We thank the two reviewers very much for their comments, and discuss them below. In case misprints etc. were concerned, we shortened the answer to "Done.", which then indicates that we have changed it in the text exactly as recommended. We paraphrase the request or comment below (blue) with the reply (black).
We have some minor corrections to the text not related to the reviewer comments

- page 3212, equation (7): $\mathbf{n} \rightarrow \mathbf{n}_{\mathrm{b}}$ and "upward" deleted in line 11,
- page 3227: $T_{\mathrm{w,\,c}}$ must be $T_{\mathrm{s,\,c}}$ in equation (A12),

and would like to acknowledge the work of H. Blatter and R. Greve on a new enthalpy scheme (Blatter and Greve, 2014) in the discussion part of the manuscript.

Reviewer #1 (Anonymous):

RC: p. 3211, line 18: please do not use @ (although it may become popular in SMS and other short communications)
AC: Done.

RC: p. 3213, lines 10-11: what do you mean with "co-located"? I guess that non-equidistant and regular grid in terrain following coordinates means "non-equidistant" in the original vertical coordinates and regular (equidistant) in the vertical terrain-following coordinates?
AC: The term "co-located" grid is widely used in the literature on computational fluid dynamics. In contrast to the "staggered" grid, all quantities (e.g. velocity and enthalpy) are located at the same grid nodes. The implemented discretisation scheme allows "non-equidistant" grid lines in all three dimensions (not used here). The term "regular" grid is used in the literature characterise grids, that have a constant number of grid points per grid line. Finite element methods often use unstructured (or irregular) grids. See e.g. chapter 8.1 "The Choice of Grid" in Ferziger and Perić (2002).

We think the details on the chosen grid are not relevant for this study. Thus we will change the sentence to "The relevant equations are discretised using finite-differences in terrain-following (sigma) coordinates.".

Reviewer #2 (Ed Bueler):

Major concerns and suggestions

RC: 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.
AC: We agree that glaciology needs nore understanding of glacier thermodynamics and that this topic is important. We would be delighted to work on this together with the reviewer in the future! However, while developing an andvanced and fundamental approach, the ice modelling world will continue to turn and with that the simplified approach. As we became aware of the challenges the simplified approach poses for the implementation, we are still convinced that benchmarks enable a consistency check of models, that are required for prognostic studies for the contribution of ice sheets to sea level rise. The thermal regime in the current models is far from being well developed and even the simplified theory of ABKB is an advancement for current models - not to forget that some models still do not deal with the wet-cold-base condition.

Thus thinking that benchmarks are leading to a locking-in is too pessimistic, it is a first step in a long way of developing a mature description of enthalpy. In parallel to this the lab-experiment community needs to take up the challenge to perform measurements of the enthalpy conductivity in temperate ice, as even if we come up in future with a thorough theoretical description and a robust model implementation, a K_0 remains to be required with an adequate accuracy.

RC: 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.
AC: We see this in connection to the previous point. It is correct that drainage modelling is nontrivial and that Eq. (10) needs to be modified if one includes it, but this study does not focus on different kinds of drainage description and their implementation, but different kind of boundary condition and their discretisation and implementation. Using different models that all stick to one kind of drainage description or neglecting drainage at all, still allows a comparison of their ability to represent the different boundary conditions.

We see the reviewers wish that we should have written a manuscript on another topic. However, we would want to defend this manuscript and the exercise we present here. As these kind of exercises were neither published nor treated by other initiatives so far, we think we provide a valuable contribution for the trend to switch from temperature to enthalpy evolution modelling. Nevertheless, we can understand the reviewers wish very well and will continue our future research in this direction.

RC: 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.

AC: We agree with the reviewer, that model intercomparison studies have inherent flaws and their results must be taken with caution. Model or code comparisons do not provide substantive evidence that software is functioning correctly (verification).

- The numerical experiments proposed here are not meant as "model intercomparison" as in e.g. EISMINT, ISMIP-HOM, and MISMIP. We compare model results with analytical solutions. The reviewer has no arguments against the analytical solutions. Thus, "groupthink" is not a point. Although we compare model results with analytical solutions we avoid the term model "verification" and call the proposed experiments "benchmarks". We follow the arguments of Oreskes et al. (1994) that numerical models which are based on incomplete and scale dependent data are never closed systems and can therefore neither be verified nor validated. This is especially true here, since the proposed experiments are based on simplified problems.

