We have appreciated all the referees constructive comments on our manuscript. In the following we reply to some of the main points that two or more referees have raised. Then, a point-to-point response of the problems highlighted by each referee is given.

Best regards,

Shuji Fujita and Frédéric Parrenin on behalf of co-authors

RESPONSE TO MAJOR COMMENTS FROM REFEREES

For the major points of comments we use ID from M1 to M7 as follows.

M1. Online release of the data and the code

Several referees requested us to show graphs of the suggested tie point data. Also, they requested us to make the raw data of ice cores accessible. We list here how we responded.

1.1 Tie point list

A list for depths of the extracted tie point (DF depths vs. EDC depths) are provided as one of supplementary materials of the revised version. It is Supplementary material C.

1.2 Figures showing suggestion of the tie points

Figures showing suggestion of the tie points are provided as one of supplementary materials of the revised version. It is Supplementary material A. We provide in all 79 sets of figures. The figures cover entire depths range of the volcanic synchronization.

1.3 Data of the EDC cores

1) DEP data of the EDC96/99 cores with 2 cm resolution is already publicly available at National Centers for Environmental Information (NCEI). http://www.ncdc.noaa.gov/
Online resource : https://www.ncdc.noaa.gov/paleo/study/12948

2) ECM data of the EDC96/99 cores with 1 cm resolution is already publicly available at National Centers for Environmental Information (NCEI).
Online resource : http://www.ncdc.noaa.gov/paleo/study/18535

3) Sulfate data of the EDC96/99 cores
Sulfate data have not been submitted to a public database, but they are sent on request by M. Severi. Readers can also see many examples in the Supplementary material A.

1.4 Data of the DF cores

ECM data and ACECM data of the DF1 core with 1 cm resolution are already publicly available at National Centers for Environmental Information (NCEI).
Online resource :
http://www.ncdc.noaa.gov/paleo/study/18675
http://www.ncdc.noaa.gov/paleo/study/18676

As for ECM data and ACECM data of the DF2 core, data have not been submitted to a public

database, because the production of data sets are still halfway; we still need to carry out a lot of additional measurements. Readers can see all examples used for the synchronization in the Supplementary material A. In addition, they are sent on request by S. Fujita.

1.5 Code of the PC interface

We provide the code as one of supplementary materials of this paper. It is Supplementary material B. Explanations are also provided with it.

M2. Suggestion to combine two papers into one

Among five referees, three (Referees #2, #3, and #4) suggested a possibility that the two papers should be combined. On the contrary, two referees did not suggest this possibility.

Referee #2 gave a comment as follows.
Thus, the manuscript presented here has a very strong methodological focus on the evaluation of timescales with very limited direct implications for paleoclimatology. ...
A major concern I have, however, is that the overall significance of the analysis does appear too limited to justifying a stand-alone publication. If this was written more concise I could easily see these results incorporated into the methods section of the companion paper.

Referee #3 gave a comment as follows.
Synchronizing ice cores is an important task, and the synchronization performed here will be of benefit for ice core science. However, I feel that this paper doesn't stand on its own very well, and it fails to reach any substantial conclusions. I therefore suggest the authors to combine this submission with its companion paper (inference about previous changes in SMB based on this synchronization).

Referee #4 gave a comment as follows.
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.

In terms of the separate publication, referees #1 and #5 did not suggest to combine the two companion papers. The referee #5 encouraged us to keep the separate papers, by giving a comment as follows.
The results are rather technical and of interest only to a rather limited readership, but on the other hand, having good timescales for the two ice cores is an objective of considerable importance and of general interest that cannot be met without publishing the nitty-gritty details going into a time scale. Ways to make the manuscript more significant, and thus more strongly justify publication as a separate manuscript rather than as a technical section of another paper of, could be ....

Our views for our choice of publishing two companion papers are as follows.

We do not agree with comments that this work has very limited direct implications for paleoclimatology. Our work allows to compare different age scales from different ice cores and to

combine them. Such work on ice core chronologies is essential for several reasons. For example, if we have errors in both timing and duration of glacial and interglacial periods or Marine Isotope Stages (or AIMs), the possible errors from ice core chronology will be widely propagated to other paleoclimatology studies that use (directly or indirectly) AICC2012 and/or DFO2006. Climate modelers' concern is relative duration of glacial and interglacial periods, which directly influence their modeling of the earth climatic change. Thus, it seems natural that synchronization work is discussed in an independent paper.

