

Interactive comment on “Climate dependent contrast in surface mass balance in East Antarctica over the past 216 kyr” by F. Parrenin et al.

Anonymous Referee #1

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Based on a volcanic synchronization of the Dome C (EDC) and Dome Fuji (DF) ice cores, this paper describes the evolution of relative surface mass balance (SMB) at the two locations over the last 216 kyr. The SMB ratio is calculated from relative layer distances in the two cores, and corrected for ice flow thinning using previously-derived thinning functions. The resulting SMB ratios are compared to isotope-derived SMB ratios. A companion paper describes the synchronization of the two ice cores, and a comparison of their timescales.

General comments:

The synchronization between EDC and DF will benefit e.g. the development of future

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ice-core chronologies. This paper describes interesting perspectives in using such synchronization to also obtain relative SMB ratios for two locations, and it reaches important conclusions regarding the past relative SMB changes for two locations in East Antarctica. However, as its companion paper does not stand alone very well, yet is instrumental to the conclusions reached in this paper, I recommend the authors to combine this submission with its companion paper.

The paper is generally well-written, but it has some odd sentence structures that need to be corrected, as well as some repeated sentences/sections.

Specific comments:

The SMB ratio for the two locations are found by multiplying the relative layer thicknesses in the two ice cores with previously derived thinning functions for the two core sites. However, there is some circularity in this reasoning, since these thinning functions are themselves based on estimates of past SMB, which in this case (Parrenin et al, 2007) has been calculated based on an isotope-accumulation relationship. I would like to see a discussion of this aspect in the paper, including a section describing how these thinning functions were derived and the assumptions behind them.

I would also like to see some sensitivity studies on how the assumption of various reasonable thinning functions influence the derived SMB ratios, thereby illustrating the consistency of the authors' argumentation. One option would be to use the derived SMB estimates for each of the cores to re-estimate a thinning function, and use this for calculating new SMB ratios. The new and old SMB ratios should hopefully be relatively similar. Such exercise would help to show that reasonable changes in the thinning function do not impact the conclusions of the paper. This exercise is particularly important since figure 5 in Parrenin et al (Clim. Past, 2007) shows that the employed thinning functions for EDC and DF have a lot of small-scale structure, which potentially could influence the derived SMB ratios in intricate ways.

This further allows for interesting insights into how these changes in SMB ratios will

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affect the resulting timescales for the two cores. This would tie in nicely with some of the argumentation in the companion paper regarding timescale differences for the two cores, since they here conclude that changes in surface mass balance must be driving errors in the glaciologically-derived timescale AICC2012.

One of the arguments that the authors use to conclude that the vertical thinning functions are robust is the inferred negligible trend over time in the SMB ratios. A suspicion that comes to my mind in this regard is: Wouldn't that always be the case, given that the timescales of the two ice cores are produced to fit specific age markers in the cores?

Given that the authors conclude that the thinning functions are reliable, they would be able to infer not only the ratio of SMB between the two locations, but also the SMB for the two locations. There may be reasons to believe that the relative SMB ratio is less sensitive to errors in the employed thinning functions (after all, the relative thinning function shown in figure 3 is quite smooth). Nevertheless, it would be interesting to see also the derived accumulation rates for the two location, and a discussion of this topic in the paper.

The coefficients in the isotope-accumulation relationship are chosen to produce accumulation rates as close as possible to the published accumulation rates (P. 385, line 15). It would be a valuable exercise also to compare directly to these published accumulation rate histories, which provide age scales for the cores.

I suggest the authors to also display a secondary record showing the difference between the synchronized SMB ratios and isotope-derived SMB ratios. Including such data in figure 4 would make it easier to follow the authors description of percentage-wise similarities/differences of the two SMB ratios, as well as it may highlight smaller sections which differ between the two, which currently do not stand out very well based on figure 4.

In general, I recommend the authors to be very clear in the language regarding the various SMB ratios. It can be a bit confusing to figure out when they discuss variations

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in SMB ratios over time, versus differences between synchronized SMB ratios and isotope-derived SMB ratios. The new aspect of this paper is the periods where the synchronized SMB ratios does not follow the isotop-based SMB ratios, which occurs during very specific and short periods.

Technical corrections:

The authors use the word “age gap” throughout the text. To me, an age gap refers to a missing section of a core. The correct term to use here would be “age difference” or “age discrepancy”.

Where does the moisture at EDC come from? According to p. 383, it is coming almost entirely from south, e.g. from the continental inland, whereas it says on page 380 that: “moisture predominantly from the Indian Ocean sector”.

Further, as this paper is dealing with Antarctic ice core data, I suggest that authors refer to time periods in terms of AIMS, instead of Marine Isotope Stages.

P. 379, line 4: “sea level” -> “sea level change”

P. 379, line 8: the SMB is “larger” during warm periods (and “smaller” during cold periods), not “increasing”/“decreasing”.

