Anonymous Referee #1 (response)

General:
This paper uses two types of models to estimate regions of basal temperatures well below the melt point over Antarctica, one of the conditions for potential drill sites to find million-year old ice. The first model is a vertical column solution of the temperature equation, and the second extends the 3-D modeling of Pattyn (2010) that is influenced by existing ice-core temperature data and locations of subglacial lakes. The results of the two approaches are remarkably consistent (Figs. 4 and 6), with the main finding that many potential locations exist in the vicinity of several existing deep cores, but a little away from them where bedrock is shallower and thus the base is cooler. The combination of techniques is novel and interesting, and overall the approach seems viable and useful as an early scouting report for these sites. The topic is timely and of widespread interest. The paper is clear and well organized, and the results are portrayed well in various types of graphs.

Specific points:

1. The paper is clearly complementary to Fischer et al., CPD, 9, 2771-2815, in the same IPICS special issue, on much the same subject. As is probably the authors' intention, this paper should be acknowledged, perhaps briefly mentioning the differences in approaches and how each one complements the other.
We didn't see it the way the referee points it out. It is in fact the other way around. The Fisher et al. paper is a résumé of the IPICS workshop and refers to the current state of the literature. It is also based on a completely different analysis using a simple model approach at fine scale for detecting suitable sites near Dome C. We could have incorporated the older work based on Pattyn 2010 in the Fischer paper, but to keep it updated, we explicitly added the more recent work described in the Van Liefferinge and Pattyn paper. So, in fact it is the Fisher et al. paper that refers to this one and not the other way around.

2. Apart from the modeled basal temperatures and rms variability, the other two criteria in the paper seem somewhat arbitrary: ice velocity < 2 m/yr and ice thickness > 2000 m. Although these are reasonable qualitatively, why choose these particular values? For velocity, this question is brought up in the Conclusions but without much further comment on the value itself. Perhaps a simple scaling analysis of the 3-D temperature equation could quantify the 2 m/yr value (i.e., ensuring that advective tendencies are much less than vertical diffusion). The 2000 m thickness limit is attributed as a personal communication (pg. 2865) which may be intended to be changed to a reference to Fischer et al. (2013). It apparently emerges from the modeling results in that paper, and should be discussed briefly here.
We agree that these numbers seem to be arbitrary, and we now explained why the arbitrary value is not that arbitrary at all. First of all, we corrected the reference to the personal communication of H. Fischer and referred to that CPD manuscript. We also explained the choice of the 2 m/yr limit on the horizontal velocities. Using a simple Lagrangian algorithm of age (based on horizontal and vertical velocity profiles), it can be shown that for a surface velocity of 2 m/yr, the maximum span of the origin of the ice can be several hundreds of kilometers (up to 1000 km), which will definitely complicate the interpretation of the climatic signal, so larger values should definitely be avoided. Smaller values of surface velocities lead to a similar pattern of potential oldest ice sites,

but generally smaller in extent, which hamper a good visualization on a continental-scale basis. This has been incorporated in the main text.

3. One main finding in Figs. 4, 6 and 7 is that many of the potential locations are in the vicinity of several existing deep cores, but a little away from the actual core site to avoid the core's observed warm basal temperatures, to reach locations with shallower bedrock and thus cooler base. It would be interesting to indicate how much shallower (in m of bed elevation) compared to the core site, and to give an idea of the bedrock roughness scales for these regions around the cores. If too rough, that could raise the caveat of rough terrain mentioned at the top of pg. 2867.

The purpose of the paper is to have an idea of potential sites on a continental scale. The fact that the areas where drillings have been carried out are suitable, but not the exact spot, is due to the choice of the drill spot: paleoclimate people tend to select the zones of thickest ice in order to retrieve a longer dataset. Our analysis shows that thicker ice may hamper such interpretation, as it tends to be warmer at the base, and this has been proven in the field. One will always find zones of thinner ice in the vicinity of the domes, but to finally select a drill spot, a much finer-scale analysis needs to be carried out. The roughness is an issue, but we don't think that a 5 km database of BEDMAP2 reveals all the subtleties of the relief to determine this. Also, our method is not capable of defining areas that can be considered as 'rough'. The 'how much shallower' is then a function of the local accumulation rate, horizontal flow field, etc., so it is not feasible to put a number on this. In the end, for a site selection, a multiparametric geophysical investigation will be necessary.

4. It is a little disconcerting that the results of the 2 model approaches agree so well spatially (Figs. 4 vs 6). I would expect the additional observational constraints of ice core temperatures and sub-glacial lake locations, applied in the second approach and not the first, would have more of an effect. Presumably the sigma value (radius of influence, in Eq. 11) is important. Values of 0 to 200 km are tried, but no individual results are shown. Just the mean and RMSE over this range of values, along with other changes, are shown in Fig. 5. This obscures the potential effect of the largest sigma values - if 200 km is correct, then perhaps many of the sites in Fig. 6 would not be suitable, in contrast to Fig. 4. To show this, perhaps add a "worst case" map corresponding to Fig. 6, but with sigma=200, and also using max(G) of the GHF datasets, etc.

