The Cryosphere Discuss., 6, C2580–C2606, 2012
www.the-cryosphere-discuss.net/6/C2580/2012/

The Cryosphere Discussions

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.

A. H. Jarosch et al.

alexander.jarosch@uibk.ac.at

Received and published: 20 December 2012

We would like to express our gratitude to both reviewers for their invaluable comments on our manuscript. In this response letter we have compiled a detailed list of replies to each individual comment made by both reviewers. Note that we first applied corrections to our manuscript in response to reviewer #1 and afterwards reworked the manuscript in response to reviewer #2, so some errors reviewer #2 identified have already been corrected. In the revision process the equation numbering changed. All equation numbers in our responses refer to the newly revised manuscript except when stated otherwise. The equation numbers in the reviewer comments refer to the discus-

sion paper.

1 Anonymous Reviewer #1

We would like to thank reviewer #1 for the very detailed comments on our manuscript, for reviewing our equations in great detail and for suggesting helpful changes of our text. We have already replied to the main general comment of reviewer #1 in a short comment in "The Cryosphere Discussions" (tcd-6-C2182-2012.pdf) and have included these results now in the manuscript in Figure 3 as well as in section 7.1.

Reviewer #1 found a mistake in our equations for the exact solution. This algebraic mistake, which is corrected now, did not change the overall outcome of our benchmark and the results of our numerical scheme comparison. Nevertheless we would like to thank reviewer #1 for pointing out this mistake in our manuscript, as it gave us the chance to re-evaluate our solutions. Now Eqs (48), (49), (50), (56), (57), and (58) are corrected and the numerical benchmark experiment was recomputed. That resulted in a slight change of the numbers in Table 1 (also corrected now), but the qualitative results did not change. Figure 3 has been updated as well.

With regard to the second part of the general comments, we have re-edited the text and formulae carefully and have considered all comments made by reviewer #1. A detailed response is given below.

1.1 Specific Comments

We respond to all comments made by reviewer #1 in the same order as stated in the original review.

1. **Comment:** Page 4038 line 2 "low-order" is the shallow model not zero order? Do

you mean different orders of shallow models? Why not “zero-order”?
Response: Reviewer #1 is correct, the shallow ice model is zero order. We have changed the respective text to zero-order.

2. **Comment:** line 16 replace “to” at start of line with “for” suggest to replace “planet’s” with “glacier’s” also suggest to clarify which ice masses, it is not clear what “These” is referring to here, do you mean mountain glaciers?
Response: We have changed the text to the suggestions made and have clarified the text in the introduction, which has been rewritten to be more clear now.

3. **Comment:** Page 4039, Line 3, suggest add “er” to “High"
Response: “er” added to the text.

4. **Comment:** Line 4 suggest “their high computational demand"
Response: “high” added to the text.

5. **Comment:** Line 11 “similar formulation” please clarify in what sense similar, a finite element or something else?
Response: Both other papers use finite differences, so we have changed the text accordingly.

6. **Comment:** Line 15 add e.g. in front of reference, sentence starts with “Many” implying there are many others
Response: “e.g.” added.

7. **Comment:** Line 19 “such a model” clarify, not clear what kind of model, you are using finite difference scheme in this papger and above finite element models are discussed
Response: We have clarified this part and explicitly state that the model discussed is a finite difference model.

8. **Comment:** Page 4040, Line 1-2 suggest to rewrite “basic shallow model .. can be cast as” to something like “the continuity equation together with Glen‘s flow law are the equations solved for the shallow ice model” , suggest to write Glen’s flow law, rather than just Glen’s law
Response: The suggested re-formulation of the statement has been used.

9. **Comment:** Equation (1) and (2) have plus between the time derivative of s and gradient of q, but equations (4), (6), (8) and (10) there is a minus, be consistent or define q as negative D*grad s for consistency
Response: We thank reviewer #1 for pointing this out and have defined $q = -D\nabla s$ in the updated manuscript, which leads to a “plus” in equation (1a). This standard way of defining the continuity equation is now consistent throughout the manuscript in all relevant equations.

