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Interactive comment on "Mass-conserving subglacial hydrology in the Parallel Ice Sheet Model" *by* E. Bueler and W. Van Pelt

Anonymous Referee #2

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Mass conserving subglacial hydrology in the Parallel Ice Sheet Model - review

This paper describes a new sub-component of the open source ice-sheet model PISM, which accounts for subglacial drainage of meltwater. The model and a number of subcases are described in considerable detail and then the numerical implementation is described. A simple steady state solution is used to test the numerical method, and the model is then applied to the whole of the Greenland ice sheet.

I enjoyed reading this paper. It represents to my knowledge the first serious attempt to include an evolving subglacial drainage model within an ice-sheet scale ice-sheet model, and the results are encouraging. As such, I would like to recommend publication. However, I have a few issues that I think need to be clarified or thought about first.

The major comments are here, followed by some specific but more minor points.

1. The first term of (33), involving the pressure derivative and which represents changes in englacial water content, ought to appear in (34a) also, since this term derives from the mass flux into/out of the englacial system, and it is the addition of this term to the *mass* conservation equation (34a) that gives rise to its appearance in (33). As it stands in (34), subtraction of the first and third equations puts the $\partial P/\partial t$ term into the opening/closure equation $\partial W/\partial t$, which I don't see justification for.

2. p4738, l11, and this section generally - is it clear that these arguments prove stability for the *system* of equations in this model (in which the coefficients in (60), say are varying at each timestep due to the pressure evolution)? The analysis here seems to be for a standard advection-diffusion equation on its own, but it is not immediately clear to me that standard results can be used here. I have no doubt that the method is stable, but I think if the stability properties are to be discussed in this much detail, it needs to be done for the whole system together, and not for the individual components of the operator splitting separately. Or if there is an argument as to why this is sufficient, that should be included.

3. The boundary conditions should really be described in more detail. It'd be helpful to state mathematically what boundary conditions are imposed (in section 5 say), rather than having it algorithmically described in section 7. In particular, the diffusive nature of the W equation suggests that one should apply some sort of conditions on W at all boundaries, but these are rather hidden, and in section 9.1 it is claimed that there are convergence issues associated with a jump in W, which seems at odds with the diffusive term. I suspect the boundary conditions are mostly imposed by step (vi) on p4742, but I was not entirely clear on what is meant by 'not computing' the divided difference contribution to the flux divergence.

Finally, I felt the paper might be shortened without losing detail; there are a number of places where the discussion of relatively simple points is laboured. Sections that might be reduced include section 2.4, section 4.3, section 6.2, section 7.1, section 9.2.1, (could just reference Aschwanden et al for much of this?), section 9.2.4, and the appendix.

Specific comments

1. p4708, l3, also throughout - I do not see why the parabolic equation is always described as a 'regularization', which suggests some element of artifice. For the physical system described, the equation *is* parabolic, and there is no need to treat it as a regularization.
2. p4708, l7 - I'd temper this by saying that till is 'sometimes' observed, as I don't think it is true that it is always observed.
3. p4708, l20 - it is not the inclusion of wall melt in the mass conservation equation that leads to the instability but rather then inclusion of wall melt in the kinematic opening-closure equation.
4. p4711, l9 - given the coupling with PISM, it seems a bit odd to say that you 'accept' the hydrostatic approximation, since you should be calculating P_o consistently with the ice flow. As I understand it P_o is always hydrostatic for the level of approximation in PISM, so this would seem a better justification.
5. p4713, l11, also throughout - I find the repeated reference to the 'advection-diffusion equation' a bit misleading as although it has advection and diffusion terms, it is rather different from what is normally associated with that term, as the velocity depends on the pressure which is evolving simultaneously. Perhaps this is my own connotation of advection-diffusion, but I think it should be emphasized that (12) is not stand-alone and is inherently coupled to more equations.

6. p4717 - the prescription of a minimum value for N seems a bit arbitrary - could it be explained briefly what this physically represents? (*e.g.* this is the level at which the till becomes sufficiently deformable that a cavity system is developed and that effectively caps the water pressure?) I would have thought a critical pressure, rather than a critical fraction of overburden, might be more reasonable? That aside, I found the prescription of W_{til}^{max}, and subsequent derivation of till thickness η (22) rather odd, since it seems more natural to prescribe the thickness of till η and have W_{til}^{max} derived from that (and δ and P_o). As it is, η varies as the overburden varies (when coupled with ice flow), so that there is implicit redistribution of sediment.
7. p4721, (30) and following sentences - it is a bit confusing to write $P = P_{FC}(W)$ here (and in (29), and similarly in the appendix), as the formula depends upon P_o and therefore space, as well as on W. It'd be clearer to include x as an additional argument here ((30) is not then a clean porous-medium equation).
8. p4723, l5 - this sentence reads rather strangely. Aren't most of the parameters 'user-adjustable'? What is meant by temporal 'detail' in the pressure evolution - is it suggesting that $\phi_0 = 0$ is 'correct'? Later that paragraph, what is meant by diffusive 'range', and would it not scale as $\phi_0^{1/2}$?
9. p4723, l16-22 - this algorithm is certainly a lot more computationally efficient than the method used to solve the elliptic variational problem of Schoof et al (2012), but it should be noted that the schemes are not solving exactly the same problem (at least, for non-steady states, which is where the computational cost lies). Difficulties of Schoof et al's method stemmed notably from discontinuities in W associated with unfilled cavities, which are absent in the current problem.
10. p4727, l6 - I'm not sure how much we know that the system is close to steady state 'much of the time', so I'd recommend removing this; justification for looking at steady states is probably not required.

11. p4728, l1 - clarify that this statement is for a given discharge?

12. p4729, l11 - I am confused by the 'solution' $W = W_r$ to (45). This would only be a solution if the ice surface were a very particular shape?

13. Section 6.2 - the discussion of the boundary conditions here seems unnecessarily confusing and it could be much clearer just to state the shape, sliding velocity, and boundary conditions that are used, rather than explaining in generality how the solution works. Note that W_c has only been defined in the appendix so comes out of the blue here. Since $r = L$ is the edge of the domain, the distinction between L_- and L seems pedantic (the definition of variables outside of the domain has not yet been given, and is more of an algorithmic issue).

14. p4731, (48) - φ_0 is ω_0?

15. p4731, l7 - presumably the numerical value for W^* given here corresponds to a particular parameter set? It must depend upon k, H_0 etc?

16. p4736, l20 - the right hand column here seems unnecessary?

17. p4739, l25 - The numerical values of timesteps here and on p4732 could be brought together to save space and avoid repetition. The value of ϕ_0 used seems rather large; if a smaller value were used (going towards the elliptic limit) might the timestep restriction become restrictive?

18. p4748, l15, and figure 11 - I was a bit confused by the comparison of W and P/P_o; what significance is P/P_o believed to have? Doesn't a lot of this information come just from the steady state relationship between W and P in (A4)?, The caption is a bit confusing when it refers to 'pairs' (W,P), but what is plotted is really P/P_o.

19. p4749, l9 - what is the 'actual diffusivity of the advective flux'? 'diffusive nature of the advective flux' might be clearer.

20. p4749, l15 - this statement is rather vague, and I'm not sure what it's trying to say.

21. p4751, l17 - something missing from this sentence?