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Interactive comment on “Effects of nonlinear rheology, temperature and anisotropy on the relationship between age and depth at ice divides” by C. Martín and G. H. Gudmundsson

Anonymous Referee #2

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The present manuscript is a study presenting the effects of the ice anisotropy on the age-depth distribution at ice divides. The model employs a full-Stokes approach for modeling the ice flow coupled with an ice fabric evolution model and an anisotropic law for the constitutive equation for ice deformation. The paper is an extension of the work presented in Martin et al. (2009b) with the added temperature as field variable. The manuscript is well written with clearly constructed figures. This work provides interesting results notably by mean of the comparison of the model results to available analytical approximations used to infer the vertical velocity and age at ice divides.

Although the inclusion of the temperature field in the calculation is a major new part

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of the paper in comparison to Marin et al. (2009b), the effect on the shape of the isochrones seems small which somehow reduces the impact of this new model feature. However by comparing their model with the analytical solutions of Dansgaard and Lliboutry, the authors demonstrate that those approximations have large disparities with a fully modeled system, which certainly questions their validity at ice divides. Finally, even if not fully discussed in the manuscript (see below), the choice of $\alpha = 0$ gives larger Raymond bumps.

I have two major comments:

- The authors do not really explain how they numerically manage Eq. 4a at the bottom of the domain. The authors assume a zero slip condition at the ice bed which means that the age tends to infinity there. And more generally, why don't the authors compute the basal melting rate at the ice bed? It's even less understandable why this term is omitted since the model is fully thermo-mechanically coupled now. With the melting rate taken into account, the model should then properly handle the age solution at the bottom.

- The question of the ideal location for an ice-core extraction is approached by the authors (p. 2235, l. 26 -> p.2236, l. 18; p. 2237, l. 8-14) and they conclude that divides with fully-developed fabric are ideal locations. Locations with fully-developed fabric with stiff ice in regards to deformation have naturally better chance to display an old ice but searching for the ideal location to find the oldest ice possible should also take into account the bedrock thermo-dynamical conditions and complex topographies. The Dome Fuji ice core is good example for this matter. The ice there was found to have melted at the ice-bedrock interface whereas the location of the drilling site was believed to provide a fully-developed fabric and old ice. The fabric has greatly recrystallized because of the melting point temperature reached at the ice bottom but it did not recrystallize to form a fully-developed fabric anymore. The authors should then in my view also discuss the importance of the basal bedrock thermo-dynamical conditions in finding the ideal location for ice cores and clearly say that their model

configuration (itself very simple) does not take this part into account.

Additional remarks:

- Title: I don't think that the manuscript describes the effects of nonlinear rheology on the relationship between age and depth.

- p. 2224, l. 1: "but the effects of anisotropy on ice-depth distribution have, so far, not been described." -> I think you mean here "age-depth distribution".

- p. 2225, Eq. 3a: are you sure that there is not a missing $1/(\rho c)$ coefficient to the flux divergence term on the right-hand side?

- p. 225, l. 13: are the heat conductivity and specific heat temperature-dependent or constant values? Maybe it would be better to add this information to Table 1.

- p. 2225, l. 14: "dissipation" -> "dissipation power". The relation given for $Q_{\{D\}}$ seems to be incorrect, I believe that the $1/2$ factor in front of the trace should be removed.

- p. 2226, l. 15-17: "We follow this approach and use the invariant-based closure approximation (IBOF) proposed by Gillet-Chaulet et al. (2006). As shown by Chung and Kwon (2002), the general form of the IBOF closure approximation is..." -> I think the wording here is confusing. The IBOF closure was not proposed by Gillet-Chaulet et al. (2006) but as you mentioned indirectly by Chung and Kwon (2002). So I think it's better to say that the IBOF was formulated by Chung and Kwon (2002) and quote Gillet-Chaulet et al. (2006) as one of the ice flow related applications.

- p. 2226, l. 22 -> p. 2227, l. 4: here too, the fifth order polynomials for β_i were proposed by Chung and Kwon (2002), so you should say "Following Chung and Kwon (2002) we assume that β_i are polynomials..." and then quote Gillet-Chaulet et al. for the fitting procedure.

- Eq. 6: is $\eta_{\{0\}}$ also given by Eq. 9? Because then, with $\alpha = 1$, you will still get a strain rate term in your fabric evolution equation, right?

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- p. 2231, l. 26 -> p. 2232, l. 4: the authors don't really acknowledge that for $\alpha = 0$, the model gives noticeable larger Raymond bumps. How this compare to real data, is the $\alpha = 0$ or the $\alpha = 1$ closer to reality? I think more discussion is needed for this result.

- p. 2232, l. 11-13 and Fig. 2: why is the K-Woodcock distribution very different between $\alpha = 0$ and $\alpha = 1$? If a look at the eigenvalue $a_{\{3\}}$, both configurations show a strong single maximum fabric at steady-state. But for the Woodcock K-value, the fabric developed by the strain rates is actually girdle in the bump with a single maximum at its bottom. Any reason for such differences for the Woodcock K-value?

- p. 2234, l. 1-27: in the description of the constraining procedure for β , α and n , the discussion heavily refers to an unpublished paper (Martin and Gudmundsson, 2012). It would be better to extend the discussion with a little more details and not rely on a paper that the reader can't read at this point. Also, how is the value for the term γ chosen?

- p. 2234, l. 4-8: "It can be argued that using a α value close to unity makes our model approach more consistent, since $\alpha = 1$ implies that the stress acting on the microscopic crystals and the polycrystal are identical. This is indeed one of the assumptions made in the development of the rheology model we employ (i.e. the uniform stress approximation, see Eq. 8)." -> but isn't that you could also consider similarly that the strain rates acting on the microscopic crystals and the polycrystal are identical and basically have Eq. 8 inverted? Then $\alpha = 0$ would be also similarly consistent with your flow law.

- p. 2235, l. 9-13: "In agreement with the results obtained by Hvidberg (1996)..." -> have you tried to do experiments with higher values for the geothermal heat flux? Any changes?

- p. 2236, l. 25 26: Please remove the reference to Martin and Gudmundsson (2012) because this a paper in preparation.

- p.2236, l. 26 -> p. 2237, l. 2: (2) and (3) should be removed from the summary

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because they are clearly not results from this manuscript.

- Fig. 1: "upper panel" -> "upper panels". "lower panel" -> "lower panels".
- Fig. 2: the eigenvalue should not be referred as " a_{33} " but rather λ_3 or just a_3 .
- Fig. 3: the question somehow relates on how you treat the basal conditions for the age. Why is the age not computed close to the bottom? On Fig. 4 though, there seem to be a solution for the age computed by the model at the bottom.

Interactive comment on The Cryosphere Discuss., 6, 2221, 2012.

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