- The observed order of accuracy measured as the root-mean-square deviation (RMSD) to the analytical solution has been obtained in the series of vertical mesh refinements from $\Delta z = 10\,\mathrm{m}$ to $0.5\,\mathrm{m}$ and is shown in Figure 1. The models TIM-FD^3 and ISSM show approximately first-order convergence ($O(\Delta z^1)$) as $\Delta z \to 0$, while in COMice the RMSD drops only for Δz below $2\,\mathrm{m}$. The finite difference discretisation scheme in TIMFD^3 is formally second-order accurate in space (and time) and the finite element models ISSM and COMice use linear basis functions, thus one would expect second-order convergence ($O(\Delta z^2)$) as $\Delta z \to 0$ for smooth problems. But this is not the case here, since the observed order of accuracy depends on the strength of discontinuities (conductivity ratio between cold and temperate ice) and on the CTS implementation details.

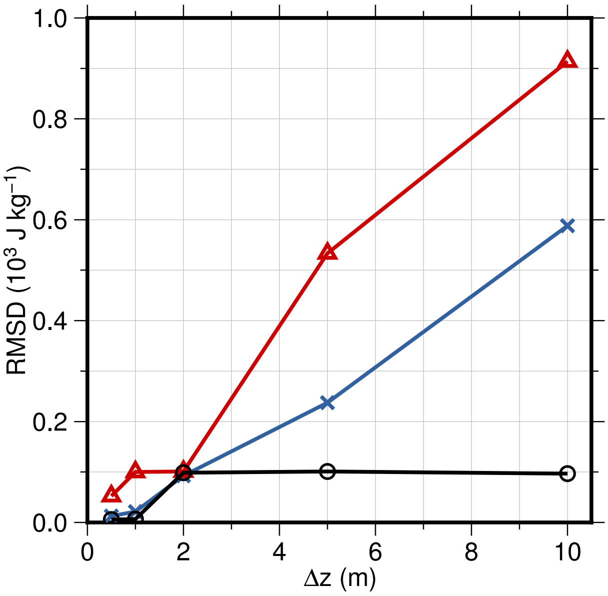

Figure 1: The root-mean-square deviation (RMSD) of the model results to the analytical solution (Experiment B) for different vertical grid resolutions Δz. Model results for TIM-FD^3 (blue crosses), ISSM (red triangles) and COMice (black circles) are obtained for the lowest conductivity ratio $CR = K_0/K_c = 10^{-5}$ applied to all models.

We could include this text and the figure in the manuscript at the end of section 5.2. or just state the first-order convergence (Editors choice).

- We can't say how these experiments will be used in the future and if they are used "to justify assertions that model results for real systems (e.g. Greenland) are also good". As mentioned by the reviewer the real system is far more complex as the proposed experiments, but as far as we know, an analytical solution for the enthalpy including ice flow and subglacial hydrology does not exist. Thus, testing the implementations of the enthalpy alone is the first logical step. We think, the point that our "set of developers who both communicating about their model designs" is an advantage, as we all implement the same "physics" limited only by the specific model frameworks.

We think it is not "too early in the development of polythermal energy conservation schemes for this exercise", but too late since continental scale simulations for the Greenland Ice Sheet that apply the enthalpy formulation are already published (Seroussi et al., 2013; Aschwanden et al., 2012) without any reported type of code verification.

RC: 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 ; 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), or the boundary condition should be adjusted to generate a matching basal melt rate of a_s , 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.
AC: The basic concept of the proposed experiments is that we ignore all other influences (stress-balance coupling, ice thickness evolution, drainage) on the numerical enthalpy scheme. We agree with the reviewer and will point out the inconsistency between the prescribed vertical velocity and the constant ice thickness in the text directly after equation (14), as: “Note that this set-up is not mass conservative. There is no process considered that balances the accumulation rate required for a constant ice thickness. But for simplicity we do not account for ice thickness evolution.”

Detailed, line-by-line comments/suggestions

RC: 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.
AC: We will use “tests” instead of “tests particularly”.

RC: page 3208, lines 8–9: The sentence “Since. . . experiments” should perhaps be moved to be the second sentence of the abstract?
AC: Done.

RC: page 3208, line 10: If the abstract has two paragraphs I would suggest that the second one start with this sentence “We compare simulation. . . “
AC: Done.

RC: 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. . . “.
AC: Done.

RC: page 3209, line 11: “to topology” → “to THE topology”
AC: Done.

RC: page 3209, line 12: “CTS exist and” → “CTS EXISTS and”
AC: Done.