P. 379, line 18: “not closely . . . changes” -> “not reflected in the isotope profiles”

P. 380, line 19-23: Shorten this section. Lines 19-20 provide the same information as lines 20-23.

P. 381, line 2: Using both “product of” and “multiplied with” is a repetition. Delete one.

P. 381, line 2: add: “thinning function” -> “thinning function accounting for the changes with depth caused by ice flow”

P. 381, line 4: “of ice” -> “of the ice”

P. 381, line 8: “applied for” -> “applied to obtain past estimates of SMB for”

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P. 381, line 8: “correction of water” -> “correcting the water”

P. 381, line 9: “and” -> “and/or”

P. 381, line 12: “constrains on” -> “constraints from”

P. 381, line 12: delete “relative”

P. 381, line 14, 23: remove “independent” (what is it independent from?)

P. 381, line 16: “in stratigraphy” -> “in the ice-core stratigraphy”

P. 381, line 16: “which are often represented by” -> “such as”

P. 381, line 17: the sentence “the ice-equivalent. . . markers” can be removed without loss of information.

P. 381, line 19-21: “The thinning function. . . TALDICE.” I suggest to move this sentence to the following paragraph (starting line 27) dealing with the estimation of SMB from DF and EDC.

P. 381, line 27: “propose a new approach” -> “use a similar approach”

P. 381, line 29: delete “based on . . . stratigraphies” (is repeated in the next sentence)

P. 382, line 1: “from” -> “at”

P. 382, line 4: “relative changes in ice thicknesses covering” -> “changes in the relative thickness of the ice core sections covering”

P. 382, line 4: “between” -> “in”

P. 382, line 6: “SMB pattern” -> “relative SMB pattern”

P. 382, line 14: “peaks of gabs” -> “largest discrepancies”

P. 382, line 28: remove one “are”

P. 382, line 24: “two 2000 km. . . East Antarctica” -> “two remote dome summits . . .”

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East Antarctica located 2000 km apart”

P. 383, line 2-3: "Present Dome Fuji. . . SMB" -> "Presently, Dome Fuji shows a spatial gradient in SMB"

P. 383, line 22: "effects to" -> "effects on"

P. 383, line 13-23: This paragraph can be shortened and/or moved to the introduction, where some of this information is already provided.

P. 384, line 19: "tiepoints were re-interpolated every kyr": Please explain what is meant by re-interpolating the tiepoints.

P. 385: Section 2.3 could use a little more explaining of the various terms in the equations, values used etc. This is especially the case for the corrections due to changes in source temperature over time.

P. 385, line 4: "mass balance are" -> "estimates of mass balance can be"

P. 385, line 6: Add a sentence of why it is necessary to correct for the isotopic content of the ocean and to distinguish site and source temperature changes.

P. 385, line 13-14: It is not clear from text what is meant by $\Delta\delta D_{corr}$, and how the value provided in parenthesis for the value of A_0 should be interpreted. The same is the case for $\Delta\delta d_{corr}$ in equations (4) and (5).

P. 386, line 1: Which are the coefficients referred to here: is it both gamma and beta, and both for source and site? What are the values used? Perhaps provide these in a table.

P. 386, line 20: Provide a plot of δD values in figure 2 for easy comparison of the two.

P. 387, line 1: This correlation (when it exists) - and the comparison between isotopes and accumulation rates in general – is hard to see from figure 4. I recommend to generally improve the layout of this figure.

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P. 388, line 23-26: I have a hard time believing that simply by looking at the shape of internal isochrones around the domes, one would be able to infer whether or not the two employed thinning functions are correct, and whether the glacial layers are thinned more than expected. Please clarify/expand.

P. 389, line 13, 16: “at both sites” -> “between the two sites”. I found it difficult to read this section, but with the above replacement, I think that the correct meaning is obtained.

P. 389, line 25: A prominent exception from this rule is also found in the WAIS Divide core over the last glacial transition (WD community members, 2013). Please add this reference.

P. 389, line 24: The difference in SMB ratio is not primarily during the past interglacial periods, but by the end of the last interglacial (MIS 5d), which is the main period that the synchro-based SMB ratio differ from the isotope-derived SMB ratio.

P. 391, line 7: Why would (3) precipitation intermittency and seasonality and (5) snow-vapor exchanges affect the relative SMB rate over longer periods of time? It would affect isotopes, and thus isotope-derived estimates of past SMB, but I don't see why would it affect actual SMB rates. Please explain or remove.

P. 391, line 19-20: SMB's dependence on bedrock topography must be irrelevant in this context, as the bedrock will be (very) stable, and thus should not provoke changes in SMB rates at a specific location over time.

P. 393, line 2: “far away”-> “over large distances”.

P. 392, line 21: “evidence” -> “shows”

P. 392, line 21: “do not scale” -> “do not always scale” (most of the time, they scale quite well!)

P. 392, line 27, P. 393, line 2: “both” -> “the two”

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P. 393, line 4-5: These age scale differences are not (currently) a part of this paper, but in the companion paper, and do not add anything to the conclusion of the paper in its current format.

Figure 1: I suggest adding to the map the dominant wind directions for the two ice core locations, since this will ease the understanding of the arguments in the text.

Figure 2: A bar plot of the number of tiepoints within each 1000 year period (instead of tiepoint density) would be a more intuitive way to display this data. Add plot of δD to figure. τ (in legend) has not been introduced in manuscript. Y-axis label of upper panel is difficult to read and understand.

Figure 4: It is not immediately apparent what is what on this figure. Instead of having labels on top, these should be added as text next to the various data series. Which “modelled” SMB is plotted? The ocean-corrected or source-corrected one? Y-axis label on 2nd panel should read ΔT_{site} , not just “temperature”. Is it really necessary to provide a figure of isotopes, isotope-based temperature as well as isotope-based SMB? The important part of this plot is the lowermost panel, which is not given sufficient space. Grey line on lower panel is difficult to see.

Interactive comment on Clim. Past Discuss., 11, 377, 2015.

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