REVIEW OF *ENTHALPY BENCHMARK EXPERIMENTS FOR NUMERICAL ICE SHEET MODELS* BY T. KLEINER AND OTHERS

Summary. This manuscript is built around a key sentence stated in the introducton:

> *Thus far we are lacking analytical solutions for thermo-mechanically coupled polythermal ice flow to test the enthalpy implementations in ice sheet models.*

Subject to disagreements about experimental design choices (below), this paper makes useful progress toward remedying that absence.

Two "experiments" (model setups) are given, with large parts of these experiments analytically solvable. The experiments are well-designed for testing two modeled phenomena: Experiment A addresses the time-dependence of basal melt rate (including freeze-on) if the ice above the ice is cold and not moving, while Experiment B addresses the location of the CTS in a column of downward-moving ice (constant vertical velocity) and with constant-in-time strain heating. Thus useful tools, and a small amount of analysis, are provided to designers of ice sheet models who would want to improve the handling of basal thermodynamics, or who want to model polythermal glaciers. On the other hand, major aspects of modeling the thermodynamics of glaciers are either not addressed by these experiments, or are, perhaps, prematurely tied to a simplified and incomplete theoretical basis.

The paper is well-written and does not waste journal space or reader time. Nonetheless I would hope for a round of revisions to address a longish list of small issues (below).

Major concerns and suggestions. The description of boundary conditions in this paper (pages 3211–3212) is far from fundamental because it is part of the "simplified theory" of section 4 of Aschwanden et al. (2012), denoted ABKB from now on. The current manuscript is not based on the more complete concepts of sections 2 and 3 of ABKB.

Thus, though I am an author of ABKB, and I know that it was written with best intent, when reading the current manuscript I felt that what the field of polythermal glacier modeling needs is more study of glacier thermodynamics, with awareness of what is and is not implementable, *not* the canonization of the details of ABKB into an intercomparison.

Specifically, the analytical solutions here may have the effect of "locking-in" the incompleteness or inconsistency of the ABKB simplified theory. For example, the function $E(T, \omega, p)$ in equation (1) depends on p, even though the claim is that the ice is incompressible and thus changing pressure cannot do work by changing volume. Also, formula (1) for $E(T, \omega, p)$ is not the only parameterization, as pointed out by ABKB, as one could take c_i to depend on T; ABKB illustrates this.

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For a further example, both Greve (1997a) and ABKB point out the need for nontrivial drainage modeling in complete polythermal models. This is not addressed in the current exercise; if drainage of water is nonzero on the temperate side of the CTS then equation (10) must be modified, depending on the form of the drainage relation.

Thus one would wish that a mostly-intercomparison approach to benchmarking was replaced with a both a more careful continuum analysis and numerical analysis of a single well-designed numerical model. Intercomparison of numerical models has inherent flaws:

- Intercomparisons are generally subject to "groupthink." Here the effect may be very strong, because apparently the three numerical models had their enthalpy implementations generated by a set of developers who both communicating about their model designs and who were, at the same time, building these "benchmark" experiments. Thus the spread of the results on these experiments has almost no meaning; it is comparable to the spread of opinions about an event from three witnesses who sat together, talked about what they saw, and chose the questions asked of the witnesses.
- Though analytical solutions are presented here, there is minimal attempt to report on grid refinement convergence rates of individual models, other than Figure 4. Apparently most quantities have first-order convergence (e.g. $O(\Delta z^1)$ as $\Delta z \to 0$), but this is not reported.
- It is virtually certain that, in the future, good-looking results on these very simple benchmarks will be used to justify assertions that model results for real systems (e.g. Greenland) are also good. (I.e. based on the history of EISMINT, ISMIP-HOM, and MISMIP.) However, effective application of enthalpy models to real systems *requires* model features untouched here, among them: upper surface enthalpy boundary conditions for melting, needed drainage model to avoid $\omega \gg 0.01$ in strain-heating temperate ice, and coupling to both sliding and subglacial hydrology to determine basal energy boundary condition.

