Reply to the Editor

We have now considered in detail the comments from the referees and have corrected the manuscript in accordance. As the reviewers suggested, the discussion and conclusions of the paper depended on unpublished references and some important points were missing in the discussion, the choice of parameters was not clearly explained and some details were missing from the figures. We have rewritten the Discussion and Summary Sections, changed Figures 3 and 4 and incorporated most of the reviewer comments throughout the manuscript. We include below our reply to the reviewers comments in a point-by-point bases.

We believe that the revised paper responds to most of the reviewers' suggestions, and that the paper has been considerably improved by those suggestions. We would like to add that we are very grateful to all the reviewers and the editor for their comments and suggestions.

Reply to Reviewer #1

This is a clearly written manuscript on the effects of nonlinear anisotropic rheology, temperature and fabric on the age field in the vicinity of ice divides. Not all technical details are described but they are usually properly referenced (except for the inference of β). This is a follow-up of Martin et al. (JGR, 2009). The main differences with respect to this paper are:

- verification of the results with another code (ELMER-ICE). This is good to know, also it probably does not justify in itself a publication

- inclusion of the temperature field, which has an effect on rheology. Qualitatively, the shapes of the isochrones are unchanged with respect to the isothermal simulation. This should be clearly stated in both the abstract and the conclusion.

We have included the result in the conclusions. It is important to notice that we state that temperature has a strong "quantitative" effect but it does not affect the "qualitative" aspects of flow.

- comparison of modeled vertical velocity and age profiles with the analytical solutions of Johnsen and Lliboutry. It is shown that, while these analytical solutions are valid for the flank, they are not for the dome, even when adjusting the parameters of the analytical solutions. This is an important result for people using these analytical models to date the ice at a dome. For the flank, the authors should give what are the best parameters to fit their simulations (zk for Johnsen, p for Lliboutry).

Done. We have included a paragraph with the best-fit parameters in Section 3.2.

- Inclusion of both the case $\alpha = 0$ (Taylor model) and $\alpha = 1$ (static model) in the evolution of fabric. The authors show that the double Raymond bumps are wider in the case $\alpha = 0$ than in the case $\alpha = 1$. This is an important result of this manuscript and it opens the prospect of inferring α from isochrones data around divides. This result is not enough emphasized in the current manuscript. Also, the choice of α is not discussed with respect to other studies (e.g. Gillet-Chaulet et al., PhD) based on the VPSC model.

That is not strictly true, the width and amplitude of the bumps not only depend on alpha but also of the others rheological parameters, mainly n and beta. For a given value of alpha, the width and amplitude of Raymond bumps increases with n and decreases with beta. Both cases, alpha=1, beta=0.1 and alpha=0, beta=0.01, where chosen because the former is the one that better fit observations and Pimienta (1998) experiments, and the later the one used in Martin et al 2009. We agree that this was not very clear in the manuscript and we have rewritten the discussion.

Major comments:

- rewrite abstract: "we also show that divides... at the flanks" This is already discussed in Martin et al. (JGR, 2009), so I would remove this sentence. "In addition, these divides... radar data" ditto, already discussed in Martin et al. (JGR, 2009), so please remove. the fact the width of the Raymond bump depends on the α parameter is missing from the abstract, please add it.

The fact that "ice under them [ice divides] can be up to one order of magnitude older than ice at the same depth at the flanks " is, to the best of our knowledge, a new result of this paper, and certainly it was not discussed in Martin et al (2009). Regarding to the second sentence "In addition, these divides... radar data". It is true that that is not a new result but the objective of the abstract is not only to summarize the results but also to put them into context and, as in this case, to explain the significance and application of the results.

rheology: why using the static model and not the more realistic VPSC model as used by Gillet-Chaulet et al.?

According to Castelnau et al. 1996, the uniform stress model gives a good estimate of the experimental response of an anisotropic ice polycrystal, but it underestimates the anisotropy of its fabric. The main limitation of the Static model with a linear rheology was that it is impossible to reproduce the experimental results by Pimienta and others (1987). This is not true in the case of the non-linear rheology described in Section 2.3 as parameters beta=0.1 and n=3 reproduce the lab experiments obtained by Pimienta and others (1987). We have included this fact in the discussion.

