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The Cryosphere
Discussions

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3, C175–C180, 2009

Interactive
Comment

Interactive comment on "Diagnostic and prognostic simulations with a full Stokes model accounting for superimposed ice of Midtre Lovénbreen, Svalbard" *by* T. Zwinger and J. C. Moore

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Aside from the critics towards an improvement on the quality of some of the presented Figures – which shall be dealt with later – we would like to respond on the remaining open points:

The age distribution in fig. 4 seems to be erroneous. The age must increase monotonously along a particle trajectory (the diagnostic run is steady state?), which *can obviously not be the case in Fig. 4. The steep jump between stake 5 and 4 also seems to be pointing to some kind of flaw in the calculation, and also the decreasing age in the tongue area. The pure advection equation (Eq. 12) is known to be difficult to solve numerically. Did the authors use a diffusion term to stabilize? This may explain part of the non-monotonous change along trajectories.*

Looking at Fig. 4 of the submitted paper, please bear in mind that a cut-off value has been introduced. The alleged jump is not really a jump but rather a high gradient that is continued towards the downstream side if rendered without a this cut-off value (see first attached figure).

Also be aware that the stake-line (we will avoid the perhaps misleading wording "centre line" in future revisions) is not automatically a flow line. It definitely is not at the tongue of the glacier, which explains the decreasing age in this region of the stake-line. This is to be seen in the second attached picture. It shows that the flowline (white dashed) starts to clearly deviate from the stake-line (white solid) between stake 4 and 3.

We are aware – and that is also explicitly mentioned in the text – that the solution of the advection equation in steady state with no-slip conditions at parts of the domain is anything but a trivial task. To that end, we chose the well known implementation of the Discontinuous Galerkin method as it was presented by Brezzi et al. [1] (we will insert that reference in a revised version of the article). We think that a detailed explanation of the numerical method would go beyond the scope of the Journal. This method does not contain any artificial diffusion terms to stabilize the solutions and even is able to account for discontinuities – as its name suggests – in the solution. In our opinion it comes clear from the text that the solution has to be interpreted with care, especially in the vicinity of the bedrock. Certainly, we could try to "tune" things by introducing artificial outflow at the bottom (turning it into a well-posed problem); sliding, as the main issues occur in the frozen part of the bedrock, certainly does not improve the results. Nevertheless, the results (especially the comparison with the dip-angles) were

so encouraging that we are of the opinion that this is worth being published.
Reference to be added:

[1] Brezzi, F, L.D. Marini and E. Süli. 2004. Discontinuous Galerkin methods for first-order hyperbolic problems, Math. Mod. Meth. Appl. Sci., 14, 1893-1903

How accurate is the computation of the dip-angle with Eq. (15). Having in view the strange result in Fig. 4, the evaluation of Eq. (15) seems to be very difficult. This is even more the case since derivatives are a bit difficult to calculate accurately in a course resolution finite element field, and in the denominator, you have a difference of derivatives, which makes it even more difficult?

Same arguments as before count here. We clearly placed a warning in the text and in the explanations on the derivations of the measurements (which themselves contain sources of error that are explained in the text) and the computed results. Despite the difficulties, the principle distribution of dip angles seems to be reproducible. We think that this is a result worth being published. If the reviewer(s) are of different opinion, I guess it is upon the editor to decide whether these results in combination with the explanations are worth being included or have to be dropped. At the end of the day, that is what we get if dealing with diagnostic (steady state, fixed surface) runs. The alternative would be to do prognostic runs which would demand accurate forcing not just over centuries, but rather millennia – and that is what we do not have at hand.

The logic of neglecting air pressure is wrong. It's the gradient of the air pressure which is negligibly small, and thus, air pressure can be treated as a constant background pressure field, which does not contribute to flow for an incompressible fluid.