So it may be too early in the development of polythermal energy conservation schemes for this exercise, but the exercise is already done. Better models will be developed, perhaps with compressibility of the mixture, or with physically-based drainage, and they will have to be severely-crippled to use these "benchmark experiments" and analytical solutions, which are based on earlier versions of the physics.

Finally, there is only one part of the Experiments which I believe needs correction or significant clarification. The issue is in Experiment B. Equation (14) says vertical velocity is constant, with value equal to the surface accumulation rate $a_s^\perp$; no problem. Each ice column is the same (i.e. there is no x-dependence of the velocity or any other quantity) so there is no divergence of the flow to balance the surface accumulation. Both geothermal flux and frictional heating are set to zero, so there is no basal melt rate by equation (7). But then conservation of mass says that there must a growing column height H (e.g. the bed descends at rate $a_s^\perp$), or the boundary condition should be adjusted to generate a matching basal melt rate of $a_s^\perp$, even at the initial time. What is going on? At very least the inconsistency should be clearly acknowledged! I think the test is still useful for numerical testing but it requires breaking models which conserve mass.

Detailed, line-by-line comments/suggestions. Several of these suggestions would improve the english usage.

- *page 3208, line 3*: The phrase "tests particularly" is slightly awkward. It can be said as just "tests", or "addresses". The word "particular" is not needed; it is clear.
- *page 3208, lines 8–9*: The sentence "Since ... experiments" should perhaps be moved to be the second sentence of the abstract?
- *page 3208, line 10*: If the abstract has two paragraphs I would suggest that the second one start with this sentence "We compare simulation ..."
- *page 3208, lines 13–16*: I had to read this sentence twice to recognize that the idea was very clear: the enthalpy gradient on the cold side of the CTS was influenced by the conductivity of temperate ice. My small suggestion to improve readability is to write "enthalpy gradient ON ~~at~~ the cold side of the CTS GOES TO ZERO ~~vanishes~~ in the limit of vanishing TEMPERATE-ICE conductivity, ~~in the temperate ice part~~ as required ...".
- *page 3209, line 11*: "to topology" → "to THE topology"
- *page 3209, line 12*: "CTS exist and" → "CTS EXISTS and"
- *page 3209, line 18*: Break run-on sentence: "... cold-ice scheme. A simplified ..."
- *page 3210, line 5*: I suggest this clarification: "Compared to thermodynamics USAGE, the "enthalpy" described ..."
- *page 3210, line 8*: "With" → "In"
- *page 3210, line 15*: "at pressure" → "at THE pressure"
- *page 3210, line 20*: The definition of symbols K_c, K_0 should be moved to here, before their use.
- *page 3210, line 20*: Because of later usage of the symbol "$\mathbf{q}_i$", the formula for the conductive flux should also be given here. That is,

$$\mathbf{q}_i = -\begin{cases} K_c \nabla E, & E < E_{pmp}, \\ k_i \nabla T_{pmp}(p) + K_0 \nabla E, & E \geq E_{pmp} \end{cases}$$

- *page 3210, line 21*: *In equation (2):* "$\mathbf{v}\nabla E$" → "$\mathbf{v} \cdot \nabla E$"
- *page 3210, line 22*: The transpose symbol here in describing $\mathbf{v}$ is unnecessary. The "dot" notation is used in the divergence already, and should be used in all inner products, in which case the "shape" of vectors is irrelevant, as it should be.
- *page 3211, line 5*: *General comment.* This section on boundary conditions should be more self-contained. As written it depends too much on the reader having Aschwanden et al. (2012) in-hand.
- *page 3211, line 6*: "corresponding to" → "from"
- *page 3211, line 8*: I suggest "The decision chart for the local basal conditions ..." → "At the ice base, the decision chart for local conditions ..."

