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Geoscientific Model Development Discussions

Interactive comment on "Performance and applicability of a 2.5D ice-flow model in the vicinity of a dome" *by* Olivier Passalacqua et al.

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In *Performance and applicability of a 2.5D ice-flow model in the vicinity of a dome* the authors compare the flow velocities (and, to a lesser extent, geometry) produced by the Stokes' solver Elmer/Ice to that produced by a so-called 2.5D model, which uses a paramaterization of flowline width to reduce the dimensionality of the model equations while still approximating the full 3D physics. A critical parameter in this process is the flowline width, which they approximate from surface elevations using an ad-hoc (but largely unspecified) method. They find that the 2.5D model works very well when the width is specified exactly, and that much of the error in the 2.5D model output is a result of inaccuracies in flowline width, which can be exacerbated by using an incorrect sample size when evaluating surface elevations. Additional complications arise when conditions are non-isothermal.

General Comments:

For this paper to warrant publication, it would require its methods to be outlined in much greater detail than they currently are. The primary nuance of the work, mainly the computation of R from a surface DEM is given only a few lines of description. The metrics by which the authors judge quality are primarily left unstated. Many modelling assumption (i.e. that surface elevations can act as a reasonable proxy for the direction of a Stokes'-based velocity field) are utilized but unjustifed. Suffice to say, this work would not be repeatable based upon the information given here. I would like to offer up an opinion on whether the results presented are significant to the field of glaciology, but lack a sufficient understanding of the quality of the results to do so.

The paper is rife with incorrect grammar, spelling, and otherwise improper English, to the extent that it partially inhibits understanding of the methods and results contained herein. As a reviewer, I don't really see it as my responsibility to correct for these issues, particularly when at least one of the co-authors is a native English speaker, and I will not provide comments on these points. As such, I would suggest that the authors employ or otherwise recruit a capable copy editor to polish the manuscript. It is presently not ready for publication for this reason, scientific merit aside.

Specific Comments:

Abstract: the abstract introduces a significant amount of jargon, such as "scanning window", "reversed surface convexity", and "partly reversed velocity profile", which the reader cannot know the meaning of without reading the paper. This undercuts the purpose of the abstract.

P1 L8: The relief in question is on the order of tens of meters, yet the authors suggest that the so-called "scanning window" should be on the order of kilometers. This directly contradicts this line.

P1 L16: the equations in question are either Stokes' equations, or they are not; the "full-" modifier is unnecessary.

P1 L21: Durand (2011) does not appear to actually address this issue, though it does use a flowline model. Also, what are the "regular flanks of an ice sheet"?

P1 L22: "plane strain" does not seem to be used correctly here.

P2 L1: I think I know what is meant by "a particular vertical surface", but this would be greatly clarified by addition of said surface to Fig 1.

P2 L14: Why is the parenthetical "(column-flow model)" included here? It does not seem to serve a purpose and doesn't seem to be referenced later.

P2 L22: "In particular,..." I do not understand this sentence.

P2 L31: width of the flow *tube*?

P2 L31: A DEM gives the shape of the surface and its contour lines when velocity is available, as well.

P2 L34: Assuming that flow is always oriented along the surface gradient is a wrong assumption, and the differences between considering gradient-based curvature and velocity-based width goes far beyond issues of numerical accuracy. Have a look at any computation of balance velocity ever made; if flow is routed down-gradient then the surface velocity looks like a river network, which is silly. Longitudinal stresses matter for flow routing!

P2 L35: I don't know what "scanned" means, and as such I cannot assess whether the authors' statements about ambiguity in surface curvature have any merit. It would be better to hold off on elaborating upon it further until some basic definitions have been stated.

P3 L10: On "diverging geometries": flow fields can diverge, because they are vector fields. Scalars cannot diverge, and the fields representing geometry are certainly scalars.

P3 L17: What "1D parameter" are we talking about here? Where is this question returned to in the rest of the text?

P3 L25: Please state more clearly that the 3D model is used to construct a set of surface geometries based on different choices of lateral boundary conditions. This 3D model output is then taken as input to the 2.5D model. In particular the computed surface elevations from the 3D model are used to generate the curvature for the 2.5D case.

P3 L27: "Finally we discuss the importance of ...". I do not understand this sentence. "Certain authors"? Meaning the authors of this paper? Or Reeh?

P3 L31: "Similar synthetic geometry". Similar to what?

Sec. 2.1.: It would be useful to specify, as did Gillet-Chaulet and Hindmarsh (2011), that the edges of the domain do not correspond to a physical boundary. Indeed, the authors could draw a considerable amount of inspiration from that paper on how to describe the setup of the model experiments. To understand what was being done in this paper I had to read that paper, and most readers would appreciate eliminating the intermediate step.

Sec. 2.1.: Please distinguish between a dome (what is ostensibly being modeled) and a cylinder (the shape of the mesh).