- discussion, first §: the fact that β is inferred from comparison to data is only briefly mentionned here. It should be already described in the previous sections and with more detailed. There are only references to three papers. Two are submitted or in preparation and the third one (Martin et al., JGR, 2009) does not describe the choice of β, as far as I could check. I reckon γ has been chosen to 1 but this is not really discussed. So I would dedicate a entire (sub)section on the inference of α, β and γ from comparison to data.

We agree. We have rewritten the discussion to include those points.

- discussion, p. 2236, l. 6: It is not clear that the divide is really the best position for getting old ice. Is not one of the double bumps the best?

That obviously depend on the depth we are considering as the oldest ice is right at the bottom. For about 2/3 of the depth the oldest ice for a given depth is under the divide.

- rewrite summary: First sentence OK. Second sentence: the fact that "the anisotropy description is compatible with laboratory measurements of rheology" is not a result from this manuscript. This should be removed. The fact that "the variation in modeled fabric distribution with depth agrees closely with comparable ice core measurements" is not a result from this paper. This should be removed. third and fourth sentences OK. Add a sentence to tell that the α parameter influences the width of the double bumps.

We agree, we have removed the paragraph and rewritten the conclusions.

- fig. 3: for the Liboutry model, w is outside the range of possible solutions even for t=0, but this is not the case for the age. So it seems there is something wrong in the calculation of the age in at least one of the two methods.

We are only showing the results of the full system at t={1/10,2/10,...,1,2,...10}td. There is no solution at t=0. This was a bit confusing thanks to the colorbar starting on 0. We have changed the colorbar so that it starts on 0.1 td.

Minor comments:
- p. 2230, l. 5: maybe you can add a ref to Parrenin Hindmarsh (JG, 2007) where this formula is explicitely written.
Done

- results, p. 2230, l. 20-21: tell the reader that it means your total ice volume in the domain is constant through time.
Done

- results, p. 2233, l. 3-7: this paragraph is not clear and should be rewritten. "First, ..." age always increases with depth, there is no maximum in fig. 3!
Done

- fig. 1, legend: I reckon you meant "... of five time the initial ice thickness", since because ice thickness evovles with time, your x-axis range would also change with time.
Correct. Caption changed.

- fig. 2: left panel should be $\lambda 3$, not a3,3 , since it is the eigenvalue.
Done. (It is just notation, isn't it?)

- fig. 3, legend: mention that Johnsen is left and Lliboutry is right.
Done

Reply to Reviewer #2

The present manuscript is a study presenting the effects of the ice anisotropy on the age-depth distribution at ice divides. The model employes 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 Matin 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 of the paper in comparison to Matin 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 termo-mechanically coupled now. With the melting rate taken into account, the model should then properly handle the age solution at the bottom.

We agree that the steady-state age at the base tends to infinity in steady-state but all the simulations presented in the paper are transient, including the partial derivative of age with respect to time. At the frozen base, Equation (4a) simply transforms into (\partial Age / \partial t =1). Pure advection is an equation difficult to solve depending on the numerical method selected. Some of them impose artificial boundary conditions or special treatment for stagnant areas, but this is not the case of the method we use. As discussed in Section 2.5, we use a semilagrangian method to solve Equation 4a and 5a, and it doesn't require any artificial boundary condition or special treatment at the base. As mentioned in Section 2.5, we find this method numerically more stable for areas with stagnant ice than any finite element method tested. As a note a side, the solver we use for Equation (4a) is in the process of becoming an open-sourced option of Elmer/Ice.

It all the simulations presented, the temperature is below the melting point and as discussed previously there is no need to introduce artificial melting to solve Equations (4a) or (4b). Diffusion is introduced in lagrangian methods by interpolation, but it is numerically consistent as it becomes smaller as the resolution increases.

- 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 meting 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

We totally subscribe those remarks. We have included a clarification in the Discussion Section (P14 L18-22).