In the analysis leading to the paper, we did look at the effect of all parameters separately. The use of the different sigma's in the size of lake influence is an additional way to add uncertainty, which would not be the case if we would only display one. The combination of the 200 km influence area with the maximum G of the datasets (probably Fox Maule) only leaves the Gamburtsev Mountains area as solely plausible, since the lack of lakes, the generally low heat fluxes (whether they are correct or not) and the thin ice account for sufficient low basal temperatures. Exactly the same area is depicted in Figure 4 as likely with low ice thickness and in Figure 6 as likely with high RMSE. So both measures (low thickness, high RMSE) give you the worst case scenario. All other divide areas have thick ice and do have lakes in the vicinity, which is also reflected in lower RMSE in Figure 6.

5. A related point: to compare directly to the RMSE color coding in Fig. 6, perhaps another version of Fig 4 could be added with the color coding representing the RMSE from Fig. 3.

Both model approaches cannot be presented in the same way. The simple model looks at minimum GHFs to reach melting (therefore the farther away from that value, the better

it is to reach cold basal ice), while the second model calculates the basal temperature based on a set of different GHF values (in which the GHF gives the main uncertainty). We did try several ways of representing the potential old ice areas, but the sigma from Fig 3 is completely different from the RMSE in Fig 6, which would only add to confusion if we represented both. Both model approaches are complementary.

6. In section 4.2, the description of the technique for blending velocities glosses over the potential problem that observed interferometric velocities are surface measurements, whereas the balance velocities are depth averages. Is this a problem, and/or is it accounted for at all? I think this is a new aspect that does not come up in Pattyn (2010).
Good point, but it was taken care of. We translated the balance velocities (which are indeed vertical mean values) into surface values using the SIA approach, which means that surface values are 5/4 the vertical mean value. This was equally done in Pattyn 2010. This is also the reason why we mention that the surface velocities were DERIVED from the balance velocities throughout the manuscript.

7. Also in section 4.2, it would help to mention a few other details in the solution method, even if they are as in Pattyn (2010) and described there (and definitely discuss them if they are not as in Pattyn, 2010). These are: (i) use of the SSA equations over large subglacial lakes and ice streams, (ii) setting of sliding velocity to zero if the base is frozen, and (iii) an iterative convergent procedure allowing for temperature influences in (i) and (ii). If these aspects of the procedure are the same as in Pattyn (2010), then one short sentence for each would suffice. But if any of (i)-(iii) are not done here, then justify why not.
We used the same approach as in Pattyn 2010 with respect to the remarks here. We therefore added this information to be complete: "Similar to \citet{pattyn10}, a shelfy-stream model is used to correct for the ice flow over large subglacial lakes and basal sliding is only allowed when the base is temperate or within a range of 1K of subfreezing temperatures."

8. In showing just the mean and RMSE of the 3 GHF datasets, Fig. 2 is somewhat misleading. It does not show the profound differences between the regional patterns of GHF in the Shapiro and Fox-Maule datasets. Perhaps add panels in Fig. 2 showing each one separately. (Although, in the subsequent application of the model, I agree that it is adequate to use the mean +/- RMSE as in Eq. 6).
Figure 2 is meant to give an overview of the spatial variability without going into the details of the different datasets. It shows that over several areas of the Antarctic ice sheet the variability of the GHF between the different datasets is large. For the actual datasets, the reader is referred to the publications by Fox-Maule and Shapiro and Ritzwoller.

9. In the caption for Fig. 4., the limits of Delta_G and sigma_G seem erroneous. Shouldn't it be "for Delta_G > 5 mW m-2 and sigma_G < 25 mW m-2" ?
Thank you for spotting this error. This is indeed an error in the Figure caption.

10. The concluding section contains a good discussion about non-steady states and glacial-interglacial time scales of forcing (pg. 2874, lines 8-25). Perhaps some useful quantitative limits on these effects could be inferred from a recent paper on these issues for Greenland (Rogozhina et al., 2011, JGR, 116, F01011).
We added a sentence in the discussion: "For instance, \citet{rogozhina11} demonstrate that for the Greenland ice sheet, basal temperature differences between an ice sheet

initialized by a steady simulation (as in this study) and those generated by a paleoclimatic simulation can be up to 4.5$\circ$C."