- *page 3211, line 10*: Around here, symbols $T'(p)$, H_w, $\mathbf{n}_b$, q_{geo} should be explained, before their use. (I.e. move text here from top of page 3212.) For instance, a sentence could start "To describe these situations we define $T'(p) = \dots$". Very importantly, the direction and meaning of $\mathbf{n}_b$ should be clarified; is it upward or downward?
- *page 3211, line 15*: "layer with" → "layer, BUT with"
- *page 3211, line 18*: "@" → "at"
- *page 3212, lines 1–2*: The sentence "In this ... thickness" should be put before the equations which need this notation.
- *page 3212, line 2*: "In addition" → "NOTE THAT, in addition"
- *page 3212, lines 2–5*: The sentences "In addition to the temperate base condition, ... is only incorporated for numerical reasons" are not clear.
- *page 3212, lines 5–6*: I think the sentence "The type ... time dependent" is a general idea which should be moved to the start of subsection 2.2.
- *page 3212, line 9*: "obey" → "obeys"
- *page 3212, lines 13–*: As hinted in the manuscript, the jump conditions could be stated in more generality, and more correctly if the CTS is not horizontal, by using vector notation instead of coordinate derivatives. Perhaps (8) should be restated more generally using vectors? Then (10) could be unchanged, but with clarification that it comes from three assumptions: melting-CTS, no drainage, and horizontal-CTS.
- *page 3212, lines 22–24*: The sentence "The enthalpy scheme covers the case of melting conditions ..." misrepresents enthalpy models using a mixture theory, so I think it should be rewritten. There is no assumption in an enthalpy model that the conditions are melting at the CTS. It is perfectly legal for there to be a reduction of water content in flowing temperate ice, to generate cold ice, *if the way the heat flux is parameterized*—i.e. how heat flux is related to enthalpy gradient and pressure gradient—*allows it*. For example, freezing at the CTS can occur merely if ice thins enough to bring ice which is at the pressure-melting temperature back below that temperature, though of course this is not a steady-ice-geometry situation. Also, in the regularized theory with $K_0 > 0$, such a freezing CTS is hypothetically possible. Whether such a freezing CTS is physical, indeed how heat moves in temperate ice, is an open question, but it is not prohibited by an "enthalpy scheme".
- *page 3213, lines 9–*: The description of numerical codes in section 3.1 is missing basic information. It seems to me that for each model there should be a summary description with some basic facts: (i) an expansion of the acronym if appropriate (i.e. "ISSM"=what?), (ii) whether the code is open and if so how to get it, (iii) a citation to a model description or foundation paper, (iv) whether the code is a 3D or flow-line only model, and (v) whether the code allows changing ice geometry.

  Furthermore the description of the three models is written and structured differently in each case. Could the models be described in a consistent table so that a reader can see an apples-to-apples comparison?

