatives. The enthalpy for the soil, as well as the liquid water content are presented later since in our opinion it is not necessary to know how the enthalpy function is defined. Proof of this is that the mathematical model, Eq.1,2,3,4, can be applied also for the Neumann problem and the Lunardini one.

5. **ML**: Eq. 9: Although their meaning is relatively obvious, please do not forget to define indexes i and n.

   AR: Done

6. **ML**: Line 179: I think Eq. 12 corresponds to the implicit case, not semi-implicit, am I wrong?

   AR: No, you are not. There is an error in the apex. It is not n+1 but n and the same applies to Eq. 13.

7. **ML**: Eq. 12: Source terms seem to be expressed at the beginning of the time step in this formalism. In surface models, I think that it is relatively common to express the source terms which depend on temperature at the end of the time step in order to improve the stability especially because of the longwave surface radiation function of surface temperature. I think this does not affect the possibility to apply this algorithm because when these terms are linearized, it just adds terms in the coefficients of the A tridiagonal matrix and in the b vector without changing the formalism. But maybe the authors could just mention that source terms do not necessarily have to be expressed at the beginning of the time step to allow this algorithm to be applied.

   AR: As regards the up-welling longwave surface radiation, does it not enter in the governing equation since it represents the boundary condition of the problem (the surface energy budget). In this case it enters in the discretized equation for the uppermost control volume in the numerical flux through the upper boundary representing the soil surface.

   The source/sink term S could be expressed in an implicit manner if the matrix A remains at least positive semidefinite and symmetric matrix. Thus, it is necessary to pay attention that the implicit discretization of S does not affect the feature of A: entries on the main diagonal should be positive, off diagonal should be negative and symmetric. If the requirements on A are fulfilled, then the NCZ algorithm can be used.

8. **ML**: Line 432 It was solved

   AR: Done

9. **ML**: Lines 436-438 This is true, but this development has not really been finalized until now and all recent works using Crocus are still based on a simple bucket approach for liquid water percolation.

   AR: We have reformulated as “Even though all recent works using Crocus are still based on a simple bucket approach for liquid water percolation (Morin et al., 2012; Lafaysse et al.,2017), D’Amboise et al. (2017) implemented a routine for water flow in the snowpack based on the Richards equation, which is characterized by nonlinear behaviour like the enthalpy equation. To

solve it, they adopted an approach based on Picard iteration with variable time steps (as in Paniconi and Putti, 1994)."

10. **ML**: Table 1 and Appendix A14: I don't really see what is the difference between Crocus and SNOWPACK. I do not understand what means "phase change are accounted for as volumetric heat sinks (melting) and sources (refreezing)" and why a nonlinear solver would be "not required". Is SNOWPACK algorithm really different from the DECP approach? Is it possible to better explain the difference if any?

    AR: Also SNOWPACK uses the DECP approach and the manuscript has been changed, accordingly.

11. **ML**: General comment about Appendix A13 and A14: I also think that most multilayer snow schemes simpler than Crocus and SNOWPACK and typically used in climate or hydrology models are also based on this DECP approach. Maybe it could be mentioned somewhere.

    AR: We have included in the model review also the snow routine of the ORCHIDEE model (A15) and that of the JSBACH model (A16)

Typos

1. **ML**: Line 22 these models,

   AR: Thank you, we have corrected it.

2. **ML**: Line 175 orthogonal

   AR: Thank you, we have corrected it.

3. **ML**: Line 312 to match the aim

   AR: Thank you, we have corrected it.

References

CASULLI, Vincenzo; ZANOLLI, Paola. A nested Newton-type algorithm for finite volume methods solving Richards' equation in mixed form. SIAM Journal on Scientific Computing, 2010, 32.4: 2255-2273.

PRENTICE, I. Colin, et al. Reliable, robust and realistic: the three R's of next-generation land-surface modelling. *Atmospheric Chemistry and Physics*, 2015, 15.10: 5987-6005. https://acp.copernicus.org/articles/15/5987/2015/acp-15-5987-2015.html

HANSSON, Klas, et al. Water flow and heat transport in frozen soil: Numerical solution and freeze–thaw applications. *Vadose Zone Journal*, 2004, 3.2: 693-704.