If we combine two papers into one, it will be a lengthy, unfocused paper, which we decided to avoid. It is natural that we must explain methods and procedures nicely, as referee #5 suggested us to provide such information in more detail. The comparison of the age scales also makes this manuscript not an only methodological one.

Moreover, we now include a discussion on the phasing of the EDC and DF isotopic records, made possible with the volcanic synchronization. Furthermore, we include a discussion on the comparison with the tephra stratigraphic link. Please see new Sections 4.3 and 4.4. Such discussions give more significance to the current manuscript.

To better clarify these points to readers, we increased related statements in abstract, introduction and in concluding remarks.

M3. Robustness of the suggested tie points

A few referees (e.g., Referees #1 and #4) raised a question about the robustness of the tie points. We agree that our explanations were insufficient regarding this aspect. We provided additional explanations in the revised paper, as we explain below.

When at least one peak event is found at a timing within several kinds of Dome Fuji core signals (that is, DF1 ECM, DF1 ACECM, DF2 ECM or DF2 ACECM), we can mark the presence of a volcanic event at that timing of the core. Similarly, when at least one peak event is found at a timing within several kinds of EDC core signals (ECM, DEP or sulfate), we can mark the presence of a volcanic event. If we find at least one peak signal in multiple core signals at the same timings, it means that volcanic event happened at that timing.

We did use the height of the peak signals for identification of the prominent signals in the stage of initial survey to find the ~650 major tie points. In the CPD paper, we wrote that we did not use the peak height, which was imprecise. Prominent peak signals were often commonly observed in multiple kinds of ice core data from multiple sites. However, the height of the peak signals were often highly variable due to spatially and temporally heterogeneous depositional conditions by winds on the surface of the ice sheet. Thus, at the stage of the second survey to extract as many plausible minor tie point peaks as possible, the height of peaks were not major criteria to choose or reject the candidate peak signals. Regardless of height of the peaks, we can easily identify candidate peaks from ice core signals (please see examples in the Supplementary material A) considering their patterns of appearance (pattern matching). In the paper, we stated "When the patterns of data fluctuations agreed between one or more sets of data at DF and EDC, they were extracted as tie points with confidence". We can justify this procedure because at each depth range, we found only unique matching pattern. We never find alternative patterns of matching through

inspections that we repeatedly performed. Thus, regardless of the height of the peaks within the matching, we could use even small peaks as a confident tie point.

We now gave more explanations in the method section (2.2).

M4. A few referees (#2, #3 and #4) asked whether or not peaks in ECM signal, peaks in DEP signal and peaks in sulfuric acid is fully interchangeable. Points are as follows.
(i) Are there no sulphate species present in ice that are not from volcanic eruptions?
(ii) What about volcanic HCl and marine DMS?
(iii) Does the high (alkaline) dust loading during the glacial affect the electrical measurements by neutralizing volcanic acids present in snow?
(iv) 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.

