

Review Comments

Title:	Stable accumulation patterns around Dome C, East Antarctica, over the last glacial cycle
Journal:	The Cryosphere
Date:	06/30/2017

General Comments:

In this paper, the authors use the spatial pattern of model derived paleo-accumulation rates [generated as part of a companion study (Parrenin et al. 2017)] to constrain the processes controlling surface mass balance around Dome C, East Antarctica, and infer the long-term stability of the ice divide presently located there. A 1D model inversion computes three free parameters – the average accumulation rate, the average geothermal flux, and the average velocity shape function – given temporal variability in accumulation set by the time-series derived from the EPICA Dome-C core. Using the geothermal flux and velocity shape function from that inversion, the authors then recompute the time-history for the accumulation rate to perfectly match the observed isochrons. From this accumulation rate product, the authors make the claim that the “surface topography of the Dome C regions has not changed significantly over the last glacial cycle [128 ka].”

There is a history of researchers using modeled isochron position to determine paleoaccumulation rates near several of the deep ice cores in Antarctica and Greenland, most notably at WAIS Divide (e.g., Koutnik et al. 2016; Neumann et al. 2008) and NGRIP (e.g., Fahnestock et al. 2001). Both this study and the companion paper are missing all reference to this body of work, which, if read, would have highlighted challenges associated with deriving a unique solution for accumulation rates that go undiscussed in the current manuscript. Most notably, I think the authors of the previous works would take issue with the following model choice, stated in the companion paper:

“We use a 1D pseudo-steady (Parrenin et al., 2006) ice flow model, which assumes that the geometry, the shape of the vertical velocity profile, the ratio $\mu=m/a$ and the relative density profile are constant in time.” (Line 104-105, (Parrenin et al. 2017))

As seen from the work at WAIS divide (Koutnik et al. 2016), the accumulation history trades-off non-uniquely with the local horizontal stretching rate and burial history (which is dependent on distance from divide), so by holding their “p” shape value constant in time and ignoring horizontal advection, the authors of this work **prescribe** a result with no changes in flow regime (ie, unchanging distance to the divide) at every position. This makes it impossible to constrain the actual stability with their results as presented, as their conclusion that the divide is stable is baked into their model assumptions.

In the absence of the ability to prove that the Dome C divide has been unchanging for 128 ka, I find it difficult to see the novel contribution of this paper.

Specific Comments:

The primary limitation of this paper is in the model formulation (which itself is under-review, making this manuscript harder to evaluate in isolation). Previous literature has documented a more thorough approach

for this research question. The analysis of isochrons connected to the WAIS divide ice core included an exhaustive exploration of model space, varying both divide position and flow dynamics. Even over a 9.2 ka reconstruction, non-uniqueness in the WAIS divide solution made it difficult to make claims about accumulation and divide position with high certainty. Constraining results over 128 ka will be proportionally more difficult. To reach the level of rigor demonstrated in previous studies, this work needs to do the following:

1) Define “stable” –

This manuscript describes the accumulation pattern and divide position as “relatively stable” several times, without any context for what that term means. Migrates or evolves at rates less than X? Has not moved beyond a range greater than X over time period Y? There is a general lack of precision in both the discussion and conclusions that must be refined.

2) Clearly establish criteria for evaluating stability –

In the current draft, the authors have not done enough to identify **how** they have inferred divide stability. My reading of the draft shows them making two claims: (1) spatial patterns in small-scale accumulation variability have been persistent through time, and (2) large scale accumulation gradients consistently show lower accumulation to the South. Without getting into the accuracy of the model produced accumulation rates, it is not clear that either of these conclusions justify an inference of divide stability. The authors ascribe small scale surface variability (which controls local mass balance) to processes at the bed, which means their temporal stability is not at all controlled by divide position. The large-scale accumulation pattern seen today depends mostly on relative distance to the coast. Without a clear marker for divide position in the current accumulation field, along with a demonstration that such a transition has not migrated with time, information in the large-scale gradient also seems insufficient to prove divide position has not changed.

3) Evaluate divide stability in the context of other evidence –

A perfectly stable divide over the last 128 ka would develop a prominent, measurable Raymond arch. Given no Raymond arch presented, my suspicion is that there is not one under the current divide, which should provide evidence for at least some instability in the system. Given this observation, the authors could provide a bound for **the most** stable the divide could be.

