Referee #2

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

Referee #2

Overall, the paper as it stands is interesting, but it is not clear as to what the authors actually did with respect to their assumptions about ice flow. The ice flow section needs to be expanded so that the work is at the very least repeatable. There is a large interest in oldest ice, so the work presented here is relevant to the community at large.

The boundary conditions affect the results, and the model formulation, in general, is unclear. This is highly problematical. With a fixed surface condition, that might mean that the authors also use a fixed surface gradient. It is unclear what the boundary conditions the authors use, and these need to be stated explicitly. With no slip at the base of the ice sheet and the surface elevation and surface gradients fixed, the horizontal fluxes depend critically on the preceding parameters in Glen's flow law–-temperature and fabric principally. The authors adjust surface temperature, but not surface accumulation (or so it reads, again, this is unclear), while using two different geothermal heat flux values over a wide area. With the heat flux through the ice constrained by the bed and the surface, then the only adjustment to the flux out the edges of the domain is the crystal fabric. This is what the authors find, but the result itself is predetermined by the model set up. This assumes that I have read the boundary conditions correctly, which are inadequately discussed. If this is the case, then the results are only robust inasmuch as they depend on the assumptions. Again, a little work ahead of time with paper and pencil would have shown that their results are predetermined.

The reviewer made a clear analysis of the constant dilemma a modeler is confronted with: One has to introduce assumptions in order to compensate for lacking data and model abilities. And, indeed, the results are to be interpreted in view of these assumptions. We clearly state these assumptions within the text, such that a scientist capable of understanding the principle laws of continuum mechanics should be able to follow them and make this judgement. Regarding model abilities, the deployed code Elmer/Ice is a state-of-the-art implementation that at least overcomes the issue of correctly describing the flow dynamics under a dome of an ice sheet. The prescribed fabrics are either (deduced from existing cores) valuable approximations or extreme cases (isotropic, completely vertically aligned fabric) that should give a qualitative maximum range for the results obtained within this study.

The Stokes equation gives an instant and unique velocity/pressure field for a given shape and viscosity distribution, the latter being determined by the temperature and fabric distribution. If the reviewer refers to this as being predetermined, then he is correct – but this applies to any solution of the Stokes equation. As for the last sentence, we feel a reply in the spirit of the proffered advice is due. The reason why we did not deploy the pen and paper method is that, obviously by the extreme complexity of the bedrock geometry, we see numerics as the only way to solve the flow problem on this three-dimensional domain. If the reviewer found a general analytic solution for the Stokes problem on arbitrary geometries with arbitrary boundary conditions (or a sufficiently good approximation to it), he should bring it forward as this would be a major breakthrough not only in theoretical Glaciology, but also in Mathematics in general.

Does that mean that the paper is unpublishable? I think that the paper is publishable with some cleaning, but the model and its boundary conditions need to be clearly articulated. There will also need to be caveats about the surface conditions stated explicitly in the paper.

We have added basic model equations and Table 1 which lists the boundary and surface conditions. We also discuss the simulation time and age limitations as is raised in the figure comments

The text itself requires some basic fixes.
The figures are problematical and will require more work.

Title: The title as it stands is problematical and does not reflect the content of the paper. Also the use of question formats as titles detract from the body of work.
We think the title exactly matches the study and – also in lack of an alternative title offered by the reviewer – stick with what we wrote.

Introduction:

P291, L24: Nucleation and oldest ice are two different things. Nucleation occurs 34 million years ago (Creyts et al, submitted; Rose et al., 2013), but oldest ice is really only from 1.5 million years ago (Severinghaus et al, 2010; Fischer et al. 2013). The wording about nucleation is irrelevant and detracts from the message. The wording about nucleation should be eliminated. There are also areas that do not act as nucleation centers that likely have oldest ice, so that distinction is not relevant here.
*We disagree. Clearly the oldest ice that is hypothetically possible is where the ice sheet nucelated earliest. This is discussed in the references we cite. We are unclear how we should know about a merely submitted paper (*Creyts et al, submitted)*, and in any case such works are of course unciteable in reputable journals.*

P291, L9: It is in no way clear how complex ice flow is near the dome, presumably it is relatively simple. I was surprised that the authors do not address Raymond bumps in their flow model and introduction.
What is the relevance of Raymond bumps? Raymond bumps are not supposed to develop into something visible, because of the large time scales involved (see Martin et al); the characteristic time scale being a/H. So even if the Raymond effect is at work, it would not be marked by any up warping of isochrones.