10. **Comment:** Line 7 replace “usual constants” with something like “A and n are the rate factor and power law constants in the Glen’s flow law”
Response: suggested replacement text used.

11. **Comment:** Line 9 take out “usual”
Response: done.

12. **Comment:** Line 10 take out “so”
Response: done.

13. **Comment:** page 4041, line 1 “can be used” – clarify what you mean here, what can be used? Add “the” in front of so-called.
Response: The “can be used” refers to the obstacle problem, and we believe the text is quite clear here. Nonetheless we re-wrote the passage in the attempt to be even more precise in our wording.

14. **Comment:** line 2 “This” what? Clarify what it is that allows advances? And what do you mean by theoretical advances?

Response: We have rephrased this part to explain what theoretical advances can be made.

15. **Comment:** Line 12 “Roughly, these update ... " rewrite it is not clear what “these” that are referred to are
Response: We have rewritten this sentence for clarity.

16. **Comment:** Equation (6) why is the m in i+1 time step and not i?
Response: Reviewer #1 is correct, there should be $\dot{m}^i$ here, which we use now. Even though one could argue that with a lagged scheme like this, it is hard to tell whether $i+1$ or i is more correct.

17. **Comment:** Equation (7) later on in text you refer to this as projection step (e.g. line 11 page 4043) or post-processing (e.g. line 4 page 4046), chose one term for it and make it clear when the equation is introduced what it will be referred to later in the text
Response: We have removed “post-processing” from the manuscript and only use “projection step” now. Also we now introduce this term with Eq. 7.

18. **Comment:** Page 4042, Line 1 typo in “inequality”. are the “inequality contraints” given in Eq (2)? please clarify
Response: We have included a reference to Eq (2) and clarified the sentence.

19. **Comment:** Line 5 can you give a reference for this statement?
Response: We now cite Evans (1998) and Kinderlehrer and Stampacchia (1980).

20. **Comment:** Page 4044, Equation 14 here is plus in front of the gradient of q, and in equations 12 and 13 there is a minus in definition of q, please make sure that the sign is consistent throughout the paper
Response: The manuscript has been rechecked for consistency of the definition and sign of the diffusivities.

21. **Comment:** Equation 15 Why do you introduce y-dimension here (and in eq 14 and 16) all the compuations in the paper are done in x-dimension only so it is not necesssary to add these terms, unless you want to refer to them, however, I cannot find reference to these terms in the text. In the hn+2 term there should be +, not -, between hk,l and hk+1,l as it is the average of the two, Same in Equation 16, replace the minus with a plus in that average term.
Response: Eq (15) and (16) have been corrected as suggested. We introduce the y-dimension in these equations as the numerical benchmark discussed in section 7.2 is a three dimensional benchmark and we would like to keep the paper as general as possible, thus not limiting our results only to x-direction. Most of time in the manuscript we only discuss one spatial direction to preserve space as the extension into both horizontal dimensions is simple. We have added a footnote pointing out our motivation to save space.

22. **Comment:** Page 4045, Line 15-18 all the stepping is done in x, or k index, but in Figure 2 the stepping is in l index, suggest to change in either place, probably more logical to change l to k in Figure 2
Response: We have corrected Fig. 2.

23. **Comment:** Page 4046, Equation 17, the second h should have k+1 index (not only k)
Response: corrected.

24. **Comment:** Page 4047, Line 4, here is projection step mentioned, do you want to call it that, or post processing step?
Response: As mentioned above, we have decided to consistenly call it projection step, so no edit in this case.

25. **Comment:** Line 5, “likely to acquire ..” how? Because m is positive? Clarify
Response: We already describe the case in which the upstream cell receives mass from a cell further upstream. Also a $\dot{m} > 0$ would create a non-zero ice

thickness in this cell, as reviewer #1 points out correctly. We have included this fact in the text.

26. **Comment:** Line 14 “Consider a vanishingly small ice thickness in cell k,l” what do you mean, is h=0 in k,l or is it vanishing?
Response: We have replaced “vanishingly small” with “very small” to indicate that h is very close to zero in this case but to avoid the impression that $h = 0$.

