Answer to reviewers

"A full-Stokes ice flow model for the vicinity of Dome Fuji, Antarctica, with induced anisotropy and fabric evolution" by Seddik, Greve, Zwinger and Placidi
(*The Cryosphere Discuss.* **3**, 1-31, 2009)

Review by C. Martin

General comments

[CM1] 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 reason for the weak fabric produced by our previous simulation is that the integration time was too short. We now run all simulations over 100 ka (with very high computational cost, that's why it took us so long to come up with the revision), so that the numerical solution is very close to the steady state, and this solves the problem. In order to overcome stability issues, we had to introduce some numerical diffusion in the fabric evolution equation (22), which is explained at the end of Sect. 3. Anyway, the computed fabric (Fig. 9, black curves) now shows a monotonic transition from isotropic conditions at the surface to a strong single maximum at the base.

[CM2] 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.)

This point is now obsolete because we obtain a strong single maximum fabric which agrees pretty well with the data by Azuma et al. (1999) and Miyamoto (personal communication, 2009; paper in preparation). In the 3rd paragraph of Sect. 5 we discuss the comparison with these data.

[CM3] 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.

Our apologies for giving the impression that some of our findings are entirely new. We have added a number of references throughout Sect. 4 in order to avoid that misunderstanding.

[CM4] 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.

We have reduced the dependency of the paper on the doctoral thesis by Seddik (2008). In particular, the boundary conditions are now all written as formulas (Sect. 2.2.2), and more details are given on the numerical solution strategy (Sect. 3). Only for the derivation of the evolution equation for the second-order orientation tensor [Eq. (22)] and the IBOF closure relation (23), we leave it with references to Gillet-Chaulet (2006), Gillet-Chaulet et al. (2006) and Seddik (2008) in order to keep the paper concise.

Specific comments

[CM5] 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 "induced anisotropy"

Throughout the manuscript, we use the term "flow-induced anisotropy". However, in these two passages the expression "flow model" precedes the word "anisotropy" immediately. In order to avoid duplication of the word "flow", we'd prefer not to change "induced anisotropy" to "flow-induced anisotropy".

[CM6] (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?

The weakness problem is now obsolete (see answer to [CM1]). Rephrased to "(iii) the fabric shows a strong single maximum at Dome Fuji"

[CM7] 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?

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

We'd like to keep the 2nd paragraph of the introduction. It is actually not so long, and we think it is interesting for the reader to get some background information about the Dome Fuji drilling project. However, we have extended the introduction significantly. In addition to the Dome Fuji story, information is given about observed anisotropic fabrics in ice cores near topographic domes, in particular the Dome Fuji ice core, and about models for anisotropy in polar ice. A hierarchy of complexity for such models is given, and the CAFFE model is put into this context.

[CM8] 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.

The relevance of anisotropy is made clear in the introduction paragraph starting with "The fabric (…) of ice cores drilled near topographic domes…", where we state that strong anisotropy is typically found in such ice cores, including Dome Fuji. As for the full Stokes approach, please see the last paragraph of the introduction: "Since in the vicinity of topographic domes of ice sheets the widely used shallow ice approximation (…) is not valid, we use the full Stokes model Elmer/Ice (…) instead, which is applied to…".

[CM9] 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?

We'd like to keep the heading with the keyword "full Stokes". However, the heading of Sect. 2.2 is "Dynamic/thermodynamic model equations", thus containing the information about temperature.

[CM10] 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?

That's a matter of interpretation. "Thermodynamic" refers to the temperature, but not necessarily to the flow. In order to underline that we deal with flow and temperature, we write "dynamic/thermodynamic". This is actually quite a common term.

[CM11] 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...

OK, restructured. The Arrhenius law now appears as Eq. (4).

[CM12] L11 ..no-slip conditions are assumed to prevail. Wouldn't it be much clearer and true to state that you assume no-slip condition

Changed to "no-slip conditions are assumed".

[CM13] 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.

Done (along with the other boundary conditions, see new Sect. 2.2.2).

[CM14] L15 ...horizontal temperature gradient...

Changed.

[CM15] 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.

That's right. However, we'd like to keep the passage in order to explain where it comes from.

[CM16] Eq. 13. I think the election of the parameter \iota 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.

This is now discussed (2nd last paragraph of Sect. 2.3).

[CM17] 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?