These are important points. In inland plateau of East Antarctica, layers of high concentration of sulfuric acid are often observed both in interglacial and glacial periods. In the supplementary material, we can see that spikes of sulfuric acid are in most cases synchronized with peaks of ECM, DEP and ACECM. Meaning of electrical conductivities measured by these techniques have been discussed (e.g., Wolff , 2000, Fujita et al., 2002b and 2002c). Acid is clearly a cause of electrical conductivities measured by these techniques. In case other kinds of acids dominate such as HCl or HF, such peaks should be also detectable. However, in ice physics, these ions are known to diffuse/migrate rapidly in ice (as vapor and liquid in firn, and as solid in ice). Thus, such signals should be smoothed out relatively rapidly after deposition. Products of DMS oxidation (sulfate and MSA) can affect electrical signal of ice cores, but mostly for ice cores drilled near the coast. In the present case, Sharp peaks are all attributed to volcanic origin. High alkaline dust loading during the glacial can affect the electrical measurements by neutralizing volcanic acids present in snow in the northern hemisphere such as Greenland. In the plateau region of East Antarctica, effect of such dust loading to electrical properties does not erase acidity peaks of volcanic eruptions. Ice is basically acidic.
We agree that there are rare cases of volcanic layers characterized by HCl and HF as the 18 ka event found in the Byrd Station core by Hammer et al.(1997). However, signal of this eruption, presumably occurred in West Antarctica, was not found yet in Dome Fuji core despite our large efforts to search for it through chemical analysis. In EDC core, the signal was found. But It is much weaker than it is in West Antarctica. Generally volcanic eruptions at West Antarctica is rare source of ECM peaks signals at least at Dome Fuji (Please see M5 below).
In addition, as for ECM, DEP and ACECM data of the DF core and EDC core, when there are peak signals, we have not seen examples caused by non-sulfate reasons. Moreover, in the Supplementary material A, please see that sulfate peaks and other signals (ECM and DEP) have very good correlation over wide depths. Overall, we can justify that we can use an approximation that peaks in ECM signal, peaks in DEP signal and peaks in sulfuric acid are commonly useful as signals from large volcanic eruptions. We added more explanations in the method section.

M5. Origin of volcanic eruptions. Referee #1 asked whether the majority of signals are from eruptions of West Antarctica or not.

We would like to highlight this point because this problem is closely related to M4 above.

As for origin of peaks of sulfuric acid found in East Antarctica, there are many example of papers. A list below shows origins of volcanic eruptions of ice core signals found in East Antarctic firn cores over the period of the last 800 years.

Year, Volcano, and location are listed.
1991 Hudson Chile ,1991 Pinatubo Philippines ,1886 Tarawera New Zealand,1883 Krakatau, Indonesia, 1835 Cosiguina, Nicaragua, 1815 Tambora, Indonesia, 1762 Planchon-Peteroa, Chile, 1673 Gamkonora, Indonesia, 1641 Parker, Philippines, 1600 Huaynaputina, Peru, 1595 Ruiz, Colombia, 1452±10 Kuwae, SW Pacific, 1330±75 Cerro Bravo, Colombia, 1257 Samalas, Indonesia

We can see that they are from volcanoes located in the low latitudes of either hemisphere or volcanoes located in the high latitude of southern hemisphere. Therefore, the peaks of the sulfuric acid represent volcanic eruptions in a wide range of regions on the earth. West Antarctica is close to East Antarctica. But volcanoes of West Antarctica should have minor proportion within the huge number of sulfuric acid peaks. One of such examples is the 18 ka event (Hammer et al., 1997), which was not found in Dome Fuji ice core despite a large effort to search for it. In addition, we cite here a statement by Cole-Dai et al. (2000). "Volcanic signals found in an Antarctic ice core can be either from volcanoes located in the middle southern latitudes (e.g., South America and the South Pacific) and the high southern latitudes (the Antarctic continent and the subantarctic islands), or from volcanoes located in the low latitudes of either hemisphere. Additionally, a low-latitude eruption must be sufficiently explosive to inject volcanic materials directly into the stratosphere in order for its aerosols to be transported to the polar atmosphere and deposited in Antarctic or Greenland snow."
We added information to the main text, in the method section (2.2).

M6. Questions about radar isochrones and reliability of the two employed thinning functions (Referees #2 and #3)

Referees #2 gave a comment as follows.
Page 418: L. 1-2: What parameters in the ice form the isochrones visible in the radar? Dust? How do you then link the O_2/N_2 age markers to the radar profiles? Do the radar soundings have the resolution and dating accuracy to detect the "climate events" discussed in the manuscript? How are they matched to the ice cores?

Referees #3 gave a comment as follows.
P. 418, line 1-2: I have a hard time believing that simply by looking at the shape of internal isochrones over these large distances, one would be able to infer whether or not the two employed thinning functions are correct.