RC: page 3209, line 18: Break run-on sentence: “. . . cold-ice scheme. A simplified. . . "
AC: Done.

RC: page 3210, line 5: I suggest this clarification: “Compared to thermodynamics USAGE, the “enthalpy” described . . . “
AC: Done.

RC: page 3210, line 8: “With" → “In"
AC: Done.

RC: page 3210, line 15: "at pressure" → "at THE pressure"
AC: Done.

RC: page 3210, line 20: The definition of symbols K_c, K_0 should be moved to here, before their use.
AC: We will restructure the text around equation (2) accordingly.

RC: Because of later usage of the symbol "$\mathbf{q}_i$", the formula for the conductive flux should also be given here.
AC: Now as equation (3). Equation (2) now uses $\mathbf{q}_\mathrm{i}$

RC: page 3210, line 21 : In equation (2): "$\mathbf{v}\nabla E$" → "$\mathbf{v}\cdot\nabla E$"
AC: Done.

RC: 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.
AC: Done.

RC: 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.
AC: We tried to keep this part of the text as short as possible because the derivation of the boundary conditions (BC) themselves is not the subject of this work and discussed in detail in the given reference (Aschwanden et al., 2012). We could expand the section of BCs in case the editor wants us to do so.

RC: page 3211, line 6: "corresponding to" → "from"
AC: Done.

RC: page 3211, line 8: I suggest "The decision chart for the local basal conditions …" → "At the ice base, the decision chart for local conditions …"
AC: Done.

RC: 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?
AC: Done.

RC: page 3211, line 15: "layer with" → "layer, BUT with"
AC: Done.

RC: page 3211, line 18: "@" → "at"

AC: Done. See also reviewer #1.

RC: page 3212, lines 1–2: The sentence "In this ... thickness" should be put before the equations which need this notation.
AC: Done. See also RC(page 3211, line 10).

RC: page 3212, line 2: "In addition" → "NOTE THAT, in addition"
AC: Done.

RC: page 3212, lines 2–5: The sentences "In addition to the temperate base condition, ... is only incorporated for numerical reasons" are not clear.
AC: We will change "$\nabla T' \cdot \mathbf{n}_\mathrm{b} \geq \beta/K_\mathrm{c}$" to "$\nabla T' \cdot \mathbf{n}_\mathrm{b} = \beta/K_\mathrm{c}$" and remove the sentence "In both conditions the "greater than" case is only incorporated for numerical reasons."

RC: 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.
AC: Done. We will rewrite the first paragraph in subsection 2.2 accordingly.

RC: page 3212, line 9: "obey" → "obeys"
AC: Done.

RC: 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.
AC: We will rewrite this part using vector notation in Eq. (8). We will acknowledge the melting CTS and horizontal CTS assumptions for Eq. (10). Drainage does not play a role as ice has no moisture at both sides of the CTS in case of melting conditions.

RC: 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".
AC: We agree with the reviewer. The enthalpy method in general does not prohibit freezing conditions at the CTS, but the basal boundary conditions used in Aschwanden et al. (2012) do. For a temperate layer at the base equation (5) holds at the base and does not allow for an enthalpy gradient grater than zero as required for freezing conditions at the CTS. Further, the numerical models applied here do not allow a discontinuous (enthalpy) solution. We will

change this paragraph accordingly. See also comment above.

RC: 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?
AC: We have added the missing information to the description of the models. We could provide an extra table in case the editor wants us to do so.

RC: 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?)
AC: See our comment to Reviewer # 1 (similar question). In principle TIM-FD^3 allows grid refinement in all directions. In applications based on gridded data-sets only grid refinement at the base is used. For model comparison purpose all three models apply the same vertically equidistant layers (page 3218, line 9).

RC: 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.
AC: We may have used the therm “consistent” in a misleading way. The last part of the paragraph now reads as: “...However, this approach cannot handle the abrupt changes of conductivity at the CTS. We use the harmonic mean of the conductivities, as suggested by Patankar (1980, chap. 4.2.3) not only at the CTS but for all conductivities evaluated between grid nodes.” The use of the harmonic mean has nothing to do with the size of neighbouring cells. The distance to the position where the conductivity is required between grid nodes needs to be accounted in the harmonic and arithmetic mean approach for the discretisation scheme.

RC: 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.
AC: Done.

RC: 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))?
AC: In Greve’s analytical solution for this experiment no artificial diffusion has been used. During numerical simulations it showed that no artificial diffusion is needed for stable results

either. Thus, for best comparison of the numerical scheme with the analytical solution, no stabilization has been used in ISSM here. This, of course, implies by no means that in other velocity regimes no stabilization of the advection scheme would be necessary.

