Anonymous Referee #2

Received and published: 28 March 2015

Review of Fujita et al. and Parrenin et al.

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

Two papers by Fujita et al. and Parrenin et al. were submitted as companions. They both use volcanic matches between the Dome C and Dome Fuji ice cores to synchronize the timescales. The result is that the relative depth-age scales show considerable disagreement in certain periods likely driven by variations in accumulation rate. The two papers have slightly different foci, with Fujita emphasizing the timescale differences and Parrenin et al. exploring the accumulation relationship. They are closely related so I have written a single review for both papers.

The two papers had a lot of overlap and I think they would work better as a single manuscript. I think the Parrenin et al. paper could fit nicely as a section or two in the Fujita et al paper. Alternatively, one paper could focus on the volcanic match (see below) and one on the timescale and SMB implications.

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 new and fundamental contribution of this paper is the volcanic match synchronization between Dome Fuji and Dome C. Evaluating the robustness of the synchronization is critical to the work. Relatively little is written about the matching and only a single example of the matches is shown (Figure 2, Fujita). I will detail my concerns about the volcanic matching first and then move on to the remainder of the two manuscripts.

Event Matching The first thing I noticed is that the previous interglacial period (i.e. 120-130 ka) has about double the match points as the Holocene (i.e. 0-10 ka). This surprised me because the previous interglacial has been thinned to less than half of its original thickness which typically makes identification of volcanic events more difficult. Some of the MIS5e peaks may also become less distinct due to diffusion. I did not see any note or discussion of this interesting feature. I do not think the volcanic activity of the previous interglacial was twice as great as during the Holocene.

See the answers to the reviews of the first manuscript.

I am also confused by the process. The authors first found “major tie points” but do not describe what that means. Typically, it is the sequence of events, and not the magnitude of a single event, which best determine the tie points. The authors need to describe their method in more detail, and provide multiple examples of what constitutes a “major tie point”.

Ditto.

The other thing that struck me was that there were no other data sets used to test the matching. What about Be10 at the Laschamp event? What about geochemical fingerprinting of tephra layers? These outside data sets would provide an enormous boost in confidence to the matching of non-specific bumps in electrical conductance and sulfate.

Ditto.

The statement on F412,L5 “We note that there are no uncertainties associated with the use of different proxy records (ECM, DEP, ACECM and FIC) for the identification of volcanic events” is wrong. DC-ECM, DEP/AC-ECM, and FIC measure different things and are not always the same. The 18 ka event (Hammer et al., 1997) is the best example of this: if you had the ECM from one core and the Sulfate from a different core, the events would look completely different. The analysis needs to be more thoughtful and describe why these different measurements record the same volcanic events often enough that it is not a major problem.

Ditto.

The appendix focuses on the semi-automated method for selecting “minor tie points”. I have many questions about this method and think it might be finding lots of incorrect tie points 1) Why is the acceptable match tolerance set as a fixed distance of 0.1m when the average annual layer thickness differs down the core (by a factor of ∼5 from the surface to the depth at 216 ka for Dome Fuji)? It would seem to make more sense for the acceptable window to be scaled to the approximate annual layer thickness. 2) On line F422,L25, they write “volcanic events as rare as every ∼154 years (in average)” but in fact the 154 years is only the average occurrence of volcanic events that can be matched. In high resolution Antarctic cores for the past couple thousand years, the occurrence of volcanic events is about ten times that (every ∼15 years, e.g. Sigl et al., 2013). In fact, matches of multiple cores around all of Antarctica reveal that upwards of 80 events in the past 2000 years (up to every 25 years) can be matched (Sigl et al., 2014). A discussion of the number of events that are identified but not matched would be very useful. 3) When my concerns in 1) and 2) are combined, it seems like there is a high probability of finding incorrect links. A 0.1m tolerance, which is a 0.2m window, is a time span of about 20 years during the previous interglacial (and more deeper in the core or at colder periods). This could lead to a very high probability of mismatching.

Ditto.

As a last point, it is unclear to me what the plans are for making the data publicly available. This is ESSENTIAL so that others can evaluate the quality of the matches themselves. I could not find the Dome Fuji data which may be because this is the first publication with it. The Dome C ECM and DEP available through NOAA Paleoclimate data archive were not of the same resolution as presented here. I did not check the EDC sulfate data. The recent paper on the NEEM timescale (Svensson et al., 2013) which was dated by matching the ECM and DEP records to NGRIP has set the standard for releasing the underlying data. This is critical because anyone can make their own determination of where the matches are robust.

Ditto.

Fujita et al. My comments on the remainder of the Fujita et al. paper are rather brief. I found the writing to be rather confusing to follow. Overall, the analysis of the causes of the age discrepancy is solid (if challenging to keep track of). I think a section that reviews the basics of the timescales and the methods and age markers used to construct them would be very helpful. Those readers already familiar with the timescale construction could simply skip over the section, while a review would likely benefit the majority of readers who haven’t kept up on the details of the timescale construction.

