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The Cryosphere
Discussions

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Interactive
Comment

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 is an interesting article in an important subject as it deals with the age vs depth relation near ice domes and may affect our interpretation of climate records in ice cores. I recommend its publication in The Cryosphere with some minor (but in my opinion important) corrections.

The article shows the application of a state of the art model (3D thermomechanically coupled full Stokes model that considers the strain-induced anisotropy) in the vicinity of Dome Fuji. The study stresses the importance of ice anisotropy and basal melting in the age vs depth relation, and shows how all the components of the stress tensor play an important role in the ice dome area.

The article is clear and it seems to be well written but I recommend it being reviewed by an English native speaker.

General comments

The model presented in the paper underestimate the strength of the ice fabric. The anisotropy is very weak as compared with any other known data and, as the authors state, with the preliminary data by Miyamoto at the Dome Fuji. My major criticism of the paper is not that the model underestimate the ice fabric but the different explanations to this fact that the authors give through the paper. The fact that the modelled fabric is weaker that the observed do not affect the results presented and on the contrary this fact can only strengthen the conclusions, as a stronger fabric will only have a stronger effect in the age vs depth relation.

The reasons given by the authors to explain the underestimation of the fabric are that: the strain rates in the area are small (page 11 lines 7-10) and that they did not consider recrystallisation (page 14 lines 1-3). The former is simply not true as *there is* a strong fabric in the area even when it is an area with small strain rates and the latter is, at least, unclear. The effect of recrystallisation is suppose to be, for polygonization, to slow down the fabric evolution for vertical compression and, for migration recrystallisation, the formation of more open fabrics at the ice base. The fact that the model do not consider recrystallisation, to the best of my knowledge, would imply that it does underestimate even more the fabric. (I detail these comments bellow, in the *specific comments*.)

Besides, the authors do not relate their results with similar studies. All the main results in the paper have been previously studied: the fact that all the components of the stress tensor are of importance in the divide (e.g., Raymond 1983), the effect of melting on age vs depth (e.g., Fahnestock 2001) and the effect of anisotropy in age vs depth (e.g., Pettit 2007). (All these references are just examples I know and not a exhaustive list of merits.) This does not reduce the importance of the paper and, on the contrary, highlights the interest of the study. This last comment is clearly evident in the paper introduction (Section 1). The authors describe the area of study and extensively the core drilling at Dome Fuji but there are no comments about the importance of their work or how does it compare with previous studies or the state-of-the-art or what is new or what is expected.

Finally, the paper relies, in my opinion, too much in Seddik 2008. As I comment below, the general reader will appreciate that some minor comments regarding the numerical methods are not in the paper, but I recommend to write the full set of equations and the outline of the numerical method in the manuscript.

Specific comments

Title

- ...with induced anisotropy... I would say something more specific as strain-induced anisotropy or flow induced anisotropy or similar. (or eliminate induced.)

Abstract

- L1 again 8220;induced anisotropy8221;
- (iii) False. Figure 9 shows a nearly isotropic to weak girdle fabric (and not a single maximum fabric). I will comment more on that later but why not call it weak fabric?

1 Introduction

- The second paragraph (page 2 L1-13) is too long and inconsequential: is it so important to know that the first drill didn't hit the bed or how many seasons were needed to drill the second one?

TCD

3, S10–S16, 2009

Interactive Comment

- I miss a State-of-the-art so the reader can know what is new and interesting in this paper .

- No comments about why do you use such a complex model. It is surprising the amount of glaciologist that still think that anisotropy is a second order effect and how many (usually not the same) than longitudinal stresses are negligible everywhere. You could explain why are you using this anisotropic, full Stokes model in this area.

2 Full-Stokes flow model with anisotropy

- You are solving for the temperatures as well. Why not call this section Governing equations and numerical model or Model or Numerical model or Equations or similar?

2.2 Dynamic/Thermodynamic model equations

- I promise I won't suggest changing all the Section titles but is not dynamic part of thermodynamic, and is not the model in this section thermomechanical anyway?

- L10-16 are a bit confusing. Why don't you include the Arrhenius definition of the rate factor in the text and spend a new paragraph explaining the different terms T', Tm...

- L11 ..no-slip conditions are assumed to prevail. Wouldn't it be much clearer and true to state that you assume no-slip condition

- L13 I would suggest to write the energy jump condition. I know there is not much mysteries there but it may help to understand how the melting is calculated.

- L15 ...horizontal temperature gradient...