We will replace "For the presented simulations no numerical stabilization has been used." with "For best comparison to the respective analytical solutions no numerical stabilization has been used here.".

RC: 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.
AC: Done.

RC: page 3215, line 4: Presumably: "...nonlinear algebraic equations" → "...nonlinear algebraic equations AT EACH TIME STEP."
AC: Done.

RC: page 3215, lines 5–17: This part of the description of COMice is confused. (My paper copy is full of question marks!)
AC: We will change the description of the COMice model in order to clarify the points below.

RC: page 3215, 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!
AC: We will drop "exactly" here as it is misleading. The circumcenter is well defined for triangles or tetrahedra. For other mesh elements Comsol provides a natural generalisation.

The sentence will be rewritten as: "The operator interpolates the enthalpy solution to the circumcentre of the mesh element to which the point belongs."

RC: page 3215, 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.
AC: What we meant here is, that the expression `circumcenter(E)` is constant on each triangle.

The sentence will be replaced by: "In doing so, `circumcenter(E)` is constant on each triangle and discontinuous along the edges."

RC: page 3215, 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.
AC: We agree with the reviewer. The sentence will be replaced with: "Therefore the conductivity jump is located on a mesh edge."

RC: page 3215, 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(h1), where h is a scale for element size, but that also goes without saying.
AC: We will remove this sentence.

RC: page 3215, 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.)
AC: We would like to mention the Heaviside and the smoothed Heaviside function as they appear as the most obvious approaches to implement the contrast in conductivity. The smoothed Heaviside function has been used in the Comsol implementation of the enthalpy scheme in Aschwanden and Blatter (2009). We will add the reference to Aschwanden and Blatter (2009).

RC: 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?
AC: See our comment to the first point in the "Major concerns ..." part.

RC: 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 ... $\Psi = 0$" could be removed.
AC: Done.

RC: page 3216, line 11 : The combination "geothermal heat flux" is redundant, even if common. "Geothermal flux" suffices.
AC: Done.

RC: page 3216, line 16 : Usage: "running" → "run".
AC: Done.

RC: 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.
AC: We agree, for a better representation of glacier thermodynamics in numerical models, drainage is important. But, at the moment a commonly accepted physical model based on observations does not exist.

RC: page 3217, line 2 : Here "enthalpy transfer" means "enthalpy flux from advection"? I don't understand "transfer" in this context.
AC: We will change this sentence to "To test the numerical solution for enthalpy in a vertical ice column with ice advection, we apply the "parallel-sided polythermal slab" set-up as given in e.g. Greve and Blatter (2009)." to clarify this point.

RC: page 3217, lines 9–24 : Equations (12)–(15) should be stated in the physical coordi- nate z. Use of the scaled coordinate ζ should be limited to Appendix A2.

AC: Done. We will move the variable transform to the appendix.

RC: 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.
AC: See our comment of the last point in the "Major concerns" part.

RC: page 3217, line 12: Again: "geothermal heat flux".
AC: Done.

RC: 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 Ψ should be given, and perhaps a figure which shows it. How concentrated near the base is the strain-heating source?
AC: We will provide the equations for Ψ, the viscosity and the effective strain rate.

RC: 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.)
AC: We agree with the reviewer and will change the text according to his recommendation.

RC: 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.
AC: The numerical values are now given in the text and equation (15) and (16) have been removed as well as the reference to equation (15) above equation (A22).

RC: 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.
AC: We will acknowledge the general scope of the experiment before going into the detailed description.

RC: 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.
AC: See comment above.

RC: page 3218, line 21: In this sentence, "asymptotically reached" actually means "not reached." What is the magnitude of the difference from steady state?
AC: We agree with the reviewer. The usage of "asymptotically reached" is misleading in this context. Although the steady states are reached asymptotically in theory, in numerical

modelling the steady states are reached within a given tolerance. We will therefore remove the word "asymptotically" at any location in the results chapter and report the magnitude of difference to the steady state for the quantity discussed in the text.

RC: page 3220, line 11: "comparable time step" → "comparable modeled time"
AC: Will be changed to "comparable modelled time".

RC: 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.
AC: We will state the location of the CTS in section 4.2.

RC: 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.
AC: Done.

RC: page 3221, line 8: "differ from" → "differ noticably from" (or similar)
AC: Will be changed to "differ noticeably from".