Ditto.

The final paragraph of the conclusion makes a strong case for large uncertainties in the age markers, and hence the underlying timescales. I wish there had been a discussion of whether the age uncertainties given for the two ice core timescales (DFO2006 and AICC2012) are compatible with the work here.

Ditto.

I also wonder about the use of the term “age gaps”. This makes it sound like ice of certain ages is missing, which isn’t the case.

Ditto.

Parrenin et al. The Parrenin et al. paper focuses on the ratio of surface mass balance at the two sites. Overall, this paper seems more like an extended outline than a final manuscript. The methods are not well described and the discussion items are mostly lists of possibilities. There are only two figures that are not repeats of figures in Fujita et al. I think this work would fit well into the Fujita et al. paper since much of the start of the paper is a repeat of what’s described there and the ideas in this paper could be condensed into a nice discussion section in that paper.

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 SMB has to be reconstructed from the depth-age relationship using a thinning function. This is the synchro-based SMB ratio. They also find an ocean-corrected and a source-corrected SMB ratio based on scaling to water isotopes. These synchro-based and isotope-based SMB ratios mostly agree, except between 102 and 112 ka. I wonder if the sharp jump at ∼111 ka is indicative of the wrong volcanic match.

A wrong match cannot be completely ruled out during this time period. However, if the discrepancy between the synchro-based and isotope-based SMB ratio during the period 105-113 kyr BP was due to a wrong volcanic match, the too low values of the synchro-based SMB ratio would be compensated by too-high values after 105 kyr BP or before 113 kyr BP. But no such high values are observed. We made this argument in the manuscript:
“The robustness of the volcanic match used to deduce the synchro-based SMB_{EDC}/SMB_{DF} ratio can also be discussed, especially during the 105-113 kyr BP time interval (MIS 5c and 5d), when the synchro-based and isotope-based SMB_{EDC}/SMB_{DF} ratios deviate by as much as 20%. Given the number of tie points in this interval (95), it seems very unlikely that the volcanic match is entirely wrong. Moreover, if the low values of the synchro-based SMB_{EDC}/SMB_{DF} ratio during this time period was due to an incorrect volcanic match, it would be compensated by too high values after 105 kyr BP or before 113 kyr BP. But no such high values are observed, suggesting that an incorrect volcanic match is not the cause of the synchro-based and isotope-based SMB_{EDC}/SMB_{DF} ratios difference during this time period.”

Section 3.1 – the comparison of the SMB ratio and the dD values is undeveloped. The authors write that Figure 4 suggests a “correlation” but I don’t think the authors actually performed a statistical correlation. It should be straightforward to do.

We now write that the correlation coefficient it 0.74.

Section 4.1 – This section is quite interesting. But the theme of this section – that isotopic values, accumulation rate, and site temperature have a very complex relationship, seems to be forgotten in the following sections.

Section 4.1 discusses the present-day conditions while the following sections discuss the past.

Section 4.2 – The reliability of the thinning corrections should be evaluated in more detail. Fuller descriptions would be very helpful. Explain why a negligible decreasing trend toward the past supports the thinning corrections. Also, describe the mass conservation and the relationship to the radar data more fully. How large were the radar surveys? What spatial scales need to be considered? I would also like there to be a description of how time-varying vertical strain profiles would impact the inferred SMB. For example, both of these sites are drilled near divides, where the vertical strain rate is different than at flank sites. What is the influence of allowing different vertical strain patterns through time. Also, the basal topography is rough at EDC (I'm not sure about Dome Fuji). If the dome had migrated over higher bedrock at different times, how much might this affect the inferred SMB? If this remains a stand-alone paper, there is plenty of space for additional text and figures.

We completed and corrected this paragraph as follows:
"A first argument for the reliability of the thinning evaluation comes from the fact that the SMB_{EDC}/SMB_{DF} curve, after the thinning correction, has a negligible decreasing trend toward the past. Indeed, we do not expect the SMB_{EDC}/SMB_{DF} ratio to have a trend over several glacial-interglacial cycle. This suggests that the main trends of the thinning functions at EDC and DF have been captured by the ice flow models. The part of the $\Delta z_{EDC}/\Delta z_{DF}$ curve which varies at the glacial-interglacial scale could also be due in part to the vertical thinning, with glacial layers relatively more thinned at EDC, and not correctly accounted for in our ice flow modeling exercise. There is indeed a correlation between climate some parameters that can have an impact on ice rheology, like fabric (Durand et al., 2009) or chlorine impurities (Fujita et al., 2014; Watanabe et al., 2003a). However, this hypothesis seems unlikely for two reasons. 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 and EDC (Fujita et al., 1999; Siegert et al., 1998; Tabacco et al., 1998, D. Young, personal communication). Second, if glacial ice is softer, the relative difference in cumulated vertical thinning with interglacial ice should increase with the age of the ice layers, as is shown by mechanical simulations (Durand et al., 2007), but no such effect is observed in the $\Delta z_{EDC}/\Delta z_{DF}$ curve. Third, mechanical simulations do not suggest that ice layers with a different viscosities can lead to a very irregular thinning function (Durand et al., 2007). Fourth, the interplay between the dome movement and the different strain rates at different locations could lead to irregularities in the thinning function. Indeed, the strain regime is different right at a dome than a few kilometers downstream on a flank due to the so-called Raymond effect (Raymond, 1983) and the strain regime is also a function of the ice thickness. But there is no obvious reason why these effects would lead to a thinning function correlated to the deuterium profiles. These evidences therefore suggest that our vertical thinning evaluation at both sites is robust."

