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## ***Interactive comment on “Numerical mass conservation issues in shallow ice models of mountain glaciers: the use of flux limiters and a benchmark” by A. H. Jarosch et al.***

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This is a valuable paper making an important point about widely-used numerical choices for modeling glaciers and ice sheets. It can and should be published essentially in its current form, after addressing four modest, and partly historical, issues, along with some typos which are listed below.

Summary of the paper: The announced motivation for this work is to make numerical schemes for the shallow ice approximation (SIA) suitable for simulations of large areas covered by mountain glaciers. (A more basic motivation, namely to make the schemes more mass conserving, as an abstract property, would also suffice!) The authors start

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by describing the projection step used to enforce positivity in most schemes. There is an insightful comment on a more self-consistent constrained minimization approach. Then existing finite difference schemes are summarized; see issues below. The mass conservation failure of these schemes on steep bedrock is explained. Then two new explicit schemes, differing only by their flux-limiter, are proposed. They adapt flux-limiting ideas from the numerical hyperbolic literature to the current situation. So as to verify the new schemes, steady state exact solutions on non-flat bedrock in one dimension are discussed and a particular solution with a bedrock step is constructed. The mass conservation performance of the new schemes is demonstrated to be hugely better than existing schemes on the bedrock step exact solution. The behavior of the schemes is also analyzed on a flat bed solution ("Bueler C"). This distinguishes between the two flux-limiters and suggests that the "superbee" flux-limiter should be chosen.

Four most significant issues:

1. The description of "type II" schemes used in EISMINT I (= Huybrechts Payne 1996) is simply wrong. Of course the paper does not depend much on what is "type II" anyway. This issue will be fixed if the authors read, and presumably cite, the easy paper

R. C. A. Hindmarsh and A. J. Payne, 1996. Time-step limits for stable solutions of the ice-sheet equation. *Ann. Glaciol.* 23, 74–85

They will see that "type I" and "type II" from EISMINT I are "method 2" and "method 3" from Hindmarsh Payne. They will see that type II is a much bigger change from the type I scheme that they (correctly) state in equation (16). They will see that Hindmarsh Payne consider a "method 1" which seems not to be widely-known but is a good idea. Then they can coherently report what they did or update their results according to taste.

2. This is somewhat related to the previous point, but more significant: The "Mahaffy" label used in this paper, including in the section 3 heading, is not a good term for iden-

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tifying the scheme flaw which gets fixed in this paper, namely the mass conservation failure of projection when the bed is not flat.

First, the description on pages 4041 and 4042 is reasonably clear for describing the general flavor of finite difference schemes used for the SIA, including Mahaffy's in broad outline. But equation (6) is wrong as stated for Mahaffy's scheme! (Mahaffy (1976) should be read as well as cited!) Her scheme \*does not\* lag the diffusivity as stated. Indeed, she makes a credible though unproven claim that her scheme is  $O(\Delta t^2 + \Delta x^2 + \Delta y^2)$  (i.e. as truncation error away from the margin), a claim which is false for two-level schemes which lag the diffusivity in the manner given in (6).

More broadly, the Mahaffy (1976) work is naturally identified with three choices for finite difference schemes for the SIA: (i) implicitness by ADI (i.e. Crank-Nicolson in one dimension at a time), (ii) a particular style for evaluating the magnitude of the surface gradient on the staggered grid (i.e. "method 2" in Hindmarsh Payne), and (iii) an ad hoc projection step like (7) which is not even commented-on by Mahaffy herself (but we can presume she enforced positive thickness).

The paper under review essentially ignores or is unaware of Mahaffy choice (i), because only an explicit scheme is tested. (I have no complaint about this other than that the "type I" scheme they use is not "Mahaffy" so she should not get the blame!) This paper fully adopts Mahaffy choice (ii), Mahaffy's surface-gradient-evaluation-for-diffusivity method. Note that the works Bueler et al (2005; 2007) identify the label "Mahaffy" with concept (ii); their are words like "the surface gradient ... is computed on a staggered grid by the Mahaffy (1976) scheme" and such.

In conclusion, I suggest using a term like "naive projection", or "lagged-diffusivity projection (LDP) scheme", or whatever, to identify the basic error in existing SIA schemes, which is to apply projection (7) without flux-limiting (flux modifications)! Don't just say "Mahaffy" and hope that is clear!

3. The current article is a \*desirable\* perversion of the standard application of flux-

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limiting schemes! However, because the goal is not second order accuracy at margins (which will not be achieved), and because the total-variation-diminishing (TVD) property is not proven here nor even an obvious goal, the nonstandardness of the flux limiter idea needs to be acknowledged so we don't raise another generation of confused numerical ice sheet modelers.

Indeed, the less-conserving methods that are being replaced may have the same TVD property that the current scheme may have; we are talking about diffusive PDEs here anyway! (Also I note that van Leer's 2006 review article "Upwind and High-Resolution Methods ..." points out that flux-limiting schemes for hyperbolic equations don't limit fluxes anyway, whereas the current paper essentially does! But let's not worry about \*that\* water under the bridge.)