27. **Comment:** Equation 18 is confusing, what is the x index on the q? Why is there not hk,l in the averageterm? Why is there both s and b in the gradient of the surface? Should the upper index on b in the first term be i+1?
Response: We have simplified Eq (18) and expanded on it in the text.

28. **Comment:** Line 20 do you mean the downstream cell (k+1,l)?
Response: This is correct, we indeed mean the downstream cell and we have corrected this typo.

29. **Comment:** Page 4049, Equation 21 check if the index on the first h after the equal sign should not be k-1,l
Response: Reviewer #1 is right, we have corrected this typo.

30. **Comment:** Equation 22 check if the index on the first h after the equal sign should not be k,l
Response: Reviewer #1 is right, we have corrected this typo.

31. **Comment:** Line 15 rewrite the text the two flux terms are from Eq. 20 and 19, not only 20
Response: We have edited the text accordingly.

32. **Comment:** Page 4050, Equations 30 and 31 the first has a plus sign, the other has minus sign in front of the gradient q term, should define q so it is clear that it is negative of the D grad s term

Response: q is now defined as negative D grad s (Eq. 1b) so the equations should be correct.

33. **Comment:** Page 4051, Equation 32 the first term should have dx (the index in k is changing) and the second term should have dy (the index l is change), again, why do you include y-dimension if you do not use it? Suggest to simplify all equations and drop the l index, since it is not used
Response: We have corrected the mix-up between Δx and Δy. To keep the manuscript more general as well as for the fact that a 3D benchmark is included in section 7.2, we prefer to include the second horizontal dimension in these equations.

34. **Comment:** Line 14-15 suggest to change “analytically computable solutions” to “analytical solutions”
Response: We have incorporated the suggested change.

35. **Comment:** Page 4052, Line 6 here the accumulation rate is called a, but in the text above it is m(dot), be consistent
Response: This was a leftover from a previous notation change. We have corrected our typo.

36. **Comment:** Line 7 sentence is a little strange with occupied and occupying, suggest to rewrite to something like “a continous ice region for the interval ...”
Response: The suggestion has been incorporated in the manuscript.

37. **Comment:** Equation 37 should the index on the latter x be m, rather than s?
Response: This is correct and we have replaced s with m.

38. **Comment:** Line 15 why not lower limit 0<x<xm?
Response: We have included the lower limit.

39. **Comment:** Page 4053, Equation 44 check the integration, I would think that the power on the right hand side should be n/(2n+2) (as you have also in equation (46) and rather than 4 in the denominator I get n*2(1/n), I may well be wrong, but check carefully, there seems to be confusion of n and 2 in the power term, leading to both cases
Response: This is correct, we did have a mix-up here. Nevertheless we get, after carefully rechecking our integration the factor $n2^{\frac{1}{n}}$ instead of the 4 in the denominator. We have corrected the mistake, thank reviewer #1 for pointing this out and have corrected all respective equations.

40. **Comment:** Equation 45 same comment as above
Response: Has been corrected.

41. **Comment:** Page 4055, Line 13 do you mean exact or analytical solution?
Response: We have corrected the section title as we mean exact solution.

42. **Comment:** Line 16 could you use the defined subscript x(page 4052 subscript x is defined as "ordinary derivative") to note the derivative of Q here? The superscript "Q' " has not been defined
Response: We have changed the text and use subscript x to note the derivative.

43. **Comment:** Equation 50 please check integration of equation 49, I am missing an (n-1) term, or the term that is the integration of the last bracket in equation 49, not sure how you end up with only the two terms that come out of the multiplication of the last bracket in equstion 50 with the rest. Again, I might be wrong, but please check again.
Response: Actually here we do not integrate, we differentiate as $\dot{m} = Q_x$ and the subscript indicates a derivative (cf. comment #42). The equation is correct as is, now Eq (54) in the new manuscript. We have thought about this a bit more and think the newly stated version of Eq (54) is better.

44. **Comment:** Page 4056, Line 3, again replace a with m(dot)
Response: done.

45. **Comment:** Eqution 51 check the algebra, I get xm(-1/n) in the denominator rather than xm(2n-1)/n, I may be wrong, please check. To be consistent replace x with x0 on the right hand side, or drop the marker on x on the left hand side
Response: Equation (51), which is now (55) is derived by taking the nth root of equation (53) former equation (49), so we are pretty confident that this equation is correct as is.

