The Cryosphere

Discussions

Open Access

EGU

Interactive comment on "A method for solving heat transfer with phase change in ice or soil that allows for large time steps while guaranteeing energy conservation" *by* Niccolò Tubini et al.

Matthieu Lafaysse (Referee)

matthieu.lafaysse@meteo.fr

In this paper, the authors present the first application of an iterative algorithm published in 2010 in a mathematics journal to solve the coupled processes of heat diffusion and phase changes (the Stefan problem) in a context easily extendable to any frozen soil, ice or snowpack model. I think this paper may become a major reference for the future generation of models of the cryosphere components. Indeed, I think that the failure of classical algorithms to converge towards stable solutions is poorly known in the community, at least in the snow science community I belong. The innovative character of this paper is indisputable as the references in the literature providing ways to solve

Stefan problems are extremely scarce : the main reference for ice is still Voller (1990 !!), and for snow the problem just seems to be ignored by the community while we recently got proofs that the decoupled treament of current snow models is responsible for significant errors in some situations. The paper has an excellent structure, language and overall quality and it could almost be published as it is. Nevertheless, I have a few comments and suggestions mainly in the idea of better encouraging model developers to consider this approach for their applications by providing all informations they could expect. Nothing is absolutely mandatory but I would be even more enthusiastic if the authors could consider some of these suggestions. In any case, I thank and congratulate the authors for this very high quality paper.

1 Main comments

1. 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 obscur 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.

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

- 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 $(T_k^{i,0})$. 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 d_k is equivalent to $f^{n,m-1}$ combined with d^{n-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 ?

C3

2. 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 ?

3. 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

C4

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 ?

2 Minor comments

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.

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.

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 ?

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.

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

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

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 than source terms do not necessarily have to be expressed at the beginning of the time step to allow this algorithm to be applied.

Line 432 It was solved

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.

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 ?

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.

3 Typos

Line 22 these models

Line 175 orthogonal

Line 312 to match the aim