4) Explore the possibility of a temporally variable divide position with your model –

This represents one of the biggest hurdles for the authors going forward. As written, this inverse model formulation is not suitably justified. This is in part because the model development is established in the companion paper, but the objectives of that paper and this one are quite different. In (Parrenin et al. 2017), the goal is to constrain the age of the deep ice. For that work, trade-offs in the strain thinning and accumulation rates do not matter, as their **combined** effect dictates the age of the ice. For the inferences you try and make in this paper, you need a unique solution that disentangles ice-flow effects from accumulation, setting a higher burden for the derived model. To prove that the divide has not migrated with time, and justify the claims made currently, you need to show in this work that the radar data are **incompatible** with a solution in which the divide migrates. Is it possible to reproduce the isochron field with a temporally variable p' value?

The novel contribution of the second component of this work, discussing the role of surface slope and surface curvature on local surface mass balance variability, is not clearly articulated. The role of surface topography on snow trapping has been established for decades (Whillans 1975). It seems as though the basal influence on surface topography is the purview of a different study, leaving little for this study to discuss. A more quantitative treatment, or re-evaluation of our process understanding, is required to justify this section.

Finally, there is room for improvement in the clarity of the writing. While many paragraphs are well written, there are also many points throughout the paper where the logic or structure was unclear, making it hard to follow the flow of ideas to the authors conclusions. Individual comments on the writing are provided in the technical corrections.

Technical Corrections:

<u>Line #</u>	<u>Comment</u>
12	“site of the oldest as-yet-retrieved <i>continuous</i> ice core”
13-22	The second half of this paragraph is too informal and largely unnecessary. For example, you do not need to clarify for the reader that the Dome has both an upward sloping flank and a downward sloping flank (that is the definition of a Dome). The point you are trying to make is that “total annual precipitation at Dome C is controlled by the surface gradient and dominated by large precipitation events”, but wading through the superfluous information makes it hard to get to that point. This is a common complaint I have with this manuscript. e.g.: “Modern precipitation at the ice core site is low (~XXmm/a), with infrequent storm events representing more than 50% of the total accumulation signal. Coastal air masses lose moisture as they are driven inland to higher elevation, resulting in a characteristic accumulation gradient with higher measured precipitation on the south side of Dome C.”
23-37	This paragraph starts with a discussion of dust provenance and ends with a sentence on the role of surface gradients in snow re-deposition. What is the point of this paragraph? Here and elsewhere in the paper, the writing seems unfocused. Decide what the point of that paragraph is supposed to be, and focus on that one idea.
41-43	Medley et al. 2013 are reconstructing paleo-accumulation in only the highest part of the ice column, which is isolated from the strain-history and advection effects that make this particular study challenging. This section should be where you cite and discuss the large body of literature that focuses on deep-ice paleo-accumulation inversion, which is currently missing from the paper entirely.
45	“... show a continuous existence through historical timescales...” This has an ambiguous antecedent (what is shown to exist? even though I know you mean existence of an accumulation gradient, you need to actually refer to the accumulation gradient here), and the word “historical” in “through historical timescales” is imprecisely defined. What is the timeframe of its stability?

- 58-63 Here again, the logic of this point is not clear. The last sentence, “several recent studies have shown the influence of increasing precipitation...”, does not at all speak to the original statement, that knowing the position of a dome is crucial. In this list, it would make more sense to simply say:
- “The position of topographic domes affects the spatial distribution of accumulation, which ultimately affects the geometry of the ice sheet (with its resulting sea-level implications) through time.”
- 67 Citation needed here to justify the need to know flowlines of ice particles through time to interpret ice cores.
- 69-70 Remove this sentence, it doesn’t speak to why knowing the position of the dome is crucial.
- 71-75 This point speaks directly to my biggest question about this work, but seems to be ignored through the rest of the paper. It will be hard for you to prove the dome position for the last 128 ka with this manuscript’s approach, because there is no unique solution for divide position through time and accumulation rate through time. This problem is underconstrained.
- 85/87 Calling the topography and saddle “gentle” doesn’t provide information to the reader. Either define gentle or remove.
- 102 “... center frequency of 60 MHz; internal isochrones are therefore coherent...” Coherent in what way? Are you saying, because it is 60 MHz, the wavelength is long enough that any changes in range between seasons are unresolvable by phase measurements? Coherent has a very specific, technical definition. Also, I would use just “isochrones”, instead of “internal isochrones”, as there is no such thing as an external isochrone.
- 117/149 “Pseudo-steady-state means that all parameters in the model are considered steady except for $R(t)$...” this statement about your model seems to violate the requirement set in line 71, in which you say “The location of the dome [through time] is required to model isochrones interpreted from radar surveys”. With your model, you have functionally assumed a constant divide position. This arises again at line 149, when you assume τ is modeled perfectly. If the divide migrates, τ is not modeled correctly, and you are mapping variability in divide position into accumulation history. This seems like a fundamental flaw in your method.
- 172-178 “our paleoaccumulations are valid at the ice divide and the dome where horizontal ice flow speeds are negligible.” I’m not sure the literature agrees that your assumptions hold over your domain. (Neumann et al. 2008) show that the vertical thinning through the column varies dramatically as you move further than one ice thickness away from the divide. As the divide moves around with time, the vertical thinning function changes, and the horizontal advection term becomes more important. Your domain extends nearly 100km from the modern divide, it is likely true that a 3D model is required to do this correctly. You need to provide more discussion of the region over which your assumptions are valid (or conversely, at what point these assumptions aren’t valid). This is required for line 175 also, at what depth would your assumption that τ is fitted correctly break down? Without clearer justification, I am not convinced your assumptions regarding τ (and p in the initial model formulation) hold.

