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Climate of the Past Discussions

Interactive comment on "Using ice-flow models to evaluate potential sites of million year-old ice in Antarctica" *by* B. Van Liefferinge and F. Pattyn

Anonymous Referee #1

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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

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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 cleary 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.

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.

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.

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

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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.

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.

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).

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.

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

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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).

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" ?

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).

Technical points:

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"

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

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

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.

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

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

pg. 2864, ln. 6: Change to "(with negligible horizontal advection)"

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

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

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

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).

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

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

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."

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

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

pg. 2866, ln. 24: Change to "thickest ice clearly corresponds to zones"

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

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

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

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

pg. 2867, ln. 14: Change to "- plays a significant role in determining"

pg. 2871, ln. 8; Change to "brought their number to"

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

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

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

pg. 2872, ln. 24: Change to "cold-based sites do not coincide exactly with the ice-core

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locations, but are in their vicinity"

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

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

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

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

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

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

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

pg. 2874, ln. 26: Perhaps "1. Although areas characterized by ..." (?)

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

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

pg. 2877, ln. 7: Change to "in reality be much higher than represented..."

pg. 2877, ln. 9: Change to "bedrock elevation data"

pg. 2877, ln. 11: Change to "areas thus overlooked."

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

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

Interactive comment on Clim. Past Discuss., 9, 2859, 2013.