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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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General comments

1. 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 well-known 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 intro-

duction. It is a pleasure to read such papers. This paper should be published in 'The cryosphere' (though after some revisions, see below).

2. 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..

3. 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 D_{xx} and D_{zz} 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.

4. 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?

(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.

(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?

(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.

(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!

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

(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.

5. 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))

Minor comments

1. 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.
2. p. 6, l. 6: "described" → "describes'
3. p. 6, 4 lines from the end: "Greve et al. (2008)" → "Greve et al. (2009)".
4. Fig. 10: That would help to distinguish the area where pressure melting point is reached.
5. 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.

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