Resubmission Review: "High resolution boundary conditions of an old ice target near Dome C, Antarctica"
Duncan A. Young, et al., *The Cryosphere*

Major Comments:

My major concerns with the original manuscript were that the paper needed a discussion section plus more scientific interpretation, and that the authors should own their preference for Candidate A. The authors have addressed both of these concerns thoroughly in this revised manuscript. The results section has been expanded and an extensive discussion has been added. The authors not only own their preference for Candidate A, but they also identify a specific location within Candidate A for additional ground-based surveys in preparation for drill site selection. The new manuscript is a solid piece of scientific work and is suitable for publication in *The Cryosphere* after various minor revisions.

Minor Comments:

P1, L2-4: "New ice thickness data derived from an airborne coherent radar sounder was combined with unpublished data that was in part unavailable for earlier compilations, and were able to remove older data with high positional uncertainties."

Put this statement in active voice: "*We* combined new ice thickness data... and *we* were able to remove..."

P1 L18: "...with and approximately 400 ka transition..."

Should be "an" not "and".

P2 L3-4: "(1) low accumulation, to restrict vertical thinning rates and increase temporal resolution..."

The effect of surface accumulation is a bit more complex than that. While it is true that low surface accumulation produces low thinning rates (in steady state), it is also true that low accumulation rates produce very thin annual layers. It is the second effect that generally wins out: low-accumulation ice cores (like EPICA Dome C) generally have course temporal resolution but broad temporal coverage spanning multiple glacial cycles, while high-accumulation ice cores (like WAIS Divide or the Greenland cores) have very fine temporal resolution but poor temporal coverage spanning only the last glacial cycle. It may be more accurate to say that low accumulation increases temporal coverage rather than temporal resolution.

P2 L4-5: "(3) proximity to an ice divide to limit vertical thinning rates..."

Is this a reference to the Raymond effect? The average vertical strain rate of the ice column is equal to a/H, the accumulation rate divided by the ice thickness (assuming steady state and neglecting basal melt). This value is independent of distance from the divide or of any other effects associated with the horizontal flow setting. However, the vertical distribution of strain rate within the ice column can change depending on the ice dynamic setting, even if the average value is constant. The Raymond effect can produce an upwarping of layers underneath an ice divide, effectively trading a rapid initial thinning near the surface for much less thinning near the bed, thereby preserving older layers.

If you are referring to the Raymond effect here, it would be appropriate to reference Raymond [1983]. However, the layers in the echograms shown in this paper do not appear to have a visible Raymond arch, so it is possible that either flow over rugged topography or localized wet-based conditions have destroyed the conditions which give rise to the Raymond effect. This clause could also be removed and point (3) would still stand on the basis of wanting to minimize disturbances due to lateral flow and of wanting to simplify the altitude history of the surface.

P2 L7-8: "Items 1 and 2 interact, as low accumulation limits the advection of cold, requiring low geothermal heat flow to offset melting."

Rephrase to more clearly describe the underlying physical processes: "Items 1 and 2 interact, as low accumulation limits the *downward* advection of cold *surface temperatures*, requiring low geothermal heat flow to *prevent* melting." (changes in italics)

P2 L8-9: "Items 3, 4, and 5 lead to the somewhat contradictory requirement of a flat subglacial mountain."

Only items 4 and 5 are involved in the contradiction.

P4 L1-2: "...implying that an ideal old ice target may require a very flat ice-bed interface..."

What about roughness along the flow path back towards the dome? The old ice near the bed traversed a trajectory from the dome or divide on the way to its present position, and bed roughness along this trajectory could have induced complex deformation fields that distorted the layers even if the drill site itself has a smooth bed. The early part of the trajectory can probably be discounted because the ice was high in the column and therefore mostly unaffected by basal roughness early in its history. However, the later part of the trajectory may have been subject to complex deformation near the bed. This argues for a consideration not only of the local roughness at a potential ice core site, but also of the roughness along a short trajectory pointing back towards the divide. The ideal location would then be at the downstream terminus of a smooth-bed 'stripe' oriented in the flow direction.

P5 L 14-16: "The size of Candidate A compared to the other local candidates also makes it more likely that the Van Liefferinge and Pattyn (2013) model captured basal temperatures correctly."

It is the size of Candidate A relative to the grid size that makes it more likely that the model captured temperatures correctly, not the size relative to the other candidates. The fact that the other candidates are small relative to the grid size makes it less likely that the model did a good job for them, and therefore strengthens the argument for A; however, the robustness of Candidate A should be independent of the robustness of the other candidates. A possible rephrasing that navigates this distinction is: "The size of Candidate A compared to the 5 km model grid size makes it more likely that the Van Liefferinge and Pattyn (2013) model captured basal temperatures correctly, while the small size of the other candidates relative to the model grid makes them less reliable."

P5 L31-32: "...bedrock trends are significantly disagree..."

Remove "are".

P6 L3-4: "...a 15 km offset along-track would be required to reconcile the surface slope structure and Bedmap2 bed elevation data at this location."

What about an across-track offset? Is one offset intrinsically more likely than the other for older navigation systems?

P6 L15-16: "The identification of subglacial lakes is complicated by variations in englacial attenuation that modifies the strong radar reflection due to an ice-water interface (Carter et al., 2007)."

A better reference here would be Matsuoka, 2011.

P 8 L8: "...and a second line to constrain better an oblique topographic ridge..."

"constrain better" should be "better constrain".