Reference to Gillet-Chaulet (2006) added. However, the same IBOF closure approximation is also given by Gillet-Chaulet et al. (2006) and Seddik (2008). Note that Seddik et al. (2008) don't use the formulation of the CAFFE model with orientation tensors, so that no closure approximation is needed there.

[CM18] 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?

Sorry, no, we have not tried this.

[CM19] 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 "numerical solution strategy".

Done (see answer to [CM4]).

[CM20] 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.

We suppose the reviewer means Greve (2006). This study was done with the shallow ice approximation, but fully prognostic, with an Antarctic Ice Sheet evolving through several glacial/interglacial cycles until today. This method produces notable misfits in the simulated ice thickness. Further, the detailed topographic data for the vicinity of Dome Fuji were not available by then. Hence, we think that a comparison of the basal melting rates would be of little significance.

[CM21] Page 10 L 22. "very small" 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).

We did not mean "very small" in the sense of "surprisingly small", but rather "very small compared to typical values elsewhere in the ice sheet". Sentence slightly reformulated: "The very small strain rates … result from the fact that the drill site is only ~10 km away from the actual summit position."

[CM22] 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.

All these fabric issues are now obsolete; see answers to [CM1] and [CM2].

[CM23] 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).

We agree with the reviewer. The explanation was not convincing. Please see the updated discussion on migration recrystallisation at the end of the 3rd paragraph of this section.

Review by F. Parrenin

General comments

[FP1] This paper presents for the first time (to my knowledge) a 3D full-stokes simulation including anisotropy and this paper is then a good proof-of-concept that such simulation are currently possible. The model used is based on the wellknown ELMER code from CSC which is a state-of-the-art tool for such glaciological thermo-mechanical simulation. The application concerns the Dome Fuji drilling site and in particular the prospect for old ice in this area. The paper is very well written, the figures are clear, the context is well presented in the introduction. It is a pleasure to read such papers. This paper should be published in 'The cryosphere' (though after some revisions, see below).

Thanks a lot for the positive assessment!

[FP2] The authors also use the so-called CAFFE module for dealing with anisotropy. Of course it is not a complete representation of the complex behaviour of anisotropic material since it assumes a unique viscosity parameter for all component of the deformation tensor but takes into account the non-linear behaviour of the ice material. This simplification might be more clearly stated and compared to other anisotropic representation used, e.g. by Gillet-Chaulet et al. or by Martin et al..

We have extended the introduction in order to put the CAFFE model into perspective. Please see the second half of the introduction, starting from "Many models have been proposed to describe the anisotropy…".

[FP3] My main issue with this paper is the results concerning the evolution of fabric at Dome Fuji that can be studied in Fig. 9. First, the fabric is only weakly disorientated: it is only comprised between 0.2 and 0.5, even at the base. It is not what is observed in many ice cores and to my knowledge not what simulates other anisotropic models. Second, this fabric does not evolve between 500 m and 2500 m depth, though Dxx and Dzz have constant values on this depth interval. I could not imagine how this can physically happen and suppose it could be a numerical bug in the simulation. This is for me a blocking issue before the paper can be published. The authors should correct the bug if there is one or provide a robust explanation otherwise.

Yes, there was a bug, namely the above-mentioned too short integration time. See answers to [CM1] and [CM2].

[FP4] In my opinion the Dome Fuji application is not 'over-sold' in the sense that the authors clearly state that the results are only their model's simulations and some ways to improve the simulations are proposed in the last section. Nevertheless, the assumptions used and how they could affect the results are not discussed in sufficient details. Also, the comparison with available data is not properly made.

(a) For example, what about the steady assumption? Do we have evidences for dome movements during the past? Or do we have good reasons to assume the dome has not moved?

We have added more information on the steady state assumption in the first paragraph of Sect. 5.

(b) Assuming the surface steady, the authors obtain a vertical velocity at surface which is equal to the surface accumulation rate. It would be worth showing a map of this calculated surface accumulation rate and comparing it to measured surface accumulation in the Dome Fuji area.

We have decided not to do this for reasons of conciseness. However, the new Fig. 11 (simulated vs. measured temperature profile at Dome Fuji) also demonstrates that we get the vertical velocity about right. Otherwise the profiles would not agree so well due to the influence of vertical advection.

(c) Same for geothermal heat flux: what is the spatial scale of variability of this parameter? Or at least what do we know about it?