4. It took me a while to realize that I had not thought of the flux factorization assumed here. To explain, the ice sheet literature has two factorizations of flux:

$$\mathbf{q} = \mathbf{v}h$$

where  $h$  is the ice thickness and  $\mathbf{v}$  is the vertically-integrated horizontal velocity, and

$$\mathbf{q} = -D\nabla s$$

where  $D$  is the diffusivity and  $s$  is the surface elevation. When applying (perverting) their flux-limiter the current authors are essentially proposing a third factorization,

$$\mathbf{q} = \omega h^{n+2}$$

where I've made up a new symbol

$$\omega = -\frac{2A(\rho g)^n}{n+2} |\nabla s|^{n-1} \nabla s$$

Thus we may think of the Glen law SIA mass continuity equation as vaguely like a Burger's equation:

$$h_t + \nabla \cdot (\omega h^{n+2}) = \dot{m}$$

versus

$$u_t + (u^2)_x = 0$$

But maybe this should be stated more clearly if this is the way the authors think about mass continuity? (This is a complement hidden as a criticism.)

Line-by-line comments/tipos/issues:

page 4037: The title is too long and boring. Perhaps "Restoring mass conservation to shallow models of mountain glaciers"?

page 4038, lines 3–4: This run-on sentence "... and their capability ... worldwide." adds little. How about "... for computational efficiency so as to allow broad coverage."

lines 22–23: Final phrase "... and that they will contribute substantially to sea level rise in the coming century ..." is both speculative and leaves "substantially" in the eye of the beholder. Necessary given methodological character of the paper?

page 4039, line 2: Where does " $O(10^7)$ " come from? Is it necessary to say much more than "a lot"? Do you mean "a grid of X km resolution is necessary" for these century-long model runs? I don't see a standard by which we can specify the minimum necessary resolution for any (significant) purpose, at least at the present state of understanding.

line 9: This reference to Fastook and Chapman (1989) seems obscure. Early papers by Mahaffy (1976) and Oerlemans (1981) already contain the vertically-integrated shallow ice numerical ideas. The regular grid finite element methods of Fastook have the same flaws as the regular grid finite difference methods addressed in the current work.

line 16: "rarely" is not true. The bedrock/cliff issue addressed in the current paper is almost-certainly worst in East Greenland of all glaciated areas in the world. Ice sheet people should care just as much as mountain glacier people. The fact that EISMINT and Bueler have been obsessed with flat bed ice sheets is their fault, not reality.

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line 19: I would hyphenate "second-order" and "flux-limiting" because "second" modifies "order" and "flux" modifies "limiting".

page 4040, eqn (1): The factor  $2A(\rho g)^n/(n+2)$  is so common that I would suggest writing "let  $\Gamma = 2A(\rho g)^n/(n+2)$  at this point and so on.

line 11: Perhaps break run-on and remove implication that non-shallow is easier: "... where there is ice ( $h > 0$ ). Ice geometry evolution models are ..."

line 18: New, shorter sentence here, perhaps: "Negative ice thicknesses are never realized. In addition ..."

page 4041, line 6: Perhaps break and simplify: "Our aim here is more practical. We address shortcomings ..."

lines 12–13: As noted above, by my reading the Mahaffy method is not (6).

line 14: No "roughly" needed: "These methods update ..."

around page 4042: You might point out that the methods you actually propose and implement are explicit not implicit! I was confused about this on first reading because (8) and (9) are right-on.

page 4042, line 1: This should be the start of a new paragraph. Fix "inequality". The scheme described by (8) and (9) is a great idea which has never been so clearly stated in the literature. I wish it had been evaluated in this paper. With a flux-limiter, naturally.

page 4044: Equations (12) and (13) could be put on one line with a single equation number. I was confused at one point whether a reference to (13) was exclusive of (12), but it was not.

eqn (15): Typo. The average thickness is  $h_{k,l}^i + h_{k+1,l}^i$  with a "+".

page 4045: As noted earlier, eqn (16) is not the "type II" used in EISMINT I.

eqn (16): Typo again. The average needs a plus.

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lines 4, 11, 13: As noted, these "Mahaffy" labels cause confusion. Or maybe: These labels cause the current review to say "Her contributions are being warped out of recognition!"

page 4046, eqn (17): Typo, I believe. Probably the criterion is

$$(s_{k,l}^i - s_{k+1,l}^i)(h_{k,l}^i - h_{k+1,l}^i) < 0$$

with  $k + 1$  replacing  $k$  in the fourth instance.

page 4047, lines 24–6 (not numbering oddity here and elsewhere from TCD format): I had to read this text a couple of times to understand. I believe it can be shortened, simplified, and clarified.

line 24: Perhaps remove "A small amount of". Just "Ice mass is ..." is fine.

line 26: Perhaps replace "as ice can ... during this time step." with simpler like "from mass balance or flow from the  $k - 1$  cell. Such a situation is possible in the Plummer and Phillips scheme."

line 9: I don't think "vanishing" helps here; "small" suffices.

line 11: I am trying to guess the meaning. Probably: Replace "then still" with "close to" and the sentence now reads clearly.