Section 4.3 – After reading Section 4.1, I'm not sure these simple scaling arguments to get a quantitative temperature change are worthwhile. Why should a constant temperature-accumulation relationship be trusted?

The bottom line is that the higher glacial-interglacial contrast of SMB at EDC compared to DF is consistent with a higher contrast of ice isotopes and of temperature as concluded by Uemura et al. (2012). The scaling arguments were given to show that the orders of magnitude of accumulation and temperature contrast difference were consistent, although we do not trust the temperature-accumulation relationship in the detail.

Section 4.4 – The last paragraph should be expanded. Be specific about how much the flux calculations, chronologies, and firn and ice sheet modeling will be affected. Are there conclusions in the published literature that are now challenged by the findings in this paper?

We are not aware of conclusions in the published literature that would be challenged by our findings.
We expanded the relevant paragraph as follows:
"Inaccurate estimates of SMB based on water stable isotope records can cause important errors for chemical fluxes reconstructions and ice core chronologies but also for firn (Goujon et al., 2003) and ice sheet (Ritz et al., 2001) modeling. For example, if the SMB evaluation at one of the two sites is wrong by as much as 20%, this would lead to a 20% error in fluxes reconstructions, in events duration and in Δage evaluation (that is, 500-1000 years error in the ice age / gas age difference). Concerning ice sheet modelling, Parrenin et al. (2007a) calculated a LGM-present changes of ~120 m in elevation and ~160 m in ice thickness at both sites, mainly due to an accumulation change of ~50%, those accumulation changes having been deduced from the ice isotopes. Therefore, a deviation of the accumulation reconstruction of ~20% as is observed at ~110 kyr b1950 could correspond to a ~50 m error in elevation change and ~70 m in ice thickness. Taking into account a vertical gradient of temperature of 1°C/100 m, such ice sheet thinning would lead to an underestimation of the magnitude of temperature decrease "at fixed elevation" by approximately 0.5°C."

Section 4.5 – The inference of possible impact on site temperature reconstructions is too simplified. First, this section should be integrated with the following section where the complexities of ice elevation change are discussed. But is a 50m or 0.5C change even significant given the other uncertainties in the calculations of past temperature change – i.e. isotope/temperature scaling (since borehole thermometry cannot be used).

This section has now been incorporated to the previous section 4.4 which deals with the consequences of error in the SMB estimates.

Section 4.6 – It is an interesting list of possible atmospheric reasons, but is there any way to test them? Are there measurements already completed, or that could be made, to distinguish between possibilities? The paragraph on the dome position is also too simplified. What is the spatial pattern of SMB across the domes? What is the spatial relationship between SMB and isotopes across the domes? I don't follow the logic why a migrating dome could not have an impact.

We don't see what measurements could be made to distinguish between these 3 hypotheses. We believe that new insights will come from the modeling (of ice sheet and of atmospheric processes). We now acknowledge in the conclusions that: "... new simulations of Antarctic ice sheet evolution using a new generation of ice sheet models with a realistic representation of grounding line migrations (e.g., Pollard and DeConto, 2012) will allow to explore the movements and elevation variations of the domes.". We also write in the conclusion "There is also a need for more accurate atmospheric models able to reproduce the measured present or past spatial pattern of accumulation.".
Concerning the dome movement, it seems unprobable that this mechanism would provide a SMB ratio so well correlated with the isotope records. We therefore write:
"A movement of the domes (see Saito, 2002 for the movement of DF during the past) could therefore create an apparent change of accumulation in the ice core records. In this case, we would not expect any constant relationship between water stable isotopes and accumulation rate, except if the dome movement is itself correlated to processes affecting the isotopic composition of water vapor and precipitation (e.g. via sea level changes and grounding line migration)."

One last comment, why is the companion paper referred to as “Fujita, Parrenin, et al.” when all the other references use only the last name of the first author (i.e. Church et al.)?

The companion paper is now referred to as “Fujita et al. (In revisions)”.