Review: "A method for solving heat transfer with phase change in ice or soil that allows for large time steps while guaranteeing energy conservation"

by Tubini et al.

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1 General

In this paper, the authors describe a new numerical model for solving phase change problems with applications to the cryosphere. As the authors correctly identify, phase change is central to snow, ice, permafrost, and other components of frozen landscapes. What I like about this paper is that it uses modern numerical methods. The application of Newton-Casulli-Zanolli (NCZ) method to solve the parabolic pdes that arise in phase change is a great idea. What I don't like about this paper is that (i) it is not clear what cryospheric problem they are attempting to solve, (ii) they ignore relevant literature, and (iii) their pseudocode and code are not in a form that is useful for the broader community. Additionally, I wonder if the *The Cryosphere* is the correct venue – I think that *Geoscience Model Development* would be a much better fit for this paper. As it stands, I am hesitant to support publication in its current form.

2 Remarks

1. In the abstract, the authors state that "the nonlinear behaviour of enthalpy as function of temperature can prevent thermal models of snow, ice and frozen soil from converging to the correct solution" but do not provide a description or citation for this claim. Reading further into the paper, this claim is based on a survey of experts, cited as personal communications. I appreciate the point that the authors are trying to make and it is indeed an important advance of the NCZ method, but the phrasing could be improved throughout for clarity.

2. The main advance that this paper reports is the implementation of a novel algorithm for solving phase change problems. However, the pseudocode included in the paper is not very useful as opposed to a well-documented version of the code that is easy to run. While the code is released publicly, the github documentation is unclear, written in java (for good reason), and not approachable. I was hopeful that I could run the code but that did not seem feasible.

3. This paper neglects to cite or engage with Schoof and Hewitt (2016), who derive a general enthalpy model for phase change. Schoof and Hewitt (2016) follows on from Aschwanden and Blatter (2009) and Aschwanden et al. (2012), where only the first paper is referenced in the manuscript. Beyond the numerical implementation of the NCZ method, it is not clear what this paper adds beyond Schoof and Hewitt (2016) in terms of physical understanding and the role of enthalpy in phase change.

3 Specific comments

1. I suggest replacing the title with: "An undated numerical method for solving the heat equation with phase change".
2. line 105: is θ_s defined anywhere?
3. line 115: the kink in the sfcc only matters if the authors take the derivative, which is not required! I am not sure about the value of this 'straw-man' argument about the three 'identical yet different' representations of the heat equation. First off, the language is unclear, so it is opaque as to what method the authors will actually use.
4. line 163: semi-implicit is not required, implicit is required. semi-implicit is a convenient method of mixing explicit and implicit methods to decrease time step restrictions.
5. line 169: ok, let me get this straight: the authors asked their colleagues if there is guaranteed convergence for nonlinear problems using the 'currently used algorithms' and they said no? Tell me more. Tell me why convergence is not guaranteed and how NCZ guarantees it – don't refer me to their paper. That is not the point. All the authors need to say is that NCZ offers advantages. Instead the authors generate an entire table showing that all of the methods they can think of have drawbacks, based on the word of their colleagues? The articulation of this argument needs substantial bolstering.
6. line 195: it looks like it comes down to the fact that the enthalpy is, for some reason, not monotonic with temperature, but isn't that the reason to use the enthalpy: because it is monotonic? I agree that at the melting temperature there is a jump in enthalpy governed by the latent heat, but does that mean that it is not monotonic?
7. line 202: I must be very confused, why don't you just solve for the enthalpy and use the jump conditions to determine the temperature (Schoof and Hewitt, 2016)?
8. section 4: I have no idea what the Neumann and Lunardini solutions are: describe the problems physically? I can certainly look in the appendix (and did) to find the mathematics, but until I saw Figure 2, I was totally confused at what problem you were trying to solve.
9. line 288: SUTRA uses an ϵ in the enthalpy function as well?
10. table 2: there does not seem to be monotonic convergence. given that this paper is claiming guaranteed convergence, I would have liked to see a convergence plot showing that the solution does converge at the power of the discretization, both in space and time. Also, it is worth mentioning the error order for both, especially since the method is first-order in time! predictor-corrector methods (or Heun's method) could be used instead of Crank-Nicholson to increase the resolution without the same time step restrictions.
11. figure 4: if the point is to show that the left and right panels are the same, then I suggest, plotting them on one panel and using the other panel to show the difference.

12. section 4.2: what defines the mushy zone in the Lunardini analytical solution? and how is this different than Katz (2008)?

13. line 329: is this a paragraph fragment?

14. Most figures: the axis labels as well as figure text are missing letters and difficult to read.

15. line 510: why is ϵ required? It seems that the value of the enthalpy is that there is a smooth transition across the phase change – adding ϵ negates the authors' claim that they are 'guaranteeing energy conservation', because they have added a fictious mushy zone.

References

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