With ice radars, we can see isochronous features caused by dielectric properties of the layered structure within the ice sheet, just like ECM, DEP or ACECM data. Sulfuric acid in known to be one of major causes of radio echo isochrones. In the context of the paper, we discussed a possibility of spatially heterogeneous deformation within the ice sheet. According to the concept of conservation of mass, a thinner layer (due to softer ice) at one location can only be compensated by a thicker layer in a neighboring location. Otherwise, basic principle of conservation of mass is violated. In the radar data, we did not find any such indications. Radar with a pulse width of ~250

nano second (as a very conservative case) have resolution of about 21 m, which is still sufficient to see spatial variability thickness of MIS 5.

As we wrote in the paper, enhanced thinning at some limited depth range must appear as thickening at surrounding ice. Occurrence of such phenomena seems unlikely near dome summit of Antarctica. At least we have not seen such indication in radar data. Radar data with high depth resolutions show beautiful near-horizontal layers. From a view point of conservation of mass, particular thinning at some depth range seems unlikely.

250

M7. Language

Several referees suggested a need of improvement of paper in terms of language. Text was proofread by a native English speaker (http://www.forte-science.co.jp/english/) before submission of the CPD paper. Apparently the editorial handling done was not enough, we apologize for this lack of accuracy. To enhance the readability of the paper, more attention was paid on the English formulation. A native English speaker coauthor checked English for the revised paper. Text was proofread once again by a native English speaker (http://www.forte-science.co.jp/english/) before revised submission.

260 ---

RESPONSE TO COMMENTS OF THE REFEREE #1 (C57–C63, 2015)

[R1 #1]

Fujita et al. describe the synchronization of the Dome Fuji and Dome Concordia ice cores using volcanic tie points derived from electrical conductivity data. They use this synchronization to investigate age differences between different Antarctic chronologies. Their main conclusion is that in glaciological modeling of vertical ice flow the surface mass balance (SMB) during MIS5e is probably overestimated, which leads to age errors in glaciologically-derived ice chronologies.

270

We stated in the manuscript, synchronization between the DF core and the EDC core means that the O_2/N_2-based age of the DF core can be examined in terms of the latest chronology commonly used for Antarctic cores, namely, AICC2012. In addition, several time scales, DFO2006, AICC2012, EDC3, DFGT2006 and ages of the speleothem record from China, were compared in detail, which is a major step toward improving our understanding of the chronology of Antarctic ice cores. We hope readers to see that, a crosscheck of age markers and various chronologies brought us new insights into the chronologies of deep ice cores. We claim that, this paper has broad importance in the paleoclimatology covering a long period of time ~216 kyr. In the revised paper, we highlighted MIS 5b and 5d more than the CPD version. We identified complex sources of errors.

280

The science presented here is sound. The conclusions are of interest to the ice core community, and will have implications for dating of Antarctic ice cores and the next generation of Antarctic ice core chronologies.

[R1, #2]

No direct tephra matches are provided, and volcanic synchronization is based on the matching of ECM patterns.

In the revised version, we assessed four examples of the tephra stratigraphic link based on the volcanic acid peak stratigraphic link. Please see new subsection 4.4 and concluding remarks (item xi).

In the CPD version, no direct tephra matches were discussed because presence/absence of tephra layers in ice cores are affected by local locations of volcanoes within Antarctica or near Antarctica. For example, please see Figure 4 and Table 3 in Narcisi et al. (2005). They compared tephra layers of three cores (DF, EDC and Vostok).Between ice cores, tephra layers that were identified from the same origins were rare. Thus, using tephra layers in this study causes complications of discussions. Tephra layers are useful at most a few or several cases. In addition, there is a lisk of making a wrong link when we use isolated an layer. Please see new section 4.4 on this point.

[R1, #3]

My main (and only) concern is that the manuscript lacks a robust evaluation of the volcanic match point identification. No quantitative criterion is given for assigning or rejecting potential tie points. I do not mean to imply that the synchronization is incorrect; I just think the authors could have been more thorough in vetting their results. Some additional information is available in the appendix, but there is no link to the appendix in the main text (which heightened my concern upon the first reading). I will make some suggestions for improving this aspect of the study in my comments below.

Please see M1 and M3 above. A link to Appendix B is given in Page 6, Line 22.

[R1, #4]

The manuscript suffers from grammatical errors and unusual phrasings throughout. At least one of the authors is a native speaker, and I recommend he thoroughly edits the manuscript for language.