46. **Comment:** Equation 52 is correct (apart from possibly the power of xm, see comment above) if (44) is right, my suspicion is, however, that the power on the bracket should be n/(2n+2) and 4 in the denominator should be replaced by n*2(1/n), see comment above
Response: We have corrected this equation accordingly. See also above comment # 39. Integrating Eq (55) introduces an extra 1/6 in Eq (56), (57), and (58), which has been missing in the discussion paper. Nevertheless the term $2^{\frac{1}{n}}n6$ is close to the initially stated 24 for $n = 3$. As pointed out above all results have been recomputed and there is no qualitative difference to the discussion paper.

47. **Comment:** Page 4057, Line 5 add equation 12 into the bracket
Response: done.

48. **Comment:** Page 4058, Line 2 replace increasing with decreasing
Response: done.

49. **Comment:** Line 4 suggest to replace "recreate" with "create"
Response: suggestion used in the manuscript.

50. **Comment:** Line 5 replace "of" with "for" 50 000 years
Response: done.

51. **Comment:** Line 10 add “s” in described - not clear what is meant here with modification, please clarify the text
Response: We have corrected the typo. “modification” refers to Eq 17 in this case and we have moved the reference to be more specific in the text.

52. **Comment:** Line 18 why this particular number of years? Explain or clarify
Response: The particular number of years is a given evolution time in the benchmark, as we now note in the text.

53. **Comment:** Line 24 What do you mean by “outperform” does it perform better?
Response: Yes, the MUSCL scheme performs better. We have clarified the text.

54. **Comment:** Page 4059, Line 19 “best” have you tried “all” and thereby can conclude it is the best, or is it the best of the ones you tested?
Response: This is of course correct, we have only tested the presented subset of all possible limiters. We have changed the text accordingly.

55. **Comment:** Line 22 suggest to take out “case of” do you want to use “exact” or “analytical” solution as in line 3 page 4060? Suggest to only use one term throughout the paper
Response: We have removed “case of” and only use “exact” solution in the text now.

56. **Comment:** Line 23 suggest to replace “with” with “against”
Response: done.

57. **Comment:** Page 4060, Line 3, see above is it exact solution?
Response: We now use exact throughout the manuscript.

58. **Comment:** Line 4 take out s in equations
Response: done.

59. **Comment:** Line 5 do you mean finite difference scheme as above?
Response: Yes we do, this has been corrected.

60. **Comment:** Equation A1 why do you have x superscript on the q here? Both the index and the denominator indicate x direction
Response: This is again a leftover from a convention change. We have removed all superscript x in the appendix.

61. **Comment:** Page 4061, Line 1 “projection step” – what are you referring to here? It is not clear and Mahaffy should be referenced
Response: We include a reference to Eq. 7 where we describe this projection step.

62. **Comment:** Equation A4 should the second equation have equal sign, rather than $<$?
Response: No, this should not be an equal sign (it’s unlikely cell centres will be located exactly where the ice margin should be). Instead of $<$ the second expression should have $\leq$ however (to allow the possibility of equality, however remote).

63. **Comment:** Line 9 suggest to replace “To be definite” with something like “to take an example” Numbering has gone strange, Eq. A5 and A6 is only one quation. Shouldn‘t there be a power (n-1) on the $||$ term?
Response: We have replaced “To be definite” with “to take an example”. Also the equation numbering has been corrected and the missing power has been added.

64. **Comment:** Line 22 Eq. A5 has only left hand side
Response: The equation numbering has been corrected.

65. **Comment:** Equations A7 and A8 is only one equation. Again, should there be a power (n-1) on the first gradient term?

Response: The equation numbering has been corrected and the missing power has been added.

66. **Comment:** Figure 2 suggest to replace l with k
Response: We have changed Fig. 2 accordingly.

67. **Comment:** Figure 3 and figure 4 Suggest to indicate somehow the resolution of the solution, with points on the curves, or small delta x symbol
Response: We have included a small delta x symbol in both Figures.