TCD

3, S10–S16, 2009

2.3 CAFFE model

- Page7 L16- Page 8 L12. If I've understood right you don't use this, I really think you should eliminate this that only can confuse the reader. You use Eq 13.
- Eq. 13. I think the election of the parameter ι is worth to mention. If I understand right, you founded in Seddick 08 that it produces the best fit with a EDML ice core. $\iota = 1$ will make Eq 13 identical to Goedert 03 and Gillet-Chaulet 06 with null iteration parameter.
- Page 9 L4. Shouldn't you refer the reader to Gillet-Chaulet 06 where the reader can find all the fitted parameters of the IBOF clossure approximation fitted to GOLF or have you fitted in Seddik 08 the polynomial expansion in the IBOF closure approximation to CAFFE? If the latter is true you should mention it.

  I was looking for a while for the IBOF approximation in the Journal of Glaciology paper by Seddik et al 2008. Should you have something for the references in the text like Seddik 2008a and Seddik 2008b for your thesis and the paper?

3 Finite-Element model

- (This is merely a comment). I'm not sure if your grid is fine enough in the divide area as to capture the gradients in the viscosity due to the non-linearity of the rheology (Raymond effect). This effect is assumed to be important to an extension of a few times (3) the ice thickness around the divide (3x2500 7.5km around the divide)(e.g., Hvidverg 96). And I'm not sure neither about how the steady geometry will affect to the results. However, have you tried to compare age vs. depth at the divide with age vs. depth outside the divide area (more than 3xH)? If so, is it different the age vs depth at below the divide compared with age vs depth at the drilling location?

TCD

3, S10–S16, 2009

Interactive Comment

- Pag 9 L15 Referring the reader to a PHD Thesis for minor details is generally appreciated as most readers won't be interested in them but you really should outline your 8220;numerical solution strategy8221;.

4 Simulations for the vicinity of Dome Fuji

- One of the strengths or your model is that is thermomechanically coupled. That is very important in areas where base is at melting point. How you first select the geothermal flux is interesting (Page 10 L5-11) but you should comment how the values of the melting at the base you calculate from the full Stokes compare with those from Greve 2002. Besides, I think a figure with the values of the meting rates you calculate from your model, possibly compared with those calculated with Greve 2002, should appear in the paper.
- Page 10 L 22. 8220;very small8221; doesn't sound very scientific. The typical strain rates under an ice divide are order accumulation/H, in Dome Fuji around 1e-5 yr-1. The strain rates values you obtain at the drill are similar to those expected at the dome (accumulation/H).
- Page 11 L 7-9. I think the sentence is just wrong. First, the strain rates in Dome Fuji are not that small as compared with other well known drilling sites: Siple Dome (1e-4 yr-1) , Summit Greenland (8e-5 yr-1), Vostok (7e-6 yr-1) or Dome C (7e-6 yr-1). Second, even when low strain-rates are going to make the fabric evolution slower (Eq. 13) they will imply also that the ice is transported slower as well, and in steady-state we may still expect strong fabrics towards the base. Third, even if my second point is wrong, the fact is that in some drilling sites, where the strain-rates are comparable with those in Dome Fuji, there is evidence of strong girdle and single maximum fabrics. Fourth, even if the third point is wrong, there are evidence (Miyamoto personal communication?) of strong single maximum fabrics in Dome Fuji even when there are small strain rates in the area .

- Page 11 L 11-14. As I have said before, the eigenvalues in Figure 8 don't show a single maximum fabric. a22 is nearly 1/3 all the way to the bottom, that means that there is a vertical plane (YZ?) where the fabric is randomly oriented. Nearly all the way the values of the smallest eigenvalue are 0.28, that is a nearly isotropic fabric or, being very optimistic, a very broad girdle fabric. At the bottom a1 0.2, a2 0.33, and a3 0.45 that is a girdle fabric (e.g., Wang 2002). The typical values in other drilling sites can be found in literature, for example: EMDL core (Eisen 2007, Fig 3) the NorthGRIP (Wang 2002, Fig 2) or Siple Dome (Diprinzio 2005, Fig 3) .

- Page 11 L14-16. In all those examples I've mentioned earlier, there is very small along-ridge flow and $Dyy <<< Dxx$ and even when it is true that all them show girdle fabric it is also true that all in all of them the fabric collapse to a single maximum fabric.

5 Discussion and conclusion

- Page 14 L1-3. I found very difficult to believe that such a surely large discrepancy is due to recrystallisation. Migration recrystallisation is known to be important at the very warm base of the divide but, in the rest, we may expect only polygonization. The effect of polygonization crystal fabric is suppose to be slow down the fabric evolution (at least for vertical compression) and that, in case of its existence at Dome Fuji, would imply that your model underestimate even more the fabric. Besides, with migration recrystallisation the new crystals created near the base are oriented in a direction favourable to the macroscopic deformation, producing a weaker fabric at the ice base (Van der Veen and Whillans, 1994; De La Chapelle and others, 1998).

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TCD

3, S10–S16, 2009

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