- *page 3213, line 10*: What does "co-located" mean? (*If this is important enough, say which variables are which grid points. Otherwise drop it.*) What does "non-equidistant and regular grid in terrain-following ... coordinates" mean? (*If grid is finer near the base of the ice column then state that?*)
- *page 3214, line 5*: It is very unlikely that a linear variation model of conductivity "violates the basic requirement of a consistent heat flux" if merely changing to a more accurate interpolation scheme (i.e. harmonic means) is acceptable. Presumably all that is meant here is that because neighboring cells (grid spaces) have different sizes, it is more accurate to follow the advice in Patankar and use a harmonic mean.
- *page 3214, line 15*: If there is an "arising non-linear system" then either an implicit time-stepping scheme is in use, or steady-state equations are being solved. This should be said.
- *page 3214, line 17*: If no stabilization of the finite element advection scheme is used then this suggests a significant limitation of the experiments which should be more clearly stated? Indeed, Experiment A has zero ice velocity. Also the advection in Experiment B is too trivial in some sense (?), perhaps because the fine-grid vertical dimension sees only a constant vertical velocity (equation (14))?
- *page 3214, lines 18–19*: The sentence "The CTS is being tracked implicitly ..." is redundant (i.e. it has been said and it applies to all enthalpy methods). It is not needed in the ISSM description.
- *page 3215, line 4*: Presumably: "... nonlinear algebraic equations" → "... nonlinear algebraic equations AT EACH TIME STEP."
- *page 3215, lines 5–17*: This part of the description of COMice is confused. (My paper copy is full of question marks!)
  - *lines 6–7*: "operator evaluates the solution exactly at the circumcentre" suggests that "exact" and "solution" are relevant to this operator. Presumably what is actually meant is "operator INTERPOLATES A FUNCTION TO the circumcentre." That a local interpolant on a triangular element is computed "exactly" goes without saying!
  - *line 8*: "is not a local condition" is unlikely. (Does `circumcentre` require the entire triangular mesh of the whole ice sheet?) Of course there is geometric information in `circumcentre` from the whole triangle, but I am confident it is not mesh-global. This sentence can be removed without loss, I believe.
  - *lines 9–10*: I am guessing "the step of conductivity is located exactly on a mesh edge" means "the conductivity is constant on each triangle and discontinuous along edges". "Exactly" has nothing to do with it.
  - *lines 13–14*: Of course the "conductivity step does not match exactly with the true CTS position", because the method is numerical. Again, "exactly" has nothing to do with it. Presumably one can say that the errors in a piecewise-constant conductivity model is $O(h^1)$, where h is a scale for element size, but that also goes without saying.

  - *line 15*: The reference to Heaviside and smoothed Heaviside functions should either be fleshed-out, so as to be intelligible to the reader, or deleted. (Readers do not know enough to appreciate this cryptic reference to the model development history.)
- *page 3215, lines 20–21*: As observed in my Summary at the beginning, it is disappointing to have experiments built around a particular chart (Figure 5) in the specific "simplified" theory from ABKB, given the many physical process approximations which ABKB acknowledges have gone into that simplified theory. Was there a re-analysis of the decision chart? What should the chart be?
- *page 3216, lines 6–8*: The third sentence in this paragraph could be moved to be the second sentence, in which case the phrase "to guarantee $\dots\Psi = 0$" could be removed.
- *page 3216, line 11*: The combination "geothermal heat flux" is redundant, even if common. "Geothermal flux" suffices.
- *page 3216, line 16*: Usage: "running" $\to$ "run".
- *page 3216, lines 22–24*: It is worth noting that the model in ABKB includes drainage, and would cause a vertical velocity in the described column. This non-constant geometry effect of thermodynamics is worth exploring, and improving relative to the ABKB model, but this is untouched in the current very limited set of experiments.
- *page 3217, line 2*: Here "enthalpy transfer" means "enthalpy flux from advection"? I don't understand "transfer" in this context.
- *page 3217, lines 9–24*: Equations (12)–(15) should be stated in the physical coordinate z. Use of the scaled coordinate ζ should be limited to Appendix A2.
- *page 3217, line 11*: As noted in the above "Major concerns" part, it would seem that mass is not conserved because there is no basal melt rate to match the surface accumulation rate, given the constant vertical velocity.
- *page 3217, line 12*: Again: "geothermal ~~heat~~ flux".
- *page 3217, line 14*: The form (i.e. z-dependence) of the strain heating Ψ is the major, and the only non-constant, input in this experiment. (*For example, the horizontal velocity is x-independent and the domain is periodic, so there is no significance to the horizontal advection.*) Thus at this point a formula for $\Psi(z)$ should be given, and perhaps a figure which shows it. How concentrated near the base is the strain-heating source?
- *page 3217, line 17–18*: The phrase about "monotony" is at least not standard usage. Perhaps: "The CTS in this experiment is uniquely determined BECAUSE THE VERTICAL VELOCITY IS DOWNWARD ~~due to the monotony of the vertical velocity profile~~." (*I do not believe that monotonicity of the vertical velocity is needed for uniqueness, though presumably downwardness (negativity) of the vertical velocity is needed.*)