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You are right, we neglected mentioning that for the mechanical problem it is essential that the the gradient of the pressure is small, too. Thanks for pointing out this sloppiness from our side. For the thermodynamical part (and consequently the thermo-mechanical coupling) it is not completely without importance in connection with not altering the pressure melting point.

Interactive comment on The Cryosphere Discuss., 3, 477, 2009.

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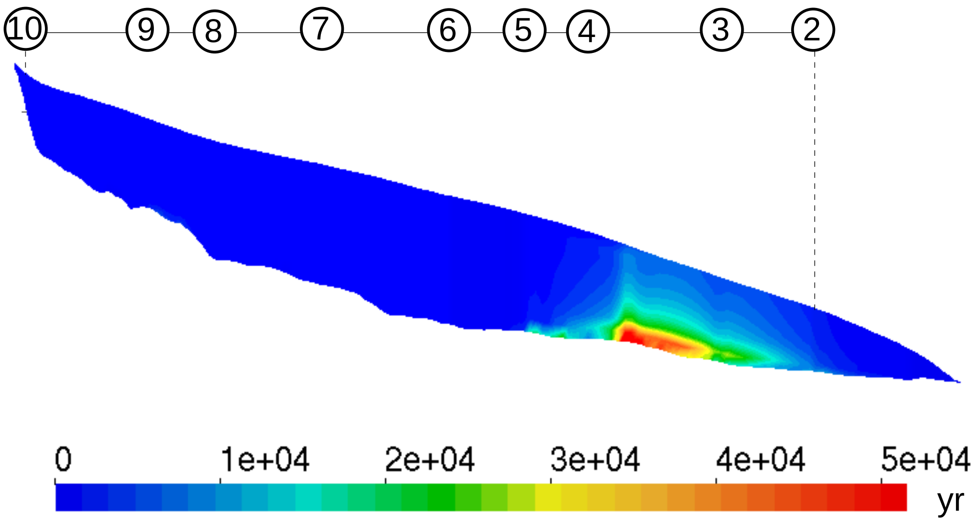

Fig. 1. Age distribution along stake-line without cut-off value

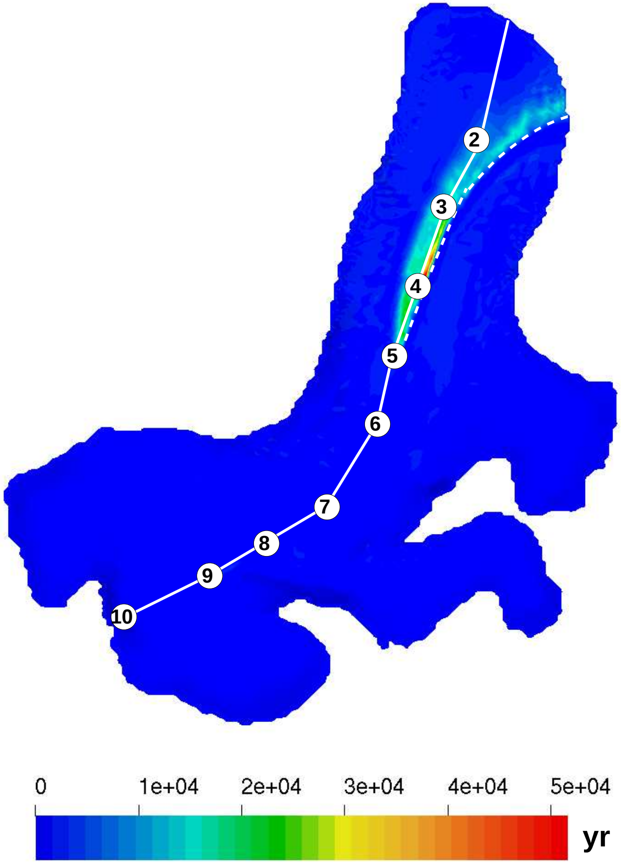

Fig. 2. Age distribution from top without cut-off value with complete stake-line (solid white) and approximated flow line (dashed white) annotated