<u>Technical points:</u>
I have many small suggested changes, nearly all regarding language details. However, it should be noted that the text is generally very clear and well written as is.

pg. 2860, ln. 11: Change to "constrained"
done

pg. 2860, ln. 14: Add comma: "ice sheet, and"
done

pg. 2860, ln. 22: Change to " such a quest"
done

pg. 2860, ln. 24-25: The phrase "which questions the strong Antarctic temperature carbon cycle coupling on long time scales" is unclear to me. Perhaps one more sentence of explanation can be added.
We rephrased this sentence as "The reason for such a quest is that the oldest part of the EPICA Dome C ice core has revealed low values of CO\textsubscript{2} from 650,000 to 800,000 years ago \citep{luthi08}, and therefore out of phase with atmospheric temperature change. This questions whether such partial decoupling between the CO\textsubscript{2} record and climate had precursors over longer time-scales \citep{jouzel10}."

pg. 2861, ln, 26: Add comma: "melting point, and"
done

pg. 2862, ln. 6-7: Change to " search for suitable drilling locations"
done

pg. 2864, ln. 6: Change to "(with negligible horizontal advection)"
We rewrote the sentence : "where horizontal advection is absent or negligible"

pg. 2864, ln. 12: Change "hence" to "i.e.,"
done

pg. 2864, ln. 20: Change to "Since this is a vertical-column model with no horizontal advection, it is only valid..."
We rewrote as suggested

pg. 2864, ln. 21: Change to "carried out for regions with horizontal velocities..."
done

pg. 2865, Eq. (5): I think the first "H" on the right-hand-side should be removed, or change the vertical variable from z' to the Greek symbol zeta' as used in Eqs. (2) and (3).

Thanks for spotting this. Has been corrected into zeta. We also changed the next sentence accordingly.

pg. 2865, ln. 12: Change to "Gmin are compared below to other GHF databases".
done

pg. 2865, ln. 17: Change to "Their values of..."
done

pg. 2865, ln. 18-20: Perhaps change to "but the spatial patterns are markedly different, and the G2 values are considerably higher in many regions."
We rewrote the paragraph: "Their values of GHF are in the same range as Shapiro (2004), but the spatial patterns are markedly different, and the G2 values are considerably higher in many regions. The third dataset G3 represents a recent update of G2 derived by Purucker (2013). This uses low-resolution magnetic observations acquired by the CHAMP satellite between 2000 and 2010, and produced from the MF-6 model following the same technique as described in Fox Maule et al. (2005)."

pg. 2866, ln. 11: Change to "prevent the bottom ice from reaching..."
done

pg. 2866, ln. 18: Change to "the largest excess of minimum GHF above actual GHF"
done

pg. 2866, ln. 24: Change to "thickest ice clearly corresponds to zones"
changed as suggested by the two other referees: "These restrictions (combined with the ice-flow speed limit and minimum ice thickness) mean that only a very few areas in the central part of the Antarctic Ice Sheet can be considered likely to host cold-bed conditions."

pg. 2866, ln. 26: Change to "These restrictions (combined with the ..."
see previous comment

pg. 2867, ln. 3: "accidented" is in the dictionary, but is an unusual word. Perhaps change to "by highly uneven" or "by very rough" (?)
we change the word in "uneven"

pg. 2867, ln 8: Perhaps change to "more likely" (?)
done

pg. 2867, ln. 11-12: Change to "- plays a significant role in determining"
done

pg. 2871, ln. 8; Change to "brought their number to"
Changed in "An initial inventory contained 145"

pg. 2872, ln. 6: Change to "where the correction"
done

pg. 2872, ln. 11: Perhaps change to "In the ensemble experiments"

ok

pg. 2872, ln. 19: Change to "sufficiently far away from" or "sufficiently removed from"
changed by "sufficiently far away from"

Pg. 2872, ln. 24: Change to "cold-based sites do not coincide exactly with the ice-core locations, but are in their vicinity"
We rewrote the sentence by: "suitable cold-based sites do not coincide exactly with the ice-core locations, but lie nearby in locations where ice is thin enough to reduce basal ice temperatures."

pg. 2872, ln. 26: Change to "Similarly to"
done

pg. 2872, ln. 28: Change to "larger range of basal temperatures due to either"
done

pg. 2873, ln. 14: Remove "equally"
done

pg. 2873, ln. 26: Change to "from poor constraints" (or "from sparse constraints")
done : "from sparse constraints"

pg. 2874, ln. 8: Change to "conditions is the glacial-interglacial"
done

pg. 2874, ln. 16: Change to " H by 100 m, which is appropriate for the"
done

pg. 2874, ln. 17: Change to "surprisingly similar to the previously calculated"
done

pg. 2874, ln. 26: Perhaps "1. Although areas characterized by ..." (?)
We add "but" after the comma in the sentence

pg. 2875, ln. 1: Change to "may be too high"
done

pg. 2875, ln. 2-3: Change to "to rely heavily on ice-flow models for corrections due to upstream advection."
done

pg. 2875, ln. 7: Change to "in reality be much higher than represented..."
done

pg. 2875, ln. 9: Change to "bedrock elevation data"
done

pg. 2875, ln. 11: Change to "areas thus overlooked."
done

Fig. 2 caption, 2nd ln.: Change to "of the GHF datasets." 3rd ln.: "are the major drill sites"
done

Fig. 5 caption, 2nd ln.: Change to "The color scale is truncated at -10 C." 3rd ln.: Change to "for the same ensemble."
done