Sec. 2.3.1: If the authors are unwilling to state the Stokes' equations completely, then it might be best to not state them at all, and just give a reference to one of Elmer/Ice's numerous model description papers. Otherwise, there are many missing definitions (e.g. $A(T)$, $\dot{\epsilon}$, etc.).

P4 L8: The strain rate tensor (written in the paper as ϵ) needs a dot over it.

P5 L8: Calling a a function is confusing in this context, because it's a constant. Just call it the accumulation rate.

Eqs. 9/10: Trigonometric functions are usually typeset upright, rather than in italics.

P5 L20: L is not used, so why is it defined here?

P6 L2: It would be useful to use different coordinate symbols for the global Cartesian system that the 3D model uses versus the local system used by the 2.5D model.

P6 L13: Asserting that an assumption is reasonable requires a reference.

P6 Sec. 3.1: This section needs some clarification. Is it the ridge which runs along $y = 0$ or the centerline of the 2.5D model coordinate system?

P6 L24: If a flow tube diverges, then the tube surface area gets *larger*, and velocities are reduced because an equal flux moves through a larger area.

P7 L20: Why neglect transverse shear stresses (i.e. σ_{xy})? Elmer can solve Stokes' equations, so technically it shouldn't be that difficult to include them here. There are plenty of width parameterizations out there (see, the works of Vanderveen on fjord wall drag, for example).

P7 L20: Please provide a page number for Jaeger (1969). Also, consider citing Hvidberg (1996) instead, as this published article is much more readily available than a PhD thesis. Also, according to Hvidberg (1996), this result is derived for the axisymmetric case. Can the authors show that it remains valid in the case where this assumption is violated (i.e. $\alpha > 2$). In any case, these equations need considerably more explanation.

P8 L3: Do the authors mean "imposed *horizontal* velocity profile"?

P8 L12: I like Elmer too, but its efficiency isn't really relevant here.

P8 Sec. 3.4: This section needs to be expanded greatly, and could do with some illustrative figures. After reading this, I really still have no idea where R comes from, and why changing the size of the sample changes this. If the surface contours are well approximated by polynomials, I fail to see why a small window shouldn't perform equally to a large window. This is really the crux of the method and a discussion of it

takes up a bulk of the results, yet it is given only a few sentences in the methods. How can a reader assess the validity of the results without knowing what the authors did?

P9 L3: Calling the case with the closed-form $R = x$ "2D" rather than 2.5D is confusing. The extra half-dimension comes from the fact that width variations are being parameterized, and that's still the case when R is known exactly. Much better would be to call these runs "2.5D with analytic R" or something like that.

Sec. 4.1.2: I wonder if using a different method to compute widths would be less error-prone. For example, since we're already assuming that the flow follows the surface gradient, why not try just finding two flowlines and computing the distance between them? That eliminates the need to compute a second derivative (usually an error prone activity). I guess if the rationale behind the way that curvature is computed were more fully explained, the answer to this question might be obvious.

Sec. 4.1.2: A bit of specificity beyond "an error range of 0-10%" is in order here. How about just reporting standard error?

Fig. 4,5,6,8: All need a legend.

P10 L7: I don't understand what this paragraph is saying, nor do I find any clues in Fig. 6. What is the significance of a concave surface slope?

P10 L19: "the error made with this more complete model seems now to be small enough to be used for dating purposes." What evidence presented herein supports this point? To make such a claim, the authors need to establish an error threshold (*a priori*) which must be met in order to claim that their method is accurate, and then go about showing quantitatively that the model performs up to this standard. At no point do I see any objective metrics for model performance in this regard.

P10 L25: What is the error here, and in what way is it consistent with Hvidberg (1997b)? Am I to take away that the 2.5D model does a better job perpendicular to the dome?

P10 L31: I am skeptical of the authors' hypothesis that there is significantly different flow directions at different points in the ice column, primarily because this would lead to symmetry breaking that does not occur in any models that I know of. It would be easy to test this idea, since evidently the authors have the full 3D model output in hand. I don't really know what's causing the strange non-physical vertical inversion of the horizontal velocity profile, but I suspect it has to do with the neglect of transverse shear stresses or vertical resistive stresses.

P11 L7: I am not familiar with the results from Hvidberg (2002). It would be helpful if the authors restated them.

Sec 4.4: This section is a non-sequitur. What is the "mass-only conservation model"? Are the authors referring to a calculation of balance velocity? If so, there are numerical considerations and different boundary conditions that the authors do not state. Unless the authors make a considerable effort to define what the model results that they are referring to actually are, this section should be removed.

Conclusions: This sections seems to be an afterthought; it is too short, ambiguous, and the conclusions stated herein are not clearly supported by the text.

Appendix: with respect to typesetting, the dot used to indicate scalar multiplication is not necessary.

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