Please see M7 above.

—- detailed comments —-

1) As mentioned above, my main concern is that it is unclear how robust the individual volcanic tie points really are. Some details are provided in Appendix A, but the description is not satisfactory and mostly qualitative in nature. The volcanic synchronization is the main contribution made by Fujita et al. (2015), as well as the basis for their analysis. Therefore it should be thoroughly tested and described. I make some suggestions for improving the clarity of the text, and for some additional tests that could be used to investigate the robustness of the result.

[R1, #5]

1A) it is not clear how a single "event" or "peak" is defined. Does it need to be observed in both DF1 and DF2 (or at Dome C, in both EDC96 and EDC99)? Does it need to be seen in ECM. AC-ECM, SO4 and DEP, or is it sufficient to be observed in only some? In Figure 2 the prominent peaks in the left of the figure are clearly not observed in all 8 windows. Also, what does it mean to have a "significantly observable" peak? (P423,L3). E.g. 4 times above the standard deviation of the background noise?

We explained it in M3 above. We carefully saw matching of patterns, rather than peak height.

[R1, #6]

1B) On Fig. 2, could you please indicate all events that were selected as part of the 1401 tie-points? This will help the readers see visually how robust these patterns are. I also recommend you add another figure(s) with several more representative ECM time series at DF and EDC, together with the selected tie-points. This will give the reader a sense of how robust these matches are. A number of such figures could be included in the supplement, to keep the size of the manuscript concise. The more figures the better, as this will allow the readers to judge the validity of the selected matches for themselves.

Please see M1 and M3 above. We provided such a figure as new Figure 2. In addition, all examples of the tie points are shown in the Supplementary material A.

[R1, #7]

1C) P423 L18: "if we find a volcanic signal in one core but not in expected depth in another core, we just ignore such single signal and nothing is recorded. Thus, lone peak is not any source of error". I do not support this argument per se. Since the most proximal volcanoes are in West-Antarctica (presumably), there is not much of a local signal and most volcanic layers should be recorded in both cores. The argument of the authors is only valid if such unmatched peaks are very rare. If they are common, the absence of a peak at the expected depth could also indicate that the cores are out of sync. Please provide some statistics on the unmatched peaks. How often do they occur in the various cores? What percentage of ECM peaks are unmatched? Etc.

As for origin of volcanic eruptions, please see M5 above. Meaning of lone peaks is as follows. First, please see M3 above. Due to irregularity of depositions, it is known that thickness of one year or more deposition is sometimes completely absent in plateau region of East Antarctica (e.g., Kameda et al., 2008; Koerner, 1971). In the present condition of the Holocene, a probability for the complete absence of annual layer is more than 8% at Dome Fuji. This fact also means that thickness of annual deposition is often much smaller than thickness of averaged annual accumulation rate. We assume that a probability for the complete absence of annual layer at EDC is similar. In addition, in glacial periods when annual accumulation rate is much smaller, this probability should be larger than present. Therefore, lone peaks can occur naturally when we compare two different cores even when two cores are drilled within ~50 m such as DF1 core and DF2 core (or EDC96 core and EDC99 core).
Statistical assessment of lone peaks and peak height will be useful to assess past depositional environment, as it was done by Barnes et al. (2006). However, we think that statistical analysis for this is beyond the scope of our paper.
We added information above to a section of Appendix B.

[R1, #8]

1D) The computer program used in the synchronization is not well described in the text, and from Figure 2 the readers cannot find out how it works internally. Is there a reference for the computer code? Could you provide some more details on how the 1401 tie points were extracted?

We provided information on how the program works in the Appendix A. A user guide of the PC interface and the code is also provided in the Supplementary material B.

[R1, #9]
1E) Are there independent records that could be used to validate the synchronization? For example, high-resolution dust records should also record concomitant variations in both cores.

At the moment, there are no continuous high-resolution dust data or ion data on the Dome Fuji core, enough to validate the synchronization.
Instead of independent records such as dust records, the authors of this paper, from different laboratories (Fujita and Parrenin) reviewed the entire results of the synchronization on the graph (such as graphs of the Supplementary material A) independently. They agreed that there were no wrong pattern matching between two cores. Only disagreement between two people was whether or not to see several tiny peaks as tie points. Nevertheless, the choice of several tie points (to employ them or not) within the matched patterns will not affect analyses and claims in this paper.

