

Interactive comment on “Spatial and temporal distributions of surface mass balance between Concordia and Vostok stations, Antarctica from combined radar and ice core data: First results and detailed error analysis” by Emmanuel Le Meur et al.

Anonymous Referee #2

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Review of Le Meur et al. 2017: Spatial and temporal distributions of surface mass balance between Concordia and Vostok stations, Antarctica from combined radar and ice core data: First results and detailed error analysis

Overview: Le Meur et al. present GPR profiles and limited ice-core data to determine the spatial pattern of accumulation near Dome C, East Antarctica. The work is motivated by the effort to find a location to drill an ice core to recover a >1 million year

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continuous climate record. An accurate surface mass balance is prerequisite for site selection. Thus, the primary conclusion of lower accumulation rates towards Vostok is important, if not particularly surprising.

My impression of this manuscript could not be any more opposite than the first referee. I found this manuscript tedious, poorly written, and ill-considered. While the main result is of interest, this manuscript suffers from a number of flaws; in particular, a lack of brevity. I found myself asking the same question over and over: why does this take ~13,000 words?

I want to highlight two specific areas where I fundamentally disagree with the other reviewer. First, to address the other reviewer's comment: "In particular I like the careful consideration of the density and error analysis". I found the density and error analysis to be lengthy, but not informative. For instance, error bars are shown on the accumulation rate inference using the DC5 density profile in Figure 8. Yet they don't overlap the accumulation inference using the S0 density profile. When I read the caption of Figure 8, it does not even mention the error bars. When I then track down the reference in the text to Fig. 8, I am referred to section 5.4. Once there, I cannot even find what uncertainty the authors are referring to. There is this section of text:

"As a consequence, an uncertainty of $\pm 20 \text{ kg.m}^{-3}$ over the first 7 m linearly decreasing to $\pm 15 \text{ kg.m}^{-3}$ at 15 m deep and remaining constant further down is proposed for these density measurements (see error bars on the figure)"

But this doesn't directly translate to the error bars in Figure 8. The authors go on to write this about Figure 8:

"Keeping in mind that the resulting errors (black bars on the figure) are probably underestimating the overall uncertainty arising from the chosen density profile, and given the fact that similar errors have to be expected with the S0 density profile (not represented for the sake of clarity), one comes to the conclusion that using the DC or the S0 density does not bring any significant changes in the computed accumulation rates"

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So let me get this straight. First you say the errors are an underestimate. Second you say the S0 density profile is not a lower bound because it is also uncertain. Then third you jump to the conclusion that the density profile is not a significant source of uncertainty. This flabbergasts me for two reasons: 1) The uncertainty on the inferred accumulation rate (based only on density and not considering other factors) is then somewhere between 1 mm/yr (roughly the difference between S0 and DC5) and 2 mm/yr (roughly the difference between the upper error of DC5 and lower error of S0) which is a third of the total variation in your 600km line survey. This is not insignificant. 2) But if the impact of the density profiles truly was “insignificant” then why did you spend so much space writing about?

Therefore, I don't find the error analysis “careful”; I find that the authors are obscuring clarity with unnecessary detail.

Second, I want to address the dating of the S cores. It becomes clear when the authors admit they can't distinguish Tambora from Cosiguina that the dating for the S cores is not reliable. Sulfate leaves no chemical composition information to tie events in cores together unlike tephra. Therefore, you have to match events based on patterns or amount of sulphate deposition. Tambora and 1809 are the closest thing in the last 500 years to a specific pattern match. I've attached a figure of sulfur from WAIS Divide (because it is the best resolved ice core in Antarctica, Sigl et al., 2013) to illustrate just what the authors mis-identified. I realize that the amount of deposition may vary by location in Antarctica, but if you cannot reliably distinguish the couplet of Tambora and 1809 regardless of magnitude, what are you actually matching? The authors note that Tambora was missing in two of the five Volsol cores, but there is nothing to suggest that Tambora itself is prone to being missed, only that small-scale irregularities between replicate cores can obscure volcanic events. Thus, Cosiguina is as likely to be missing as Tambora. It is particularly odd that this manuscript finds more events, and ties these events to specific volcanoes, than do the 5 replicate Volsol cores (see Gautier et al., 2016 Table 1).

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But this brings up another point, the authors are not presenting any of the sulphate data anyway. I'm expected to accept your matches on blind faith. All of this would not be nearly as annoying if it weren't for the fact the manuscript could be written entirely without any dating of the S cores. The DC (Volsol) cores provide a robust age scale. The IRHs are accepted as isochrones without the cross-over dates (which in theory are useful, but would require reliable dating which you don't have). So why bother with this at all?

Overall, I find this paper to be unreadable. Not because it is outright wrong, but because there is so much extraneous information that the important points are lost in a sea of detail. Frustratingly in a paper of this length, there is little in the way of discussion of the implications of these results. How about a few paragraphs on what this suggests for where to drill a deep ice core? How about comparing the 5% increase in accumulation to inferred air temperatures? Is it consistent with Clausius-Clapeyron? And what is the impact on global sea level? Over what spatial area would the increase have to be to have a noticeable effect on sea level? And a figure would really help in the comparison to accumulation inferred from other ice core sites.

I have not provided specific comments to the text for a simple reason – I believe the vast majority of the text should be deleted. The figures are in better shape, but the paper really only needs three: Figure 1 Figure 2 combined with Figure 7 and full uncertainties (not those shown in Figure 8) Figure 9 combined with Figure 12

I challenge the authors to write the manuscript in 5000 words or less. There are good thoughts within the manuscript but they are obscured by unnecessary detail and lead to incomplete thoughts on some of the important aspects. The main result of a spatial map of accumulation and an increasing trend through time is useful and should be published. The manuscript in its current form should not.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-104>, 2017.

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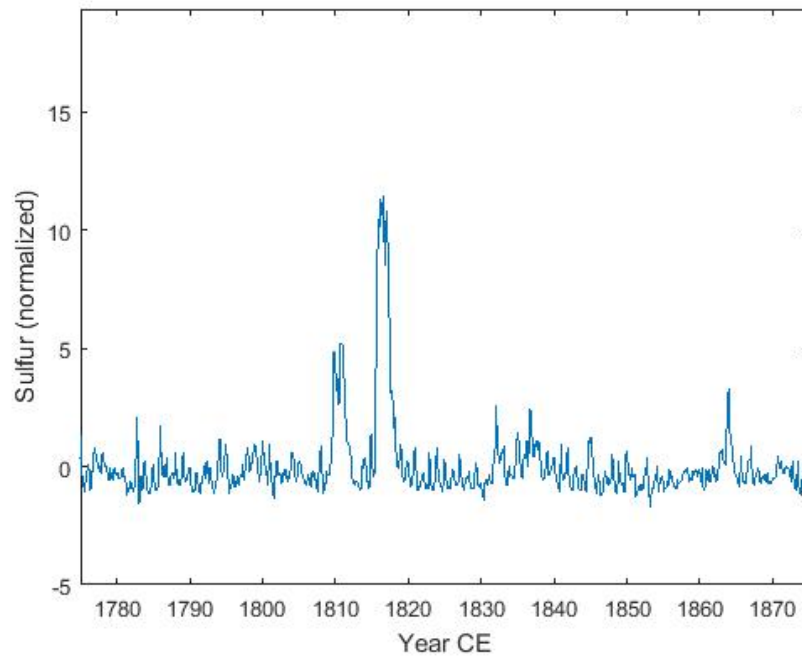


Fig. 1.