- 180 “... the assumption of constant ice thickness is fair...” – define fair. How good is it? What is the magnitude of error this might introduce?
- 201 What is “this data set”?
- 232 “marked decreasing gradient” – is the accumulation rate gradient decreasing? Or the accumulation rate decreasing?
- 234-235 “... there is no clearly visible accumulation gradient over time...” I’m not sure what this means. Do you mean the gradient in time or the gradient in space? As in, the accumulation rates don’t change systematically with time, or the accumulation (spatial) gradients don’t change systematically with time?
- 244 “... the area of high accumulation is preceded by an area of low accumulation ...” preceded describes a temporal, or otherwise linearly ordered relationship - you are describing a 2D spatial relationship. “Adjacent to” would be better here.
- 247 “Paleoaccumulation rates per isochrone-bounded layer show a similar pattern in accumulations...” I’m not quite sure what pattern you are referring to. Are you saying the spatial pattern of paleoaccumulation is similar between time periods? Text needs to be clarified.
- 277-278 “As a reminder, these accumulation maps therefore display values of detrended paleoaccumulation once the large-scale precipitation gradients from ECMWF ERA-40 has been removed.” This sentence is unnecessary. If you want to redefine what you mean by “detrended” in the previous sentence that is fine, but you don’t need two sentences that both say “we have plotted the detrended data”.
- 305-307 You state here that the meteorologic system and location of Dome C is stable, but that doesn’t necessarily follow from the preceding statement. This is where having a clear set of criteria established, describing for the reader what aspect of your results *prove* the divide position is stable, is necessary. Also, as stated above, qualify what it means to be “stable”.
- 308 “... must have interacted with the **same** surface topography ...” That claim is too grand. Draw any transect from the coast to the interior and you will find decreasing accumulation rates. South will always be further from the coast than North, so observing this gradient does little to prove the specifics of Dome C’s position.
- 334 “This could be due to ice flow increasing with distance from the dome.” It isn’t clear what you mean by this.
- 355 “... radar data used **do** not show ...” - data is a plural noun
- 366-368 “If the surface slope and curvature do not change, we can suppose this implies the position of the divide and the dome must have remained relatively stable.” Why is this true? Small scale variability in the surface (controlling curvature on a 3km scale) seems decoupled from the divide position. As you’ve pointed out earlier, and in the following paragraphs, the small scale surface variability maps well into subglacial topography (and possibly enhanced by subglacial melt). These effects are separate from those controlling divide position, which is an controlled by continental ice dynamics. I don’t think you’ve proven that the stability of small scale features == the stability of large scale features.

- 413-414 “Both suggest that the current surface topography of the Dome C region had not changed significantly over the last glacial cycle.” This is a significant claim, and there is an equally significant burden of proof to make it. I don’t believe this model, or the analysis presented here, is capable of proving this statement. Again, unless you can show that the data is **incompatible** with scenarios that include divide migration, you should not publish that the divide has been stationary for 128 ka.
- Figure 1 Eliminate “radar-lines” title. It may also be less ambiguous to refer to “gray contours” instead of “gray lines” when discussing the Bamber et al surface elevations (there are lots of gray lines).
- Figure 4 “N-S accumulation **rates** decreasing with distance from the Indian Ocean coast...”
- Figure 5 Define “very good agreement”. “Black lines [not white lines] outline the same areas of small-scale high accumulation...”
- Figure 6 It took me a long time to make heads or tails of this figure. At the very least, the radar lines should be thicker, as it is hard to see the accumulation rates relative to the curvature. It would also help to emphasize specific parts of this figure, so the reader knows how to focus their attention.

Review References:

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