In the first two paragraphs of Sect. 5 the possibility of a non-constant geothermal heat flux is addressed.

(d) The authors assume SIA0 velocities at the lateral boundary conditions. How does this assumption affect the results, in particular in the Dome Fuji area? I suppose the influence is weak but a sensibility experiment with different velocities at these boundaries would remove my last doubts.

Actually, we do not use SIA velocities, but SIA stresses. Since we are ~100 km (~30 ice thicknesses) away from the summit, this should be very well justified [short remark added before Eq. (13)], and it is the only reasonable boundary condition we can think of. During the first author's PhD thesis, we tried some other, more unphysical boundary conditions, but this always ended up in numerical troubles.

(e) The comparison with the fabric data is only briefly mentioned in section 5. This would deserved a more complete comparison, since this manuscript is very anisotropy-orientated!

In the 3rd paragraph of Sect. 5 we discuss the comparison with the fabric data by Azuma et al. (1999) [not considered previously] and Miyamoto (personal communication, 2009; paper in preparation).

(f) The temperature or inclinations profiles measured by the ice-cores scientists could also be compared to your simulations

We have added a figure that shows the simulated and measured temperature profiles for Dome Fuji (new Fig. 11). It is stated in the corresponding discussion in Sect. 4.1 that the agreement is very good.

(g) In conclusion, this manuscript would deserve a longer discussion section, with proper referencing to existing studies. This section should be separated from the conclusion section.

Section 5 is now simply called "Discussion". However, we feel that a separate conclusion section would be unnecessarily duplicative, so that we'd prefer to leave it with that.

[FP5] The un-steady parameter which has the most important influence on ice age (at least for the upper part of the ice sheet) is the surface accumulation rate. Parrenin et al. (J. Glaciol., 2006) showed that assuming the accumulation and melting rates variations can be separated in a spatial term and a temporal term, the un-steady age can be simply deduced from the steady age by a change of the time variable. This is a trick that could be used in this paper to improve the age simulations, by e.g. using the climate variations obtain from the measured isotopic content of the ice at Dome Fuji. (N.B.: in Parrenin et al. (2006), the velocity profiles are assumed spatially constant but this assumption is not necessary, see e.g. Parrenin and Hindmarsh (J. Glaciol., 2007))

This is an interesting suggestion. However, we'd like to leave it with the uncorrected steady state approach. Our goal is not to provide absolute age values with optimum accuracy, but to get some information about the distribution of the age in the modelling domain.

Minor comments

[FP6] p. 5, l. 9: why not writting a general anisotropic behavior for the ice material at this stage? The Placidi law is an approximation and that should be emphasized.

Unfortunately, there is no general expression for an anisotropic flow law from that all simplified cases can be derived. The models now discussed in the introduction (“Many models have been proposed…”) lead to different expressions, and the CAFFE model is one of them.

[FP7] p. 6, l. 6: “described” -> “describes”

Changed.

[FP8] p. 6, 4 lines from the end: “Greve et al. (2008)” -> “Greve et al. (2009)”.

All references have been updated.

[FP9] Fig. 10: That would help to distinguish the area where pressure melting point is reached.

Rather than the absolute temperature, the figure now shows the temperature relative to the pressure melting point, so that temperate areas are easily identifiable.

[FP10] Fig. 11: The fact that a better site for getting old ice might exist 35-40 km away from Dome Fuji is not discussed in the paper.

We are a bit reluctant to give a concrete recommendation for a “better” drill site. One reason is that our results are influenced by the assumption of the constant geothermal heat flux, and variabilities not accounted for may change the picture. So we’d really feel more comfortable leaving it with the discussion of the trends shown in Fig. 13.

Review by E. Pettit

General comments

[EP1] 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.

As for the dependency on Hakime Seddik’s PhD thesis (2008), please see the answer to [CM4]. Placidi and others (now 2010) has been published meanwhile.

[EP2] 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.

See answer to [FP2].

[EP3] 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.

See answers to [FP4a,c]. The neglected recrystallisation is discussed in the 3rd paragraph of Sect. 5.

[EP4] 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.

Now obsolete; see answers to [CM1] and [CM2].

[EP5] 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.

Now obsolete; see answers to [CM1] and [CM2].

[EP6] 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?

