Referee #1

Comments from the referee are in plain text and our replies are in *italics*

GENERAL COMMENTS

This manuscript presents some very interesting results in regards to the age distribution in the interior of an ice sheet. Particularly, the focus on the influence of the ice fabric is of relevance to the scientific community. I do have some reservations and concerns that need to be addressed. Some of these might be met by clarifying why certain decisions were made for the model set-up, but there are also paragraphs where I would like the authors to elaborate in their presentation and interpretation of the results. I recommend that this manuscript is accepted with minor revisions.

SPECIFIC COMMENTS

My main concern is the question of a constant accumulation rate. The authors explain that the fixed geometry (and by that I assume they mean the surface elevation?) implicitly determines the accumulation rate. Maybe I'm missing something here but it is unclear to me why that is. Cannot the horizontal fluxes out of the domain be forced to match the surface accumulation and thus keep the surface elevation constant? Or do the authors mean something else by "fixed geometry"? Since the accumulation rate often is the key factor in determining the age distribution it seems very odd not to include this, and I would like a better explanation of why this is disregarded.

The reviewer is correct in the interpretation of the term fixed geometry. It means that we solve the Stokes equation on a domain with given and prescribed elevation and bedrock geometry. This is often referred to as steady state or diagnostic run. We acknowledge that our model has limitations in the 2nd paragraph of our Discussion section. What we are really saying is that model shows that there may well be instabilities in the Kunlun region such as basal melt/freeze-on and variable geometry which make our model assumptions wrong. That is in fact a worthwhile finding and possibly an explanatory factor for the radar observations of the basal ice.

Concerning the importance of accumulation rates, over a glacial cycle the accumulation rate may vary by roughly 50%, so crudely we may expect the thinning rates to vary similarly, and hence an age scale may be in error by perhaps 50% if the present day accumulation rate were used or the glacial maximum rate used in steady state. However what we show in this paper even excellent matches to age profile in the upper 1/3 of the ice sheet do not provide enough information for dating the lower parts of the ice sheet. That is effects such as anisotropy and basal melting rates have effects far more important than surface accumulation rate in the lower parts.

The key reason as why accumulation rate variability is disregarded is a mathematical and computational one. While we agree with the reviewer that by allowing the velocity on the sideward boundaries to adjust to a given fully imposed velocity field at the upper free surface (and that's what prescribing a surface accumulation in diagnostic runs means) in combination with a fully determined velocity field at the bedrock, then mathematically it would still be a well-posed problem. However, if we also impose sidewards velocity fields (for instance one obtained from a SIA solution) it actually would be an ill-posed problem. It, nevertheless, is impossible to impose Dirichlet conditions (i.e., prescribed velocity components) at a surface and at the same time assume to be able to fulfill also a Neumann condition (i.e., surface stress). In other words, in general the condition of a stress-free free surface does not hold if prescribing velocities at this surface while keeping the geometry fixed. Also the sidewards stress distribution will certainly take values deviating from those we would expect from a SIA solution. So, the choice between prescribing a surface accumulation or, alternatively, deduce it as part of the solution in this configuration is equivalent to opt for a physically wrong (as not existing) force acting on the free surface or to make sure that the stress-free assumption prevails. We are convinced that the latter is more important (as it is for sure that atmospheric forces do not drive ice sheets).

In some sections the manuscript is a bit too brief and I would like both the results and the discussion sections expanded. Particularly, I would like to see a longer discussion of the results presented in Fig 3. For example, when comparing the run S,I ,W,50with S,I,W,60 there is a difference of approx.10mm/yr in surface velocity. As far as I understand this figure, the only difference between the two runs is the geothermal heat flux. Why does this lead to such a big difference in surface velocity? I assume that that basal melt rate equals the vertical velocity at the base, thus the melt rate is only a few mm different in the two runs.

Another example is the difference between the runs with warm and cold surface temperatures(e.g. S,1/3,C,60 and S,1/3,W,60), that seems to be similar in size or larger than the variations induced by difference in ice fabric(e.g. S,1/3,C,60and S,2/3,C,60). Based on the results is it more important to get the ice fabric correct or the temperature? Or are they equally important? This leads back to the previous question re. a constant surface accumulation rate, since higher/lower accumulation rates significantly affects the transport of cold surface snow down in the ice column thus influencing the internal temperatures
We have simplified Fig. 3 (now Fig. 4) by replotting in several subplots. The referee is correct that it merits an extended discussion of several features and we have done so. In general we are not very sure of all the reasons why the simulations behave as they do in detail as the model takes into account the complete deformation of the ice with a Full Stokes system. However the referee should not expect surface vertical velocity to equal basal melt rate since there is horizonal thinning to take into account as well. The transient runs suggest that although warm temperatures occur for much less duration than cold ones, they strongly influence the resulting ice velocity presumably because of the nonlinear nature of the flow law.

