Review of *Performance and applicability of a 2.5D ice-flow model in the vicinity of a dome*

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May 20, 2016

This is my second review of *Performance and applicability of a 2.5D ice-flow model in the vicinity of a dome*. I am glad to see that the authors addressed many of my and another reviewers' comments, and the paper is much improved, both in content and syntax. However, I maintain two significant issues with both the approach and presentation of the work.

1. I remain uncomfortable with the authors' neglect of lateral shear stresses. In their response to my initial comment on this issue (P7 L20 in the first manuscript version), the authors claim that they cannot maintain the influence of lateral shear stresses while operating in a topologically 2D computational framework. This is patently false: See the myriad papers on lateral drag in fjord flowline models as used by authors such as Van der Veen, Enderlin, Nick, and many others. The key is integration over the transverse coordinate, followed by the assumption that values in the longitudinal coordinate are averaged over the transverse one.

   I recognize that the authors' line of reasoning follows directly from Reeh (1988), who claims that lateral shear stresses are zero at ice divides and at basin centerlines (defined as a local velocity maximum), and that this is a justification for neglecting the transverse stress term in a flowline model. However, this seems to contradict the possiblity of lateral drag. The reason that this is false is because it is really the transverse derivative of lateral shear stress which balances driving stress, which is generally nonzero. Alternatively, the effect of such terms can be seen by (once again) integrating across the transverse coordinate. One immediately sees that there are lateral drag terms which emerge. I don't necessarily think that neglecting this term is a fatal flaw; I do think that this assumption ought to be stated, justified, and discussed in terms of the potential errors that it could induce in the model results.

2. The method for computing curvature doesn't make sense to me (though perhaps this is just my own ignorance). The definition of radius of curvature is the radius of the circle that locally approximates a curve. If you have surface elevation contours for each point at which you need $R(x)$, why not just fit a circle to the points near your flowline? I'm particularly nervous about the use of a bivariate interpolant: The object that you need to take the curvature of is topologically 1D (a surface contour). What is the physical

meaning of the curvature of a bivariate function? The radius of a local spherical approximation of the surface? If the latter, then that's not right. Furthermore, the last sentence of the added paragraph states that $R(x)$ is being taken as the *inverse* of the curvature of the contours, which is definitely wrong (presumably a typo, but I can't say for certain). I think a much more quantitative and rigorous description of exactly what was done to compute this radius of curvature is in order here; ultimately, I still don't understand the methods here, yet they remain the crux of the paper. If this work is based on existing methods, a reference might be nice too.

Specific Comments (Somewhat abridged until above major comments are addressed):

- P1 L20: Statement about computing time needs a citation or at least an explanation.
- P1 L24: When I said that the Durand paper wasn't applicable, I didn't mean that a citation was not needed.
- P1 L24: Maybe specify that you mean an ice cap which exhibits mirror symmetry. This isn't true for an axisymmetric one: just look at the difference in divide heights between the 2D and 3D EISMINT experiments.
- P5 L26: 'dome surface *area*'
- P6 L8: Actually globally Cartesian in the case of straight flowlines
- P8 Eq. 19: surface mass balance is traditionally typeset as $\dot{b}$ or $\dot{a}$ in glaciological literature, but that's just a notational choice.
- P8 L22: 'representative of a real ice sheet'. Citation needed.
- P9 4.1.1: Should this heading be 'analytical comparision' or something like that?

Finally some responses to a few of the rebuttals that the authors made towards my initial review.

- I asked why not use the RHS of Eq. 12 via particle tracking, rather than computing radius of curvature. The authors responded that this would be inefficient and that it would not work on a ridge. I still fail to see why this would be true. It seems to me that doing what I suggested would provide something very much like the authors' Figure 1, from which (a differentiable) $W(x)$ could be computed. At the very least, explicitly computing the flow tube would provide a boundary for the region over which $R(x)$ should be computed. I'm not trying to get the authors to change their methods here, mostly just curious at this point.
- I suggested using a more precise metric for error quantification, such as standard error. I would be happy with the authors' choice of using RMSE, however that doesn't seem to be what they are reporting. RMSE has units, and the error here is reported in terms of percentages. I would like a more explicit statement of what is actually being computed in terms of error (namely, how is RMSE being normalized?).