Anonymous Referee #1

Received and published: 26 March 2015

First of all, thank you very much for your good and constructive review.

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 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.

Both manuscript will be revised in a consistent way. Given that several reviewers asked for more material in each paper, it seems inconvenient to combine both in a single article. Therefore, we decided to keep two separate manuscripts, the first one focusing on the volcanic match and time scale issues and the second one on the SMB implications.

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.

The assumptions are briefly described in section 2.2:
"Vertical thinning due to ice flow has been estimated both for the EDC and Dome Fuji ice cores based on a 1D ice flow model (Parrenin et al., 2007a) with a prescribed analytical velocity profile (Lliboutry, 1979) and a prescribed ice thickness evolution based on a 3D model of Antarctica evolution (Ritz et al., 2001). The thinning ratio between EDC and DF is 1 for present-day and increases up to about 1.5 at 200 kyr (high thinning ratios imply that EDC thins less than DF), which is mainly due to the fact that the ice thickness is larger at EDC than DF (Table 1). After correcting the layer thickness ratio $\Delta z_{EDC}/\Delta z_{DF}$ by the vertical thinning effects, we obtain a synchro-based SMB_{EDC}/SMB_{DF} ratio (Figure 2)."
For more details, the readers can study the Parrenin et al. (CP, 2007) article.

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.

We now write in section 4.2:
"These evidences therefore suggest that the main characteristics of our synchro-based SMB_{EDC}/SMB_{DF} ratio are not due to error in the evaluations of the thinning functions at both sites. However, we can expect some errors in the modeling of the thinning functions at both sites to affect the details of our synchro-based SMB_{EDC}/SMB_{DF} ratio. For example, as outlined in section 4.4, the error in the SMB reconstruction used at both sites to model the thinning functions (Parrenin et al., 2007a) can affect ice sheet thickness evaluation by ~70 m, that is ~2%, which translates into a 2% error in the thinning function. Also, there are some spatial variations in the ice thickness around both drilling sites that can affect the thinning functions if the ice does not perfectly flow vertically (Parrenin et al., 2004). This is however difficult to quantitatively evaluate since we do not have robust estimates of the dome movements during the past."

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

The age markers put a constrain on the annual layer thickness ratio. But we could imagine to fit the age marker for wrong reason: an error in the thinning functions could be compensated by an error in the accumulation reconstructions. Here we suggest it is not the case.

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.

We could indeed reconstruct past SMB using age markers and assuming some thinning functions. It is the kind of results that come out of federative ice core dating tools such as Datice or IceChrono (see the AICC2012 articles for more details). Unfortunately, beyond the GICC05 era (60 kyr), the quality of the age markers do not allow an accurate reconstruction of the SMBs. Our reconstruction on the SMB ratio is therefore far more robust than the reconstruction of the two individual SMBs.

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.

We added a figure (Figure 4) which compares the various accumulation reconstructions for both 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.

We are a bit reluctant to add a new plot to Figure 4 (which is now Figure 5) since it is already quite complex. We reckon this figure is good enough for the comparison of the various SMB ratios.

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 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.

We tried to clarify this comparison.

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”.

“age gaps” changed to “age differences” or “age discrepancies” throughout the manuscript.

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”.

Surface winds almost entirely come from continental inland (they are the so-called Katabatic winds) but at higher elevation, the winds that bring the moisture come from outside the continent.

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.

AIMs only refer to the millenial scale variability of the last glacial period. With Marine Isotope Stages, we can refer to interglacial periods and to other glacial periods.

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

Corrected.

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

Corrected.

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

Corrected.

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

This section has been rewritten and simplified:
“The SMB in Antarctica is also function of the surface elevation of the ice sheet (e.g., Krinner and Genthon, 1999; Takahashi et al., 1994) and is affected by the redeposition of snow by wind (Gallée et al., 2013). For example, it is known that SMB differs between the windward and leeward sides of ice divides for strong-wind events (Fujita et al., 2011). In addition, local variations in the SMB are governed by the local surface topography, which is influenced by the bedrock topography (Fujita et al., 2011). There are also clear sky precipitation events as well as exchanges of water vapor between surface snow and the surrounding air which have been highlighted in Greenland (Steen-Larsen et al., 2014) and also suggested in Antarctica (Hoshina et al., 2014). Considering the exchanges of water vapor between surface snow and the surrounding air, not only high-precipitation events associated with strong winds, but also daily exposure to prevailing wind, east at DF and south at EDC, may have significant effects on SMB, water stable isotope ratios and snow properties at these dome sites.”

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

Corrected.

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

Corrected to: “thinning function accounting for the ice flow”.

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

Corrected.

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

Suggestion applied.

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

Corrected to: “after correcting the water...”

P. 381, line 9: “and” -> “and/or”

Suggestion applied.

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

Corrected.

P. 381, line 12: delete “relative”

Deleted.

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

Independent removed. It was meant to precise that this approach is independent from the water stable isotope approach.

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

Corrected.

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

Simplification applied.

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

Sentence removed.

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.

Sentence slightly adapted and moved to the following paragraph.

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

Corrected.

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

Deleted.

P. 382, line 1: “from” -> “at”

Corrected.

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

Suggestion adopted.

P. 382, line 4: “between” -> “in”

Corrected.

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

Corrected.

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

Corrected.

P. 382, line 28: remove one “are”

Corrected.