I suggest including a table of the predicted ages for the different model runs. It is very hard to extract information from Fig 3 as it is (see also my comment below about figures).
We added basal ages to fig 4 (now Fig. 5) as a table simply does not convey much useful information on its own, the full age profile can be seen from the new Fig. 4.
Finally, I'd like to see a mention of the Fischer et al., 2013 paper (www.clim-past.net/9/2489/2013/). This study concludes that there most likely is very old ice at Dome A. A brief discussion of the difference between the findings presented by Fischer et al., and the findings presented in the manuscript here would strengthen the conclusions.
There is no dating science in Fischer et al, 2013. The age at bed used is from Van Liefferinge and Pattyn 2013 which we reference, and explain why there are differences with our modeling.

MINOR MINOR COMMENTS

Abstract

Line 21: Missing "an" in "... than 1.5 million years old is **an** active and key question..."
done

Maintext

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Lines 4-5: Is the average accumulation rate also basedonthe Hou et al, 2007 study? Theway the sentence is written is not clear if there is missing a reference here.
Jiang et al., 2012 - corrected
Lines 12-13: Remove the parenthesis. Also it should be "suggest" not "suggests"
done
Lines 15-17: There should be a sentence here explaining why thicker ice leads to higher basal temperatures. I knowthis is fairly obvious, but since the paragraph includes an explanation ofwhy basal melt leads to less of old, basal ice, it might as well also explain the influence of thick ice.

done

Lines 19-20: The reference Svensson et al., 2007 is incorrect? It is a paper about the NGRIP and GRIP ice cores. A more appropriate reference would be, for example, Durand et al.,Supp. Issue Low Temperature Science, 68, 91–106, 2009. However, I do not understand why there is a mention of anisotropy developing close to the surface at all. The model runs only include anisotropy at depth. Another possible reference could be the Fujita et al., JGR,1999 paper where anisotropy was directly observed from radar at Dome F.

Thank you, its true the suggested references are better and replace the previous ones

Lines 28-29: Ice cores in Greenland do not necessarily exhibit a single maximum fabric. It is the case for the GRIP core because it is mainly influenced by vertical compression. In the NorthGRIP and NEEM ice cores a girdle pattern has been observed. One could argue that Dome A is most likely dominated by vertical compression since it is a true dome. There is a paper in review in "The Cryosphere Discussions"on the ice fabric of Greenland ice cores that might be helpful: Fabric measurement along the NEEM ice core, Greenland, and comparison with GRIP and NGRIP ice cores, M. Montagnatet al., 2014.

Thanks, very helpful and the text is rephrased accordingly

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Lines 20-25: how dense is the radar dataset? Is the choice of ~300m horizontal resolution based on the constraints imposed by our knowledge of the bed (due to radar data coverage) or is it based on model considerations?

The radar lines used to construct the DEMs are now shown on Fig 1B. The model resolution is comparable to the line density as may be seen by comparing Fig. 1B and D.

Lines 24-25: It is unclear to me what is meant by "... a zero-flux condition is applied to the temperature field..."Does this mean that there is no heat flux into or out of the domain?

Yes, that is meant. In order to avoid misunderstandings we reformulate: … a zero heat-flux (or adiabatic) condition is applied to the heat transfer equation …

Line 26:In glaciology the term "hydrostatic approximation" is not commonly used and it would be kind to readers to remind them what this entails. I assume that the authors mean that acceleration terms are disregarded, but perhaps more stress terms are included than in the shallow ice approximation? A better explanation or at least a reference to an appropriate study would be useful here.

In fact, "hydrostatic approximation" is used in theoretical glaciology (see, e.g. chapter 5.2 in Greve and Blatter 2009). It means the reduction of the vertical conservation of linear momentum resulting in the hydrostatic pressure distribution. Acceleration terms are a-priori neglected, as we solve the Stokes and not the Navier-Stokes equations with the earlier being the approximation to the latter for very small Froude number.

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Lines 1-4:I highly recommend making a figure to illustrate this. It would make it much easier to understand.

