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.

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.

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.

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.

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.

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.

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.

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

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

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.

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.

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.

P293, L16–L20: What is the surface temperature parameterization, exactly? This needs to be in the text, preferably as an equation.

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.

P294, L7: Sentence beginning with "Therefore" makes no sense here. It possibly makes no sense anywhere in the paper.

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.

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.

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.

P296, L20: That Dome A is a dynamic region is an important result from the model and should be included in the abstract.

P296, L25: This paragraph is speculation and detracts from the main message of the paper.

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.

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.

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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.

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.

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.

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.

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? 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. 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 semi-lagrangian scheme? If there is too much melt at long time simulations, then this needs to be stated, too.