P 9 L14-15: "To obtain ice thicknesses, we systematically select a window around the earliest bed return, and then automatically select the best fitting pulse waveform within that window (assumed to be a paraboloid power profile), for both the surface and the bed."

By a "paraboloid power profile", am I right in interpreting this to mean that you assume that the echo power has a Gaussian profile on a linear scale, which becomes a parabola on a logarithmic (dB) scale?

P 10 L 13-14: "Regions with a sustained specularity content greater than 0.2 were classified as subglacial lakes."

What do you mean by "sustained"?

P10 L 19: "...all subglacial lakes that were identified had low hydrostatic gradients (Fig 4)."

This is a very powerful argument supporting the presence of subglacial water, but Figure 4 doesn't really allow us to evaluate the hydraulic gradient of most of the lakes (other than the largest ones, which do indeed appear flat by eye). Some quantification of what is meant by "low hydrostatic gradients", and some quantification of whether or not "all" of the lakes do truly meet that criteria, would be helpful here. This doesn't necessarily need any addition to the figure, a simple statement like "X% of the lakes had a hydraulic gradient less than Y" would suffice.

P 14 L 2-4: "Small scale roughness, at length scales of the line spacing and below, is relevant for three reasons: 1) roughness gives insight into the pathways that basal ice must traverse; 2) roughness may provide information on past ice sheet behavior and basal conditions and 3) roughness is a key control on the uncertainties inherent in profiling radar systems."

I would add a fourth factor: 4) basal roughness forces the overriding ice sheet to develop a complex deformation field in the lower part of the ice column, and this deformation field could disturb stratigraphic continuity of the ice core record.

P 16 L12 "...will not be available for melting on the intervalley regions."

"Peaks". The word for "intervalley regions" is "peaks".

P16 L17: "...observed driven stresses..."

Should be "driving stresses".

P17 L17-18: "However, a trade-off is that maintaining a simple flow path for basal ice in such a rough environment will be difficult, and the mountainous region also induces relatively large driving stresses in the overlying ice."

I would add that it's not simply a matter of constraining the flow path (which can also be complicated by unknown changes in ice sheet configuration in the deep past), but of constraining the deformation that a particle of ice accumulates along that flow path. When the basal topography is complex, the flow field in the lower 20-30% of the ice column should be complex as well. As a particle of ice traverses this flow field it accumulates deformation, potentially distorting stratigraphic continuity. I would recommend adding a sentence here about the importance of accumulated deformation along the flow path of the basal ice.

P18 L27: "The result is that the first return will tend toward the minimum ice thickness within the beam pattern, however the measured thickness at this site will be slightly overestimated."

This statement confused me. If the first return is the minimum ice thickness within the beam pattern, then shouldn't the measured ice thickness be an *under*estimate? At first I thought this was a simple typo (overestimate vs underestimate), but on re-reading it I realize there is an alternate interpretation that also makes sense: the measured ice thickness is an overestimate of the ice thickness *at the cross-track location of the off-nadir return*. The measured thickness is an overestimate of the true ice thickness *at this off-nadir location* because the radio signal took a diagonal path through the ice to get there rather than a vertical path.

However, the measured thickness will still be an underestimate of the true nadir thickness. In the context of error analysis of profile data, the first return will be biased towards systematically underestimating ice thickness. It is this second sense of measurement bias- the bias relative to the true nadir thickness, rather than bias relative to the thickness at an unknown off-nadir position- that people will think of when they read about underestimated or overestimated ice thickness. I would recommend rephrasing this sentence as, "The result is that the first return will tend toward the minimum ice thickness within the beam pattern, and the measured thickness at this site will be systematically underestimated relative to the true nadir thickness."

Supplementary Material:

I was not able to locate a table describing the subglacial lakes in the supplementary material. A table listing the centroid lat/lon, mean ice thickness, and along-track length (plus any other variables the authors think are relevant) for each lake should be provided. This table could also be placed in the main text or appendix instead of the supplementary material, as there are only 40 new lakes.

Figures:

Fig 2:

Move the label for Candidate A from the right side of the candidate to the left side. In the current configuration it looks like the label refers to the yellow box.

The statement "Regions of disagreement between Bedmap2 and other dataset is shown by the yellow boxes in all panels" is confusing. After reading the text, it is clear that what you are referring to is a misalignment between trends in the surface slope field and trends in the bedrock topography. Clarify this in the caption.

Fig 3:

The caption states that the color scale is relative power after geometric correction. I interpret this to mean that the effects of geometric spreading on echo strength have been removed. However, the shallow layers are still much brighter than the deep layers. Is this because of attenuation, or is my interpretation of this sentence incorrect? If the whole echogram has been geometrically corrected, I would also expect the noise floor near the bed the bed to feature a color ramp, with brighter speckle below and dimmer speckle above, rather than a uniform black.

Fig 5:

There is a problem with the bed elevation and RMS deviation colorbars. Both of them have rendered with a color gradient across the colorbar in addition to the intended color gradient along the colorbar.

The label for the RMS deviation colorbar would be more instructive if it simply said, "Bed Roughness (m)". The caption can clarify that roughness is defined as the RMS deviation of the bed within an 800 m rolling window.

Fig 6:

This colorbar has the same issue as those in Figure 5.

Fig 8:

The x-axis should have the more straightforward and descriptive label, “Roughness”, with the definition of roughness (RMS deviation within 800 m window) in parentheses. As written it reads as if the units are multiples of 800 m.

“The focused data has large outliers in rough terrain, as one direction is actually more correct; for the unfocused data, the crossover is smaller, as both directions are equally wrong.” I absolutely love this explanation.