2 Ed Bueler, Reviewer #2

We would like to thank reviewer #2, Ed Bueler, for his thorough evaluation of our manuscript, for clarifying several key issues we present, and for making very valuable comments on our new flux limiting scheme.

First we would like to address the four “most significant” issues stated by reviewer #2.

2.1 “Most” significant issues

1. **Comment:** 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.
Response: We thank reviewer #2 for pointing out this error in Huybrechts and Payne and our inconsistency of reporting this error as is. We have corrected the manuscript by citing Hindmarsh and Payne and refer to the correct schemes as M2 and M3 now instead of “type I” and type II”.

2. **Comment:** 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 identifying 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 braod 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 ignors 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!
Response: We do cite Mahaffy (1976) in our paper (e.g. right after Eq 5) but we thank reviewer #2 for his insightful clarification of the root cause of the difficulties in the current SIA schemes. We agree and we should definitely not blame Mahaffy for something she did not introduce. We are also aware of Mahaffy’s choice (i) but decided to test only explicit schemes for simplicity, thus adopting Mahaffy’s choice (ii) as reviewer #2 correctly states. We decided to adopt the suggested “naive projection” to identify the basic error at first, then continue to refer to it as projection step scheme, and have removed all unnecessary references to Mahaffy in this context.

3. **Comment:** The current article is a *desirable* perversion of the standard application of flux-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 non-standardness 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.)
Response: We take the label “*desirable* perversion” as a compliment for our

approach and have removed the line about being total-variation-diminishing from our manuscript as it is no obvious goal as reviewer #2 points out. We aknowkege the nonstandardness of our approach now in the respective text (cf next comment) and explain in more detail where the idea of this flux-limiting scheme comes from. We also agree that we all currently should not worry about *that* water under the bridge.

4. **Comment:** 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:
$$\vec{q} = \vec{v}h \tag{1}$$
where h is the ice thickness and $\vec{v}$ is the vertically-integrated horizontal velocity, and
$$\vec{q} = -D\nabla s \tag{2}$$
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,
$$\vec{q} = \omega h^{n+2} \tag{3}$$
where I’ve made up a new symbol
$$\omega = -\frac{2A(\rho g)^n}{n+2}|\nabla s|^{n-1}\nabla s \tag{4}$$
Thus we may think of the Glen law SIA mass continuity equation as vaguely like a Burger’s equation:
$$h_t + \nabla \cdot (\omega h^{n+2}) = \dot{m} \tag{5}$$
versus
$$u_t + (u^2)_x = 0 \tag{6}$$

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.)
Response: We have now stated clearly our proposed and assumed flux factorization in the manuscript since we are indeed view mass continuity as described by reviewer #2. Thus it becomes clear in the revised manuscript how our flux limiting scheme is non-standard. Further we note that we are mostly interested in the positivity-preserving property of the MUSCL scheme, which limits the ice volume extraction to reasonable values in the upstream cells.

2.2 Line-by-line comments

We now continue to respond to the line-by-line comments of reviewer #2 enumerated as they appear in the review:

1. **Comment:** page 4037: The title is too long and boring. Perhaps "Restoring mass conservation to shallow models of mountain glaciers"?
Response: We agree with reviewer #2 that our manuscript deserves a less boring title and have changed it to “Restoring mass conservation to shallow ice flow models over complex terrain”. We have modified the suggested title slightly to attract the ice-sheet community as well (as pointed out by reviewer #2 below) and to be more specific that we talk about ice flow models which use the shallow ice approximation, even thought they are indeed “shallow models”.

2. **Comment:** 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."
Response: We changed the sentence to the suggested phrasing.

3. **Comment:** 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?
Response: We have removed the word “substantially” from the text as a response to the comment. That the mountain glaciers will continue to contribute to sea level in the future is not speculative as the referenced papers demonstrate, but reviewer #2 is right, the word “substantially” is not a good quantitative measure in this instance. We have decided to keep this example of an application in the introduction to not loose the applied readership early on.