RC: 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.
AC: The authors would like to argue why the suggestion made by the reviewer does not invalidate the point made in the paper. Note two observations. First, the differences in ISSM of numerical and analytical solution are not irregular, but follow a distinct pattern: The cold part of the ice column is too cold, the temperate part is too warm. Second, the harmonic mean to average the heat conductivities in the CTS-layer (element layer in which the CTS lies) strongly favours the lower conductivity K_0 for small ratios $CR = K_0/K_{\mathrm{c}}$. Assuming one would add a stabilisation to the advection scheme in form of artificial diffusion. Then overall heat diffusion along the ice column would increase. Especially the formerly quasi isolating temperate ice part would now conduct much more heat. Therefor, heat flux across the CTS increases and the jump in the enthalpy gradient at the CTS decreases. Thus, adding stabilisation would help mitigate the problem of the deviation to the analytical solution at the CTS, but the overall deviation would increase due to increased heat flux.

The authors agree with the reviewer, that this section needs additional clarification, and would like to replace the paragraph "For ISSM, those differences . . . positive one in the temperate ice column." with the following text: "As the method chosen for interpolating heat conductivities strongly favours the lower value, a quasi isolating layer thicker than in the analytical solution is artificially created. Thus, heat flux into the upper cold ice column decreases, and that column cools. Vice versa, excess heat is accumulated in the lower temperate ice column, such that this part of the ice column heats up. The result is the described negative temperature and positive water fraction offset. It scales with vertical mesh resolution, but stays detectable even on the highest mesh resolution tested here."

RC: page 3222, line 25: "the question, if" → "whether"
AC: Done.

RC: page 3223, line 9: Dimensional derivative "$\frac{\partial E}{\partial z}$" should be used here instead of "$\frac{\partial E}{\partial \zeta}$". In fact, use of ζ should be confined to Appendix A2.
AC: Done.

RC: page 3223, lines 9–10: The word "violates" makes no sense here: "and violates the condition of Eq. (10) (non-continuous)" ′ → "and satisfies Eq. (10), giving an appropriate jump in $\frac{\partial E}{\partial z}$."
AC: The harmonic mean approach of Patankar (1980, chap. 4.2.3) for the interface conductivity in TIM-FD^3 and ISSM leads to a continuous heat flux at the interface and violates the condition of Eq. (10) (non-continuous). But the harmonic mean strongly favours the lower conductivity K_0 for small ratios $CR = K_0/K_c$ and this leads to the apparent jump in $\frac{\partial E}{\partial z}$.
We will state this in the text.

RC: page 3223, line 14: "filed" → "field"
AC: Done.

RC: page 3223, line 20: "than is" → "then it is" (perhaps?)
AC: Yes.

RC: 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?
AC: With increasing K_0 the enthalpy gradient in the temperate ice layer decreases and the heat flux into the upper (cold) ice column increases. This leads to a warmer cold ice part and thus a thicker temperate ice layer.

RC: page 3224, line 5: "Therefore the enthalpy scheme allows to convert" → "Therefore AN enthalpy scheme allows ONE to convert"
AC: Done.

RC: page 3224, lines 6–7: Again I am wondering why enthalpy schemes are assumed to be incapable of modeling freezing conditions at the CTS ...
AC: See AC to page 3212, lines 22–24.

RC: page 3224, line 7: "ubiquitous" → "exclusively"?
AC: We will use "exclusive".

RC: page 3224, lines 17–19: This reviewer completely agrees with the plea in this sentence!
AC:

RC: page 3224, line 18: "is leveled out" → "is balanced by", perhaps?
AC: We think, "balanced out" sounds to positive in this context. We will replace the sentence with: "The advantage of deriving the water content by solving numerically for the enthalpy is limited by the use of a flow rate factor with a restricted validity range."

RC: page 3224, line 21 : "The proposed ~~numerical~~ experiments . . . "
AC: We perform numerical experiments (or simulations) compared to laboratory experiments. Therefore we want the keep the word "numerical".

RC: page 3225, line 3: "a clean demand for" → "a clear need for", I think
AC: Done.

RC: 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.
AC: We will replace the first sentence with: "To derive the basal melt rate for phase (IIIa) of Experiment A it is assumed that the temperature is in steady-state at the end of the warming phase (II)."

RC: page 3227, lines 3–4: Move formula (A12) before the first use of "$T_{eq}(z)$", which is in equation (A5).
AC: Done. Minor text corrections will be done in addition.

RC: page 3228, line 16: "general" → "particular", I think
AC: Done.

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