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The Cryosphere Discussions

Interactive comment on "A full-Stokes ice flow model for the vicinity of Dome Fuji, Antarctica, with induced anisotropy and fabric evolution" *by* H. Seddik et al.

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This article is certainly exploring a worthwhile and necessary aspect of ice flow in the central regions of ice sheets. Deformation due to preferred crystal orientation plays a much larger role in ice sheet flow than had conventionally been accepted. And this is especially important near ice core sites where flow can affect the ice core record. This approach to 3D modeling of the Dome Fuji region with a enhancement factor-based anisotropy model is a great forward step towards understanding the effect of anisotropy. I think this paper should be published with some key changes in the text, but major changes in the model or analyses may not be necessary.

My major criticisms of this paper overlap with the other Referees' comments, I will not repeat all of them, except those that I think are particularly important or that have not yet been mentioned.

1. It relies too much on the Seddick PhD, thesis, which I had to download and study in order to understand aspects of this paper. The other paper it seems to rely heavily on is a paper is still in review and therefore not available to read. If this paper, Placidi and others 2008, is in press by the time this is published that would be acceptable, but the "refer to Seddick 2008 for details" need to be take out and replaced with at least a brief summary of the key points found in Seddick 2008, then a reference to it.

2. This paper does not put this work in context with other recent studies of anisotropic flow near ice divides. This is very important because Seddick and others use an Enhancement Factor approach to anisotropy, and it is important to discuss the effects of this approach compared to other anisotropy models that use tensor form. If I understand the CAFFE model correctly, it is very good at reflecting flow effects due to anisotropy when one stress term dominates the state of stress. When there are two equally important stress components and one leads to a stiffening of the ice (such as compression parallel to the caxes) and one leads to a softening of the ice (such as shear perpendicular to the c-axes), it is unclear to me how successful this type of model is compared more complete tensor models of anisotropy (Gillet-Chaulet, Thorsteinsson, etc). A discussion of this is necessary to make this paper more useful to the reader.

3. The authors do not spend enough time justifying their assumptions or discussing the effects of their assumptions on the model results. In addition to the Enhancement Factor assumption for anisotropy, I am specifically thinking of the steady state assumption, the no slip assumption, the heat flux assumption and the assumption to neglect recrystallization. The authors might consider doing some "sensitivity tests" to show the sensitivity of their model to these assumptions.

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4. I cannot understand the results of no fabric evolution from 800-2500m. Especially if the "recovery" processes of recrystallization are turned off. The fabric should develop as a result of total strain the ice experiences within one stress regime. Along the trajectory of an ice particle from from 800 to 2500, there is a finite strain rate (according to figure 7), yet it's fabric is not changing. That is difficult to understand, the authors need to explain this if it is a solid result from the model or revisit their model otherwise. Fabric can develop at any strain rate as long as there is enough time and at those strain rates an ice particle would spend a lot of time in that region.

5. I do not agree with the reasons given for the weaker fabric than thin sections data from the ice core. First, small strain rates do not prevent fabric formation if there is sufficient time involved (which there typically is near a divide). Second, recrystallization processes typically work to weaken fabric, not strengthen it, so turning off recrystallization should actually make the fabric stronger than observed. I think the weak fabric is a function of the type of anisotropy model - the enhancement factor method for calculating strain rates due to anisotropy does not produce the complete strain rate tensor that will develop the fabric.

6. The paper does not have clearly stated conclusions. The findings stated in the abstract are all general statements that do not seem to necessarily lead directly from the model results (for example, 60 mW/m2 heat flux is listed as a "finding" yet it is presented in the paper as an assumption) and the conclusion section does not contain a direct statement of the conclusions: what are the key points the authors wish to convey to the research community?

Specific Comments

Abstract L5: The statement that Elmer was used does not belong in the abstract. Better to state the assumptions of the model.

Abstract L10: 60mW is given as an assumption in the paper, not a "finding"

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Intro: The first paragraph is all about the field site, this paragraph seems like it belongs several pages into the paper, and not as the first paragraph. Instead start with the scientific questions this paper is attempting to answer. Especially emphasizing the novel concepts that this analysis presents. Similarly, the history of Dome Fuji drilling is not really necessary here or perhaps at all, unless part of the question is where to drill the next ice core. What are the most important points the read needs from this paragraph?

Section 2: I think it would be better to start this section with the model description, not a description of the domain over which the model will be applied. That should come later. For example, explain the model first, independent of field site, then describe the details of the field site and the mesh necessary to model the domain and the specific assumptions about climate, geothermal flux, etc.