4. **Comment:** 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.
Response: This is of course true, there is not an ad hoc measure for which resolution is best. The "O(10^7)" comes from our experience of modelling large regions at sub-km spatial scale. Nevertheless we have replaced "O(10^7)" with “many (e.g. 10^7)” to respond to the reviewer’s comment and yet to give a rough estimate of what “many” means for the not seasoned numerical modellers.

5. **Comment:** 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.
Response: True, this is actually obscure. We have removed the reference to Fastook and cite Mahaffy now in this section.

6. **Comment:** 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.
Response: We have rephrased the sentence from "..., where steep bed topography is rarely an issue, ..." to "..., where steep bed topography can be an issue as well (e.g. East Greenland),...".

7. **Comment:** line 19: I would hyphenate "second-order" and "flux-limiting" because "second" modifies "order" and "flux" modifies "limiting".
Response: Done here and throughout the manuscript where appropriate.

8. **Comment:** 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.
Response: Agreed, we have introduced Γ and corrected all equations to use it, except in section 5, where we expand Γ again for clarity in the integrations.

9. **Comment:** 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 ..."
Response: Good suggestion, we changed the text accordingly.

10. **Comment:** line 18: New, shorter sentence here, perhaps: "Negative ice thicknesses are never realized. In addition ..."
Response: Suggested change incorporated.

11. **Comment:** page 4041, line 6: Perhaps break and simplify: "Our aim here is more practical. We address shortcomings ..."
Response: Suggested change incorporated.

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

Response: We have rephrased this part to make clear that Eqs (4) and (5) do stem from Mahaffy's work but Eq (6) comes from Hindmarsh & Payne as their semi-implicit time stepping.

13. **Comment:** line 14: No "roughly" needed: "These methods update ..."
Response: "roughly" has been removed already by suggestions from reviewer #1.

14. **Comment:** 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.
Response: Our flux-limiting method applies to implicit and explicit time-stepping methods, so we do not limit ourselves at this point to pure explicit methods in the text. It is correct that we later on only test explicit time-stepping for simplicity. To respond to this comment from reviewer #2, we have added a statement before Eq. (35) that we use explicit time stepping for simplicity.

15. **Comment:** 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.
Response: "Inequality" fixed already (reviewer #1). With Eqs (8) and (9) we want to point the interested reader to "what's next to come" and give an outlook on how to proceed with these schemes. Unfortunately we did not have the time to evaluate this approach right away but plan to do this in the near future.

16. **Comment:** 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.
Response: We now introduce Eqs (12) and (13) as (12a) and (12b) to respond to this comment.

17. **Comment:** eqn (15): Typo. The average thickness is $h^i_{k,l} + h^i_{k+1,l}$ with a "+".
Response: Already corrected (reviewer #1).

18. **Comment:** page 4045: As noted earlier, eqn (16) is not the "type II" used in EISMINT I.
Response: Correct, we have dealt with this in response to “significant issue #1” above.

19. **Comment:** eqn (16): Typo again. The average needs a plus.
Response: Already corrected (reviewer #1).

20. **Comment:** 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!"
Response: We have corrected this confusion in response to “significant issue #2” above.

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

$$(s^i_{k,l} - s^i_{k+1,l})(h^i_{k,l} - h^i_{k+1,l}) < 0 \tag{7}$$

with $k + 1$ replacing k in the fourth instance.
Response: Already corrected (reviewer #1).

22. **Comment:** 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.
Response: Also reviewer #1 has commented on this. We have clarified this text section now.

23. **Comment:** line 24: Perhaps remove "A small amount of". Just "Ice mass is ..." is fine.
Response: Done.

24. **Comment**: 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." **Response:** Good idea, we used this.

25. **Comment:** line 9: I don’t think "vanishing" helps here; "small" suffices.
Response: Reviewer #1 commented on this too, we have already removed “vanishingly” and replaced it with “very”.

26. **Comment:** line 11: I am trying to guess the meaning. Probably: Replace "then still" with "close to" and the sentence now reads clearly.
Response: We have replaced “then still” with “close to”.

27. **Comment:** eqn (18): Replace "=" with "$\approx$" and end sentence.
Response: Replaced in Eq. (18) and sentence ended.

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

29. **Comment:** lines 13–14: Remove "therefore still".
Response: Removed.