We have reformulated the issue with the 60 mW/m2 heat flux in the abstract. It should now be clear that it is an assumption rather than a finding. Except for that, the items labelled (i)-(iv) in the abstract are all model results.

Specific Comments

[EP7] Abstract L5: The statement that Elmer was used does not belong in the abstract. Better to state the assumptions of the model.

We'd like to keep the reference to Elmer in the abstract. It is quite common the mention the name of an established model in an abstract (or even in a title), and the given information that the model "solves the full Stokes equations" is useful for the reader.

[EP8] Abstract L10: 60mW is given as an assumption in the paper, not a "finding"

Reformulated to "for an assumed geothermal heat flux of 60 mW/m2…".

[EP9] 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?

We'd like to keep the Dome Fuji story in the paper because we think it is interesting for the reader to get this background information. However, we have extended the introduction significantly (see answer to [CM7]).

[EP10] 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.

One reason for starting with Sect. 2.1 “Coordinate system and domain” is to introduce the coordinate system that is required for the field equations and boundary conditions. Thus we’d prefer to keep the structure.

[EP11] 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.

We have extended this section. The boundary conditions are now all written as formulas.

[EP12] 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.

In the introduction (starting from “Many models have been proposed to describe the anisotropy…”) the CAFFE model is now put into the perspective of other anisotropy models (see also answer to [FP2]). However, given the fact that the CAFFE model is described in all detail by Placidi et al. (2010), in the current paper we’d like to leave it with the rather compact description of Sect. 2.3.

[EP13] 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 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?

Please see the new paragraph starting with “The numerical solution technique…”.

[EP14] 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?

Again, please see the new paragraph starting with “The numerical solution technique…”. As for the grid, in the 2^{nd} paragraph it is mentioned that the resolution is 0.5 km in the centre of the domain. This is about 1/6 of the local ice thickness and should be sufficient to capture the local flow regime.

[EP15] 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).

See answers to [CM1], [CM2] and [CM3].

[EP16] 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".

We have removed wordings like “surprising” and “contrary to intuition”, and simply state the fact that the basal age tends to decrease with increasing ice thickness (see the last paragraph of Sect. 4.1). As for the impact of the geothermal heat flux on the age distribution, we mention the problem in Sect. 5. A systematic parameter study is unfortunately not possible because computing times would be prohibitive.

[EP17] 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 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.

We have added folding as a possible explanation for the inclined layers. The issue of the too weak fabric is now obsolete; see answers to [CM1] and [CM2]. As for discussion vs. conclusion, see the answer to [FP4g].

[EP18] 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.

Done.

Technical comments

[EP19] Abstract L9: the word superposition to me implies linear superposition. Perhaps to be more clear, say "nonlinear superposition" instead.

It is a linear superposition, because the flow regime, expressed by the strain-rate tensor $\mathbf{D}$, can be expressed as the sum over its components, $\mathbf{D} = \Sigma_{ij}(D_{ij}\, \mathbf{e}_i\, \mathbf{e}_j)$.

[EP20] Abstract L15: "contrary to intuition" is a subjective statement not necessary (and it does not seem like a surprising result to me at all).

Well, talk to some ice core practitioners… But OK, we have deleted the statement.

[EP21] Abstract L 15: Instead of smaller and larger for age, use "older" and "younger"

We don’t agree. In our opinion, age cannot be old or young (ice can be, but that’s not our wording).

[EP22] Page 3 l20: This is an awkward sentence about the subglacial trench, perhaps split it into two.

Done.

[EP23] Page 3 L23: Can you provide the maximum slopes of the bedrock, rather than just the top and bottom elevation, that seems more interesting.

It is difficult to provide a meaningful number because the maximum slope depends on the spatial scale (how long is the coast of Norway…). But Fig. 3 gives an idea. We’d like to leave it with that.

[EP24] 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"

“Mass balance” changed to “conservation of mass”.

[EP25] 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 *2008* in the paper.

We have deleted the introductory sentence "In order to provide a closed system…", and updated all references.

[EP26] 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?

The reference to the earlier study by Seddik et al. (2008) is not meant to be a justification. We merely want to say that the assumption was made before.

[EP27] 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.

Please see answer to [CM4].

[EP28] 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.

We have reformulated the two sentences.

[EP29] Page 12 L21: "older" instead of "larger" for the age.

"Larger ages" is also used frequently in the scientific literature. We prefer this expression compared to "older ages".