P291, L13: Check for the parameters in Fischer et al (2013). They look at modest ice thickness, low accumulation rate, and low geothermal heat flux. Please add a statement about accumulation rate here.
The age at bed used in Fischer et al comes directly from Van Liefferinge and Pattyn 2013 which is cited and discussed.
P291, L8: The reference here should be to Rose et al (2013) or Creyts et al (submitted). The Bell et al paper does no analysis on the land surface.
We disagree. The Bell paper shows bed radar profiles, how is that not discussing the land surface? They show a large map of the bedrock topography in their Fig. 1. There are several relevant quotes e.g. " along the steep valley walls of the Gamburtsev Mountains."
P291, L10: The melting and freezing are not localized but occur over the range. Replace "local" with regional. Both the Creyts et al. (submitted) and Wolovick et al (2013) papers deal with the locations of the water system and the regional connections. For the most part, there appears to be distributed melting in the deep valleys consistent with what the authors propose
We disagree. By local we mean that there are disconnected places where melt and freeze-on appears. Replacing local with regional destroys any idea that such regions are in relatively small parts of the whole region of the Gamburtsev Mountains

P292, L2: Why is Dome F an analogue? Please explain with an additional sentence.
Done

Data and model:

P292, L15: … "basal sliding is ***ignored.***" The ice sheet does not care what specific parameterizations are. Ice slips along the bed for various reasons and saying that it is "not allowed" imposes some moral framework on the ice sheet.
It is clearly stated how the model is set up it has nothing to do with the ice sheet moral status The same approach of no-slip conditions has been taken in simulations of Dome F (Seddik et al., 2011), published in this very journal. We remain on a purely scientific level of argumentation and hence refuse to go into a discussion about the moral status of an ice-sheet. If the reviewer is able to come up with a constructive suggestion on how to correctly (!!) implement sliding without knowing the bedrock conditions, we are pleased to take this into account. Else, we remain with what we have implemented, as we are convinced that this is the better choice in view of missing data.
P292, L18: Are the authors actually using the Bedmap2 product here at 1km resolution? If so, the Fretwell et al. paper needs to be cited.
No we do not use Bedmap2. The data used are as discussed in the paper and now shown in Fig 1B.
P293, L11: Again, Bell et al (2011) only show localized melt in radargrams. Locations of water bodies are discussed by Creyts et al (submitted) and Wolovick et al (2013). The reference should be changed here.
Simply not true, Bell Fig 1 shows melting and freeze-on. We specifically refer to depths of these basal layers which can be seen in Bell et al Fig. 2.
P293, L16–L20: What is the surface temperature parameterization, exactly? This needs to be in the text, preferably as an equation.
There is no parametrization of the temperature applied. We simply used the average present day value of -58.5 Celsius as a Dirichlet condition and for the time-dependent evolution (which is described in Table 1) a period of surface temperature 10 degree colder than this value.
P294, L1–4: The simulations appear to have constant fabrics. Are you saying the fabrics are forced to be constant with no time evolution? If so, state this clearly here.
Yes. fabric is time-invariant, we added that point explicitly.
P294, L7: Sentence beginning with "Therefore" makes no sense here. It possibly makes no sense anywhere in the paper.
Luckily the other referee made a helpful suggestion how this sentence could be improved.
P295, L11: The geothermal heat flux pins the steady state thermal profile when the heat flux out through the ice matches the geothermal heat flux. The first statement is therefore irrelevant here.
The steady state temperature profile does depend on geothermal heat flux. But not very strongly. Are you claiming that geothermal heat flux does not affect ice temperature profile. The comment is bizarre.
P295, L17: Figure 3 is not intuitive because it is a mess of lines that are hard to read. If some physical process is implied, the reader would only understand that after careful study. The lines need to be broken out into sub figures or, alternatively, eliminate the lines that are not immediately relevant to the paper. Most papers that show modeling results never show all of the results that went into the paper.
Fig 3 (now 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.

Discussion:

P296, L2: Earlier in the paper, show how velocity depends on fabric and temperature in an equation. Here, the paragraph starts with general concepts, but it is not immediately clear what the parameterization is in either Glen's Law or the resultant vertical velocity equation.
should we include equations that are in other papers?
P296, L20: That Dome A is a dynamic region is an important result from the model and should be included in the abstract.
In the abstract we say what we think we can,: " Despite expectations of modest changes in surface height over a glacial cycle at Dome A, even small variations in the evolution of surface conditions cause large variation in basal conditions" we cannot quantify how dynamic at this stage.
P296, L25: This paragraph is speculation and detracts from the main message of the paper.
This paragraph explains some of the implications of the paper. We think it is of interest to readers.
P297, L15: There are no immediate accretion features near Dome A as mapped in by Bell et al., 2011. There is water very close to drill site as indicated by Wolovick et al., 2013.
Thanks for the information, we have much radar data than was used in the Bell and Wolovivk papers in the Kunlun region, Fig. 1B plots the radar lines.

References:

R.E. Bell, F. Ferraccioli, T.T. Creyts, D. Braaten, H. Corr, I. Das, D. Damaske, N. Frearson, T. Jordan, K. Rose, M. Studinger, and M. Wolovick (2011). Widespread, persistent thickening of the East Antarctic Ice Sheet by freezing from the base. Science, 331 (6024) 1592–1595. doi:10.1126/science.1200109.