30. **Comment:** line 14: "vanishing" is clear here.
Response: Acknowledged.

31. **Comment:** line 16: Typo? Should be "downstream cell $(k + 1/2, l)$."?
Response: This is indeed a typo, it should be “downstream cell $(k+1)$. Reviewer #1 commented on this too.

32. **Comment:** line 20 (at end): Suggest "alleviates the" $\to$ "restores". Also remove "issue described above." as not needed.
Response: Suggested edits incorporated.

33. **Comment:** page 4048, line 0: Suggest remove commas after "both" and "schemes".
Response: Done.

34. **Comment:** 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 ..."
Response: We thank reviewer #2 for clarifying this issue and have incorporated the suggested changes.

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

36. **Comment:** 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?
Response: Other than we "loosely" adapt the MUSCL scheme for the Burger's equation and identify h as the most important term in the flux equation, no, there is not. Verification was our measure by which we decided that this is a good approach.

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

Mahaffy and is the most distinctive part of her scheme.
Response: Great idea, we define the term now as Eq (14), cite Mahaffy, and reuse the term in Eqs (15), (16) and (30), which used to be Eq (26).

38. **Comment:** page 4050, line 8: Replace "right" → "correct". Confusing here.
Response: Done.

39. **Comment:** 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
$$u_t = D(u_{xx} + u_{yy}) \tag{8}$$
and with the centered-space forward Euler scheme the stability condition from positivity (maximum principle) is
$$\Delta t \leq \tag{9}$$
Response: We assume reviewer #2 meant this equation:
$$\Delta t \leq \frac{1}{2}\frac{\Delta x^2}{D} \tag{10}$$
as the review pdf did not include the complete equation. We argue that the stability criteria developed by Hindmarsh should be sufficient as they were developed for non-limited flux schemes. Our flux-limited scheme produces less flux, less diffusivity, in extreme cases in comparison with the schemes discussed by Hindmarsh. Thus we should be "safe" using his stability criteria, which will result in smaller time steps than required.

40. **Comment:** line 5 (line numbering oddity): Typo. Should be "$c_{\mathsf{stab}} < 0.1666$ and $c_{\mathsf{stab}} < 0,125$" I think.
Response: True, we have corrected this typo.

41. **Comment:** 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.
Response: Done.

42. **Comment:** page 4052, eqn (37): Typo. "x_s" → "x_m".
Response: Done (suggest by reviewer #1 already)

43. **Comment:** 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.
Response: We decided to keep the notation as is.

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

45. **Comment:** line 4: Typo. No "the" in "This must be ..."
Response: Done.

46. **Comment:** 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.)
Response: Done.

47. **Comment:** page 4057, line 14: "... numerically implement Eqns. (12), (13), and (14)."
Response: As we changes old Eqs (12) and (13) to (12a) and (12b) now, citing Eqs. (12) and (13) now is sufficient.

48. **Comment:** line 19: Suggest "adequate" → "sufficient".
Response: Replaced.

49. **Comment:** 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 ..."
Response: A very good suggestion, which we use now.

50. **Comment:** 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 Δx instead.
Response: We have updated Fig 5 and the respective text.

51. **Comment:** 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.
Response: This is of course true and so we have removed "finite difference" to be general.

52. **Comment:** 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?
Response: It could, but as "The Cryosphere" is an online journal, we decided to keep the appendix as an appendix. It is also a "cast-iron" exact analytical result that shows that centered averaging schemes for h have problems, which should not "disappear" in the supplement.

53. **Comment:** 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 $l-1, l, \ldots$ (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.)
Response: The indices in Fig.2 have been replaced to be ks already (comment by reviewer #1). We do not think that in the age of pdf documents and on-screen reading black-and-white colors are required. Thus we do not change our color scheme.

54. **Comment:** 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.)
Response: We have replaced the “magenta” line with a bold orange line as a dashed black line also does not work well. Orange should be fine.

55. **Comment:** 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.
Response: As stated in section 7, we would like to visually demonstrate how the MUSCL scheme conserves mass and the M2 does not in the Plummer & Philips problem, so we keep the figure as is.

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