We have added a new figure to illustrate the fabrics used (Fig. 2)

Lines 7-10: this sentence is confusing to me. I guess it just needs clarification that assuming the fabric at Dome A is similar to that at Dome F, the ice fabric will be isotropic in the upper parts of the column and therefore the age of the ice (in this part) can be calculated using thinning rates for isotropic ice.

yes, the wording is simplified

Line 22: what is meant by a steady state velocity profile? Do the horizontal velocities equal the balance velocities?

By steady state profile we mean the one obtained with the previously done steady state run (with varying prescribed fabrics as well as thermal boundary conditions). We reformulate to: ... and using the previously obtained steady state velocity profile … . The balance velocity, as we *understand its definition, would be the vertically integrated horizontal velocity field (e.g., Bamber, Hardy and Jouhgin, 2000) of this solution – mind that a full Stokes code like ours really resolves the whole ice column and doesn't operate on averaged fluxes.*

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Lines 10-11: This line seems slightly at odds with the figure caption for Fig. 4 where it says that simplistic expectations suggest a surface velocity of 14mm/yr(why?). A few lines explaining the distribution of RMS for different ice fabrics would be valuable.

Both the text and the figure caption say the best expected velocity is 14 mm/yr (the units are ice equivalent). We added a sentence on the fabrics: "The RMS errors in Fig. 4 are larger for both isotropic and single maximum fabrics than for the depth evolving girdle cases, as we may expect from observations on ice cores."

FIGURES

Fig. 1: it is very difficult to see which numbers correspond to which colour on the colourbars. Also the colourbars should have a value at the end of the scale. InFigure1c the contours are almost invisible. If possible I'd prefer a larger figure of the bed topography, while Figure 1a with the location map could easily be smaller.

We added the radar lines (flights and ground) to fig 1B. We do not agree with the other points here, the contours and the color bar are easy to see by zooming. Its an on-line document not a printed journal so we don't see that making panels bigger or smaller is relevant at all.

Fig. 2: I have the same comment re. the colourbar as in Fig. 1. I assume the (almost invisible) white contourlines outline areas of temperatures above the pressure melting? If that is the case please make this clear in the figure caption.

New Fig. 3 We remade this because of comments from ref #2. The contours and the color bar are easy to see by zooming. Its an on-line document not a printed journal so we don't see that making panels bigger or smaller is relevant at all. The figure caption already states: "The temperate areas at the bedrock are surrounded by a white contour." We change temperate for "pressure-melting point"

Fig. 3: It is too difficult to get information out of this figure. The colours of the lines are not different enough to make it easy to distinguish them and thereader has to study the caption in great detail to make any sense of what the lines mean. I suggest either splitting it up into several figures or plotting fewer lines. Also, it is a bit confusing that the y-axes go to 3500mbut the lines do not.

New Fig. 4 has been split in several subplots which we think are easier for most readers to grasp. Since the results obtained are far from intuitive or "prescribed" by the boundary conditions we want to show the full range of simulations we have done to illustrate that inferring behaviour from a limited exploration of parameter space is likely to result in error. We used the actual ice depth as limits in the new plot.

Fig. 4: Would it be possible to colour code the red dots also, so readers can see what the contour lines are based on?

(New Fig. 5)The contour lines are based on the misfits at the points shown, they define the contours. It is not easily possible to colour the points separately as that is actually what the contours do, so we changed the red dots to black ones and added the RMS mismatch at each point. We also added the basal ages for each point in the plot..

Fig. 5:It is unclear what the "best fit" criteria are. Best fit in terms of the radar isochrones? But it was just stated in the text that this was not enough to constrain the age at depth. I am not keen on this figure and would suggest that it is not used, and that Fig. 6 is used instead. If the authors prefer to have Fig. 5included then please mark the location of Kunlun station on this transect, and it would be nice if the radar data could be made a bit clearer.

The figure is deleted as requested

Fig. 6: Same comment re. the colourbar as Fig. 1 and 2.
The contours and the color bar are easy to see by zooming. Its an on-line document not a printed journal so we don't see that making panels bigger or smaller is relevant at all

References

Greve, R. and H. Blatter, 2009. Dynamics of Ice Sheets and Glaciers, Springer, Berlin, Germany etc.

Bamber J., R.J. Hardy and I. Joughin, 2000. An analysis of balance velocities over the Greenland ice sheet and comparison with synthetic aperture radar interferometry. J. Glaciol., 46, 67-74