- *page 3217, lines 20–24*: The value of T_s in equations (15) and (16) is not clear here. Is "$T_s/2$" in (16) in Kelvin or Celsius? Please just state the numerical values if they are fixed in Experiment B.
- *page 3218, lines 3–9*: There are two different ideas in this paragraph. (*I.e. (1) that latent heat transport is uncertain and (2) that the experiments test multiple CR and* Δz *values.*) This paragraph should be split in two and put before the detailed descriptions of the experiments and results.
- *page 3218, lines 10–14*: Again this general description of experimental design should go before the detailed description of the experiments. That is, the reader should know the scope of the experiments before the details.
- *page 3218, line 21*: In this sentence, "asymptotically reached" actually means "not reached." What is the magnitude of the difference from steady state?
- *page 3220, line 11*: "comparable time step" $\to$ "comparable modeled time"
- *page 3220, line 17*: By my understanding of the calculation in A2, and looking at Figure 3, the exact location of the CTS is known in Experiment B, and is approximately 19 m above the bed. So the analytical value for this location should be given here or in some other prominent place.
- *page 3220, line 18*: The fact that ISSM is designed around a steady state solver (for enthalpy, at least) should be stated in describing ISSM in subsection 3.2.
- *page 3221, line 8*: "differ from" $\to$ "differ noticably from" (or similar)
- *page 3221, lines 16–23*: Largish, irregular differences in ISSM, on coarse grids, are far more likely to come from an un-stabilized advection scheme than from the explanation given, namely, the interpolation scheme for conductivity across the CTS. Oscillations from inadequately-resolved steady advection would go away under refinement as mesh Peclet (relative to vertical velocity) improves, exactly as seen.

  Of course the CTS does not coincide with a node, and it never will under arbitrary mesh refinement. If the explanation in this paragraph were correct, the oscillations seen would remain on all grids.
- *page 3222, line 25*: "the question, if" $\to$ "whether"
- *page 3223, line 9*: Dimensional derivative "$\partial E/\partial z$" should be used here instead of "$\partial E/\partial \zeta$". In fact, use of ζ should be confined to Appendix A2.
- *page 3223, lines 9–10*: The word "violates" makes no sense here: "and violates the condition of Eq. (10) (non-continuous)" $\to$ "and satisfies Eq. (10), giving an appropriate jump in $\partial E/\partial z$."
- *page 3223, line 14*: "filed" $\to$ "field"
- *page 3223, line 20*: "than is" $\to$ "then it is" (perhaps?)
- *page 3223, lines 21–23*: I see this effect in Figure 3, but I do not see why it should be so. If K_0 is large, and assuming the strain heating rate is not significantly affected by K_0, then it seems to me that the latent heat in the temperate layer can (perhaps nonphysically) flow into the colder ice above, thus lowering the CTS. *Why* should the CTS instead become lower as K_0 approaches zero?

- *page 3224, line 5*: "Therefore the enthalpy scheme allows to convert" → "Therefore AN enthalpy scheme allows ONE to convert"
- *page 3224, lines 6–7*: Again I am wondering why enthalpy schemes are assumed to be incapable of modeling freezing conditions at the CTS ...
- *page 3224, line 7*: "ubiquitous" → "exclusively"?
- *page 3224, lines 17–19*: This reviewer completely agrees with the plea in this sentence!
- *page 3224, line 18*: "is leveled out" → "is balanced by", perhaps?
- *page 3224, line 21*: "The proposed ~~numerical~~ experiments ..."
- *page 3225, line 3*: "a clean demand for" → "a clear need for", I think
- *page 3225, line 7*: It should be stated more clearly in this section that there is an analytical solution starting with an initial state given by (A3), but that (A3) is only the approximate state of the numerical simulation at the end of phase II. "Is only valid for phase (IIIa)" is simply inaccurate if anyone is carefully following the argument.
- *page 3227, lines 3–4*: Move formula (A12) before the first use of "$T_{eq}(z)$", which is in equation (A5).
- *page 3228, line 16*: "general" → "particular", I think