T.T. Creyts, F. Ferraccioli, R.E. Bell, M. Wolovick, H. Corr, K.C. Rose, N. Frearson, D. Damaske, T. Jordan, D. Braaten, and C. Finn. *Submitted*. Long-term preservation of subglacial mountains by freezing under thin ice and loss of basal water. Nature Geoscience.

H. Fischer, J. Severinghaus, E. Brooke, E. Wolff, M. Albert, O. Alemany, R. Arthern, C. Bentley, D. Blankenship, J. Chappellaz, T. Creyts, D. Dahl-Jensen, M. Dinn, M. Frezzotti, S. Fujita, H. Gallee, R. Hindmarsh, D. Hudspeth, G. Jugie, K. Kawamura, V. Lipenkov, H. Miller, R. Mulvaney, F. Pattyn, C. Ritz, J. Schwander, D. Steinhage, T. van Ommen, and F. Wilhelms. 2013. Where to find 1.5 million year old ice for the IPICS "Oldest Ice" ice core. Climate of the Past, 9, 2489–2505, doi:10.5194/cp-9-2489-2013.

P. Fretwell, et al., The Cryosphere 7, 375 (2013).

K.C. Rose, F. Ferraccioli, S.S.R. Jamieson, R.E. Bell, H. Corr, T.T. Creyts, D. Braaten, T.A. Jordan, P. Fretwell, D. Damaske. 2013. Early East Antarctic Ice Sheet growth recorded in the landscape of the Gamburtsev Subglacial Mountains. Earth and Planetary Science Letters. 375, 1–12. http://dx.doi.org/10.1016/j.epsl.2013.03.053.

M.J. Wolovick, R.E. Bell, T.T. Creyts, and N. Frearson. 2013. Identification and Control of Subglacial Water Networks Under Dome A. J. Geophys. Res. F (Surface Processes) 118, 1–15, doi:10.1002/2012JF002555.

Figures:

Figure 2: The oblique view is confusing. Change the two panels to four panels. Basal temperatures should be oriented like Figure 1c for comparison. The thermal profiles can either go above or below the temperature plots. It is ***very*** difficult to read the temperature plots as-is.

As the figure has been annotated with coordinates, we not completely can agree to the argument of the reviewer. As this is partly also a matter of taste, we do not argue and will redo the figure as suggested in the same style as Fig.1 , which- by consistency - admittedly has its advantages.

Figure 3: This figure is extremely problematical. It is not clear to the reader what exactly is going on, and the legend does not help. The figure would benefit from simplification, either by adding panels and separating the lines into groups or by removing lines form the two plots as-is.

Now Fig. 4 is completely remade into several subplots which we hope is easier to grasp.

Figure 4: This is confusing. Which dots are a mismatch to what? Should the dots have colors that correspond to the contours? It looks like the ages are in kyr and the Also **simple** expectations in place of simplistic expectations.

We have remade this figure and caption However the basic information is as before. The caption says " mismatch in ages (red dots) of the radar isochrones for various fabrics". That is what the mismatch is between. Ages are indeed in kyr. "Simplistic" is the correct adjective. Definition: "treating complex issues and problems as if they were much simpler than they really are". It is what we intend to say and thats why we use it. In addition to the contour lines we also show the actual RMS mismatch for every point in the graph, and the basal age for that simulation.

Figure 5: The radar section should be removed from the results and set to the side. It is also unclear why the grid does not match the bd. I look at the radargram and see the bed well above the model bed on the right hand side.

The figure is deleted as ref #1 wanted

Figure 6: Between Figure 5 and Figure 6, it reads as if the model was run for only 1.5 million years. Is that the case?

yes

If so, the oldest ice would have to be about 1.5 million years, and that would likely be problematical for the results. Essentially, the oldest ice is predetermined by the length of the simulations themselves.

This conclusion applies only for basal no-melt scenarios, for which in the vicinity of the bedrock no steady state solution of the age equation exists. .In case of widespread basal melting a steady state (locally) may exist. In our simulation we have a clear mixture of these. Clearly we will not get the actual age of the oldest ice, but we will see where the ice is younger than the maximum run time of the model.

In rereading section 3, I do not see this information in the body of the text. Could the authors please clarify the length of their simulations and what is going in terms of the semilagrangian scheme? If there is too much melt at long time simulations, then this needs to be stated, too.

We add explicitly the computational limit.

References

Seddik H., R. Greve, T. Zwinger and L. Placidi, 2011. A full-Stokes ice flow model for the vicinity of Dome Fuji, Antarctica, with induced anisotropy and fabric evolution, The Cryosphere, 5, 495-508, doi:10.5194/tc-5-495-2011