eqn (18): Replace "=" with " $\approx$ " and end sentence.

line 13: Start new sentence here with "As  $h_{k,l} \rightarrow 0$  the expression on the right does not go to zero. Thus a finite amount ...".

lines 13–14: Remove "therefore still".

line 14: "vanishing" is clear here.

line 16: Typo? Should be "downstream cell  $(k + 1/2, l)$ ."?

line 20 (at end): Suggest "alleviates the"  $\rightarrow$  "restores". Also remove "issue described  
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above." as not needed.

page 4048, line 0: Suggest remove commas after "both" and "schemes".

line 12: The phrase "... a flux-limiting scheme is required and one can adapt a ..." makes no sense to me. It makes no sense because it does not match the ordinary meaning of "required". Flux-limiting schemes in the numerical analysis literature are only "required" if the goal is TVD and second order simultaneously. There is no assertion that either are achieved here! I think what is meant is "... a flux-modification scheme is required. We adapt one of the flux-limiting schemes from the conservation law literature, namely a ..."

lines 17–18: I would remove "along with ... boundary." as just causing run-on. It is obvious in the context.

page 4049: Around here I had cause to ask myself: Why does it suffice to limit only the thickness factor in diffusivity? Indeed, there is no heuristic argument for either why the scheme was constructed this way, nor is there a proof that there is exact or approximate mass conservation. There is verification, which helps greatly. But is there a hint on why this is a good approach?

eqn (26): The giant factor in square brackets, which computes a power of the surface gradient, could be defined once as (say)  $|\nabla s^i|_{k+1/2,l}^{n-1}$  and then reused in (15), (16), (26). By my understanding, this factor originates with Mahaffy and is the most distinctive part of her scheme.

page 4050, line 8: Replace "right" → "correct". Confusing here.

page 4051, lines 16–18 and eqn (33): I am not convinced equation (33) is correct but it may be sufficient. Note that in the constant D case for the simplest diffusion

$$u_t = D(u_{xx} + u_{yy})$$

and with the centered-space forward Euler scheme the stability condition from positivity

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(maximum principle) is

$$\Delta t \leq$$

line 5 (line numbering oddity): Typo. Should be " $c_{stab} < 0.1666$  and  $c_{stab} < 0.125$ " I think.

lines 9–12: Suggest replace "present" with "construct" in line 9. Then strike sentence "We construct ... to reproduce them." This sentence is not describing the current section and is merely attempting to report intent which is obvious at this point in the paper, and is stated in the first sentence of this paragraph anyway.

page 4052, eqn (37): Typo. " $x_s$ "  $\rightarrow$  " $x_m$ ".

eqn (38) and (39): I think there is no need to switch "q" to "Q" for this purpose, but it reads o.k. with the switch as is.

page 4054, line 14: I think "... extend the solution to the interval  $0 < x < x_s$ , we can ..." is clearer.

line 4: Typo. No "the" in "This must be ..."

page 4056, eqns (54) and (55): Left sides should be " $h_{s+}(x)$ " and " $h_{s-}(x)$ " with " $(x)$ " for consistency and clarity I believe. (This is merely stylistic I guess, not correctness.)

page 4057, line 14: "... numerically implement Eqns. (12), (13), and (14)."

line 19: Suggest "adequate"  $\rightarrow$  "sufficient".

3rd paragraph: In my opinion, because you are doing verification with a steady state solution, you should start this paragraph with clear sentences about the situation: "We start our numerical solutions with an initial condition of zero ice. We assume that the continuum solution should evolve toward the single steady state solution which we have found exactly. The results of our numerical computations for the type I scheme ..."

page 4058, lines 7–19 and Figure 5: You don't need to repeat the greenhorn error

made by Bueler et al (2005). It is not very natural or helpful to ice sheet and glacier modelers to have " $N$ " on the independent axis. It should be  $\Delta x$  instead.

page 4059, line 10: Words "finite difference" should be removed. As the authors know, finite volume and finite element methods have the same problem and with spectral methods the issue would be infinitely worse.

pages 4060–4062: I think I understand the argument in this appendix but I don't think it adds enough, nor is it substantial enough, to use up Cryosphere space. Can't it be demoted further to a note in the Supplement?

page 4067, Figure 2: The grey/light blue shading scheme shows up poorly in black-and-white copy (i.e. not at all). How about obvious hatching? Also, indices should be  $k - 1, k, k + 1, k + 2$  instead of  $l - 1, l, \dots$  (I was confused briefly whether I missed an earlier figure with " $k$ ", or if the text had one-dimensional solutions depending on " $y$ ", and etc.)

page 4068, Figure 3: Because purple is combination of red and blue, it is a poor choice for the exact solution color. How about dashed, bold black or something simple like that? (Making a good black white Figure here is at least painful and possibly hopeless. Acknowledged.)

page 4069, Figure 4: I don't think this Figure is clearly referenced from the text. I realize the situation is discussed in Section 3, but I'm not sure of the specific purpose here.

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Interactive comment on The Cryosphere Discuss., 6, 4037, 2012.

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