Section 2.2.2: Boundary Conditions - this is a very short paragraph for potentially important information. They authors need to justify their no slip boundary condition, especially since they have significant basal melt. low basal shear stress in this region can alter the internal stress distribution within the ice sheet and therefore is important to any fabric evolution. See for example Pettit and others 2002. Also the assumption of uniform geothermal flux needs to be discussed more.

Section 2.3 CAFFE model: It would be helpful to explain this model in more descriptive terms and in relation to other models capturing deformation due to anisotropy. For example, simply stating that it is based on the "deformability" which is related to the resolved shear stress on the basal plan of crystals and the orientation distribution function would provide the reader with more information. Also more description of how the Enhancement factor is derived from the deformability. But comparing this to other forms of anisotropy models is important here.

Section 3: My understanding, after looking through Seddick 2008, is that the model was initialized with isotropic ice everywhere and the velocities resulting from the isotropic

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run (or was it shallow ice approximation velocities?). This seems like a simple thing to include here. Also, how long did the model have to run before reaching steady state? What convergence criteria were used? In fabric evolution, you need at least enough time for particles to move from the surface to near the bed because the fabric depends on the stress fields experienced along the entire trajectory (otherwise you should justify why a shorter time is acceptable for developing the fabric in this situation). This means the model should run for something like 10H/bdot. More information on this aspect of the solution technique are needed. I don't think there is any particular use in comparing the solution technique to the Zwinger and others 2007 model, why not just describe the solution technique here? Is the grid fine enough to capture the aspects of flow the authors are interested in? Can they justify that this is sufficient fine?

Section 4: As I stated above, this section needs to be rewritten to describe better what the results are really suggesting and what is new, some of the results seem to be confirming what has already been found by other researchers (which is good, just need to reference those other researchers). Is the fabric really uniform from 800-2500, if so, why? And the statement that small strain rates "forbid the formation of fabric" is not true. Also this entire section needs to put in context of what other researchers have done. I cannot provide an exhaustive list but papers by Gillet-Chaulet, Martin, Godert, Gagliardini, Thorsteinsson, Pettit, and others).

Section 4: Age distribution: Although the magnitude of the age at any one point is due to anisotropy, the discussion of the age distribution (that ice is older in higher bedrock areas and younger in the deep bedrock areas) seems to be more closely related to the geothermal flux assumptions than the anisotropy. If the authors wish for this age distribution to be a robust conclusion, then they should do more systematic sensitivity analyses of the geothermal flux assumption. It is not necessary to qualify this age distribution as "surprising", I do not think it is that surprising or "contrary to intuition".

SEction 5 Conclusions: This section needs more concrete conclusions to separate it from the discussion in section 4. The discussion of the inclined layering seems to be

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again mostly a discussion of the geothermal flux assumption, perhaps this needs to be a separate paper. I also question the assumption that the inclined layering is only a function of the basal melt rate. Inclined layers in ice cores have been more to folding as well (perhaps anisotropy induced folding) such as Thorsteinsson and Waddington 2002 and seen in the ice core in Greenland (Alley). It seems the theory of inclined layers proposed by Seddick could be easily supported with radar in the area. Again, the discussion of why the fabric is too weak in these models needs to be improved.

Figures: Overall the figures are clearly presented and easy to read. They do not need any changes except for cases where the data within them needs to change.

Technical comments

Abstract L9: the word superposition to me implies linear superposition. Perhaps to be more clear, say "nonlinear superposition" instead.

Abstract L15: "contrary to intuition" is a subjective statement not necessary (and it does not seem like a surprising result to me at all).

Abstract L 15: Instead of smaller and larger for age, use "older" and "younger"

Page 3 l20: This is an awkward sentence about the subglacial trench, perhaps split it into two.

Page 3 L23: Can you provide the maximum slopes of the bedrock, rather than just the top and bottom elevation, that seems more interesting.

Page 5 L17: "the mass balance" is used for mass conservation equation. Although this is technically correct, since mass balance has a specific connotation in glaciology, it might be more clear to say "general conservation equation" instead of "balance equation"

Page 6 L8: The introductory sentence state the boundary condition are necessary is not needed. - Also Greve and others 2009 (in press) is cited as Greve and others

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2008 in the paper.

Page 8 L6: "Like in the study by Seddick... ". There is no reason to compare it to Seddick and others here, I'd rather see a justification for setting recrystallization processes to zero. Is that a valid assumption?

Page 8 L20: Perhaps some aspects of the IBOF could be included as an appendix? rather than referring to the PhD Thesis? There should be enough information included in this paper that someone could reconstruct the model, it seems that too many key aspects are left in the PhD thesis.

Page 8 L20-22: "Evidently" and "For the latter" are used strangely in these two sentences. They don't really work well as transitional phrases.

Page 12 L21: "older" instead of "larger" for the age.

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