Additional remarks:
- Title: I don't think that the manuscript describes the effects of nonlinear rheology on the relationship between age and depth.
We disagree in this particular point. The non-linearity in the rheology of ice is one of the key ingredients of the particular relationship between age and depth under divides.

- 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".
Done

- 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?
We used the standard notation with \kappa and K for the heat capacity, where K is the heat capacity

and \kappa=K/(\rho c). We have rewritten the equation for simplicity.

- 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.
Done

- 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.
Done. (Well spotted.)

- 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.
Done.

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

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

No, the non-linear extension term in the viscosity (η_{o}) is cancel out in the term S/(2 η_{o}) in Eq. (6).

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

We agree that the selection of parameters was not clear and we have rewritten de Discussion Section.

- p. 2232, l. 11-13 and Fig. 2: why is the K-Woodcock distribution very different between α = 0 and α = 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?

We also find the result interesting. The only difference is that for alpha=1 compression/extension becomes more efficient than shearing as a fabric evolution driver. We believe that this process promotes vertical girdle over single-maximum fabric in the middle sections of the divide.

- 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 gamma chosen?

We agree. We have rewritten the discussion and the conclusions so that they do not depend on

Martin and Gudmundsson (2012) or Drews et al (2012).

- p. 2234, l. 4-8: "It can be argued that using a α value close to unity makes our model approach more consistent, since α = 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 α = 0 would be also similarly consistent with your flow law.

Equation (8) assumes uniform stress approximation. The assumption of identical strain-rates in crystals and polycrystals (Taylor approximation) will lead to a different expression (e.g., Gillet-Chaulet et al. (2005)). As discussed in Gagliardini et al (2007), due to the strong crystal anisotropy, the uniform stress model has been shown to be well adapted to describe the polycrystalline ice behaviour, whereas it is the opposite for the Taylor model .

- 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?

We have made several simulations with all the reasonable values of H, QG and \theta_s. In fact, a previous version of the manuscript had a section showing the influence of temperature on age-depth. As discussed in the paper, the qualitative effect of temperature on rate factor is very important. But didn't add anything to the conclusions of this paper or to the already known effects of temperature in ice-flow under divides as, for example, in Hvidberg 1996.

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

We have rewritten the discussion and the conclusions so that they don't rely on that paper results.

- p.2236, l. 26 -> p. 2237, l. 2: (2) and (3) should be removed from the summary because they are clearly not results from this manuscript.

Done.

- Fig. 1: "upper panel" -> "upper panels". "lower panel" -> "lower panels".
Done

- Fig. 2: the eigenvalue should not be referred as "a_{33}" but rather λ_{3} or just a_{3}.
Done.

- 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.

As mentioned earlier, there is no special treatment of the Age of ice at the base. The solution at the base of the divide was not in the figure because we had a post-processing error. (For the plot we are sorting otherwise unstructured data.)

Reply to Reviewer #3

In this paper the authors explore the important, and interesting, effects of nonlinearity, anisotropy and temperature on the flow of ice at ice divides using a sophisticated numerical model.

The figures are good, but there should be better labels on the figures themselves. Take Figure 1 for instance, time is marked on x-axis which is distance (which should be labeled, time could be in between the two rows), and values should be shown as y-labels.

We decided not including the labels in the subplots as they don't add much (assuming the reader realize they are contour plots of ice divides) and they severely reduce clarity. We want to stress that the horizontal domain is described in the caption.

However, the description of fabric is simplified and assumed, which might be restricting some of the effects of anisotropy. This should at least be discussed (see f.exs. Thorsteinsson and Waddington (2002; Annals of Glaciology)).

We agree that we use a simplified description of the fabric. We don't assume the particular values of the fabric as in our modelling approach they are induced by flow. The fabric model we use relies heavily in previous efforts (mainly Goedert 2003 and Gillet-Chaulet 2006) and nearly identical to that described in more detail in Martin et al 2009b. We believe that copying and discussing all those details wouldn't add anything to the paper. Our approach has been to summarize all the main approximations taken and reference them.