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

Suggestion applied.

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

Corrected.

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

Corrected.

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

The main messages of this paragraph have been moved to the introduction.

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

We now explain:
“These tie points were then placed on an age scale (DFO2006 for DF and EDC3 for EDC) and re-interpolated every kyr. The ratio of layer thickness at EDC and DF of these 1 kyr-long intervals $\Delta z_{EDC}/\Delta z_{DF}$ could be inferred (Figure 2 and Figure 3).”

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.

Concerning the corrections due to changes in source temperature over time, we added the following sentence:
“SMB estimates can be refined by correcting the water stable isotope records for variations in the isotopic composition of the ocean and/or for artifacts due to changes in moisture sources using the second order deuterium excess record.”

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

Corrected.

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.

We added the following sentence at the beginning of this section to explain why we need to correct

for temperature and isotopic composition variations at the source:
"A simple Rayleigh model can link the variations of the isotopic composition of vapor in an air mass with the variations of its temperature (Dansgaard, 1964)."

P. 385, line 13-14: It is not clear from text what is meant by ΔδDcorr, and how the value provided in parenthesis for the value of A0 should be interpreted. The same is the case for Δδdcorr in equations (4) and (5).
Sentence modified to:
"From this, we derive a first reconstruction of accumulation, called *ocean-corrected*:

$$a_{oc} = A^0 \exp\left(\beta\left(\delta D_{corr} - \delta D^0_{corr}\right)\right) \qquad (1)$$

with β=0.015, δD^0_{corr}=-390.9 ‰ and A^0=3.1 ice-cm/yr for EDC and β=0.013, δD^0_{corr}=-403.1 ‰ and A^0=3.8 ice-cm/yr for DF."
A similar change has been applied for Eqs (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.

We now precise that the coefficients are γ_{site}, γ_{source}, β_{site} and β_{source}. We do not repeat the values here since they are already given in Uemura et al. (2012, table 1).

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

This sentence was not necessary. It has been removed to have a text more in phase with the Figures.

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.

The layout of this figure (which is now Figure 5 in the revised manuscript) has been modified.

P. 388, line 23-26: I have a hard time believing that simply by looking at the shape of internal isocrones 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.

This sentence has been clarified into:
"First, by mass conservation, an abnormally thinned layer at some place can only be explained if this layer is abnormally thicken at a neighboring place, but no irregularity is observed in the isochronal layers observed by ice sounding radars at DF (Fujita et al., 1999, 2012) and EDC (Tabacco et al., 1998, D. Young, personal communication)."

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.

These two sentences have been corrected:
"This is consistent with a recent estimate of 2.5°C for the difference in LGM-present precipitation-weighted temperature change between the two sites, with a larger amplitude estimated at EDC than at DF (Uemura et al., 2012). We therefore conclude that our inferred synchro-based SMB ratio change may be a consequence of a difference of precipitation-weighted temperature change between both sites."

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.

Sentence changed to:
"This feature has already been suggested during the early Holocene (Parrenin et al., 2007b), during the last deglaciation (WAIS Divide Project Members, 2013) and from climate simulations of the last interglacial (Sime et al., 2009)."

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.

", especially during past interglacial periods" changed to "especially during MIS5c and MIS5d".

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.

We clarified this paragraph:
"Different atmospheric processes may explain the variations in the SMB_{EDC}/SMB_{DF} ratio: 1) effects related to moisture sources and distillation along transport paths; 2) different glacial sea ice expansions in the Atlantic and Indian ocean sectors (Gersonde et al., 2005), enhancing accumulation and temperature changes at EDC compared with those at DF; 3) effects associated with precipitation intermittency and/or seasonality (e.g., Suzuki et al., 2013), precipitations being generally less important during cold periods; 4) less frequent blocking events at EDC (Massom et al., 2004) than at DF (Hirasawa et al., 2000) during glacial periods, these warm events being responsible of a large proportion of the total annual accumulation (Hirasawa et al., 2000) and 5) differences in sublimation, which is an important process since surface snow - vapor exchanges can alter the snowfall signal in-between snowfall events (Hoshina et al., 2014; Steen-Larsen et al., 2014)."

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.

This argument has been clarified:
"A movement of the domes can also modify the trajectories and therefore the origin of ice particles in the ice cores. Given that accumulation varies spatially, in particular due surface topographic variations related to bedrock reliefs (Fujita et al., 2011), this second process can also create a apparent change of accumulation in the ice cores."

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

Corrected.

P. 392, line 21: "evidence" -> "shows"

Corrected.

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

Corrected.

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

Corrected.

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.

The sentences related to age scales have been removed from the conclusion.

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.

Done.

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. tau (in legend) has not been introduced in manuscript. Y-axis label of upper panel is difficult to read and understand.

The number of tie points per kyr is exactly was is drawn (look at the bottom Y-axis title and a the legend). The main text has been slightly modified so it is not necessary anymore to draw the δD on this figure. The top Y-axis title has been renamed “ratio” for simplicity.

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 ΔTsite, 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.

We added labels next to the series. We now mention the SMB plotted are the source-corrected ones. The second panel really shows the site temperature, not ΔTsite. We think it is important to provide also figures for the temperature and SMB, since it gives an idea of the absolute shifts between the two sites, in present-day or glacial climates. We now give more space to the lower panel and the grey line is now a green line for better visibility.