Comments
P2222
L5. “Here, we ... effects of ice flow ...”. This should be turned around a little, and state that “... effects of non-linear anisotropic ... ice flow on ...”, that is, it is the nonlinearity and anisotropy (temperature) that affect the ice flow, and thus the depth-age relation.
We slightly disagree as flow, temperature and fabric depend on each other to some extend, and ice flow defines age-depth. Our modelling approach considers flow-induced anisotropy so not only anisotropy affects flow, in addition flow affects ice fabric.

L11. Change “We also show that divides ...” to “Divides ...”

L19. Somewhat strange wording, “changes in climate fit within a long
history of ...”. Compare to previous changes in climate?

L24. “... of the ice, (e.g. ...”
Done

P2223
L19. “... model used,. They which includes ...” - as one sentence.
Sentences rewritten.

L21. “... models that than assume ...”
Done

P2224
L2. Nothing in Pettit et al. (2007; 2011), maybe not?
There are several papers studying the influence of anisotropy on age-depth, we included some of them in the introduction (P2223 L17-23). (Pettit et al. (2007; 2011) are there.) In L2 we are taking about flow-induced anisotropy and transient effects on age-depth. To the best of our knowledge, there are no previous papers on this subject.

P2227

Should discuss a bit the assumptions made for the ODF chosen, even though there is a citation.

The fabric model we use relies heavily in previous efforts (mainly Goedert 2003 and Gillet-Chaulet 2006) and nearly identical to that described in more detail in Martin et al 2009b. We believe that copying and discussing all those details wouldn't add anything to the paper. Our approach has been to summarize all the main approximations taken and reference them.

P2229

L1-6. Are the authors certain that they are not missing any effects of anisotropy with these assumptions?

Does the reviewer refer to the Anisotropic SIA as a lateral BC? Outflux BC are always problematic in CFD applications. It has been discussed previously that they only affect the model solution in an area extended a few ice thickness from the lateral boundaries to the divide (Hvidberg 1996). In particular and as referenced, the use of anisotropic SIA as a boundary conditions for full Stokes models is discussed in Gagliardini and Meyssonnier 2002. We can only add that for our particular model, using anisotropic SIA, isotropic SIA or even plug-flow velocity as lateral boundary conditions didn't affect the solution in the divide area.

P2230

L10. It would be good if the authors addressed the likelihood of recrystallization occurring at the divide. What is the temperature near the base, total strain, ...

Recrystallization is likely to happen. Its possible effects are discussed in Section 4 and more extensively in Martin et al 2009b and references therein, but our model doesn't account for it. We understand our approach as a first approximation to the fabric distribution. We believe that more data and understanding of the recrystallization processes is needed in order to incorporate them to dynamical models.

P2231.

L14. Strain responsible for fabric, unless recrystallization.

We agree but we are referring to the forcing of flow-induced anisotropy as defined in Eqs (5)-(6).

P2235

L14. These effects would move dept-age relations closer to analytical solutions?

It is difficult to know how all those effects will affect the age-depth locally but all them tend to reduce the Raymond effect or the flow-induced fabric evolution, so we expect a reduction in the double-peaked Raymond bump amplitude.

P2236

L6. See discussion in Thorsteinsson and Waddington (2002; AG).

Is the reviewer referring to “Folding in strongly anisotropic layers near ice-sheet centres” by Thorsteinsson and Waddington 2002? In all honesty we don't understand what the reviewer is referring to.

L18. Would it be possible to show a “real” example of these effects? Would be a strong move.

We showed a few examples in Martin 2009b, we have referenced then a few times during the paper (e.g., p2233 L23-24).

P2237
L1. Compared to alpha=1 or alpha=0?
We have rewritten the conclusions and that sentence has been removed.

Figures
See comment in the beginning; applies to Figure 2 also.
As commented before, we decided not including the labels in the subplots as they don't add much (assuming the reader realize they are contour plots of ice divides) and they severely reduce clarity. We want to stress that the horizontal domain is described in the caption.

Figure 2. Caption: ".. girdle and at K > 1 ..."
Done.

Figure 3. Caption: "Vertical velocity (top) and age of the ice (bottom) along ..."
Done.

Figure 4. Label subplots. Caption also unclear about which is which.
Done. We have rewritten the caption.