Also, please see new subsection 4.4 about comparison with tephra stratigraphic link.

[R1, #10]
1F) figure A3 is the most quantitative of all figures, yet it still is hard to evaluate the robustness from that figure (the log scale does not help). Judging by eye, it seems there are many cases where the difference between Δz_{DF} and Δz_{EDC} is larger than 0.1 m, contrary to the claim by the authors. I think it would help if you could show histograms of both $\Delta z_{DF} - \Delta z_{EDC}$ and also $\Delta z_{DF} / \Delta z_{EDC}$.

A purpose of Figure A3 is to demonstrate good continuity in fine (0.1 m) scales between adjacent tie points. Because each panel covers wide depth range of each climatic stage, when Δz_{DF} and Δz_{EDC} is much larger than 0.1 m, effects from variable gradient of $\Delta z_{EDC}/\Delta z_{DF}$ appear on the graph. Thus, the differences between Δz_{DF} and Δz_{EDC} at larger Δz_{DF} and Δz_{EDC} are not errors.

It seems to us that histograms of Δz_{DF}-Δz_{EDC} or $\Delta z_{EDC}/\Delta z_{DF}$ will be affected by variable gradient of $\Delta z_{EDC}/\Delta z_{DF}$.

We hope readers to see continuity of the tie points in Figures 3 and A1. This continuity more or less supports the reliability of the chosen tie points.
However, there is a possibility that one wrong choice of tie points causes successive wrong choice of tie points (see [R3 #55]). To avoid this kind of successive errors, we tried to choose tie points as patterns of depth-dependent variations. This pattern recognition was indeed very important.

Another way to evaluate the robustness of the tie points are to examine the Supplementary material A. We hope that readers judge by examining it. Please see M3 above also, as for our claim of robustness.

[R1, #11]
2) Mention the companion paper (Parrenin 2015) somewhere in the introduction, and describe briefly how the two manuscripts are related.

We added a statement as below at the end of the introduction.
"In addition, Parrenin et al. (in review) reconstruct the past changes in the ratio of surface mass balance (SMB ratio) between DF and EDC sites, based on the DF-EDC synchronization in this

paper, and on corrections for the vertical thinning of layers."

[R1, #12]
3) Your explanation for the absence of events during cold periods is not completely clear to me (P413). Do you mean to suggest that during low accumulation periods the snow surface gets reworked by wind scouring etc, which removes the distinct volcanic layers? Please explain.

We mean to suggest so. This was also explained in M3 above.

[R1, #13]
4) In section 4.2: The discussion on the speleothem ages interrupts the discussion of the SMB anomalies, which makes it harder to follow the narrative of the paper. The text between P419 L10 and P420 L8 can be placed in its own, separate section. For example, make a new section 3.4. Alternatively, you can place this section between the current sections 4.1 and 4.2. I think this would improve the structure of the discussion.

We agree with the suggested problem. We placed this section between the previous sections 4.1 and 4.2, because the section contains more discussions than results. Section 4 was rearranged.

[R1, #14]
5) For figure 4: I assume there are 2 O_2/N_2 tie points per precessional cycle. It appears that the age difference oscillates, and is larger for the even numbered constraints and smaller for the odd numbered constraints. This pattern is quite consistent (only event 7 seems to deviate from this pattern, but that one is perhaps overwhelmed by the big MIS 5 anomaly). Could this mean that the SMB anomalies you identify also occur on precessional timescales? The limited number of tie points makes this speculative, of course. It may be worth pointing out.

Data look like the referee #1 pointed out. However, considering the confident intervals, it seems premature to suggest this. We hope that we can observe this point carefully when O_2/N_2 tie points with better quality is produced in future.

[R1, #15]
6) Please indicate where the ECM data and volcanic tie points can be accessed. The match points (depths) should be included as a supplementary data file. Ideally the same would be done with the ECM data also.

Please see M1 above.

—- language/technical —

[R1, #16]
Suggested corrections marked as *XXX*
Throughout the MS the authors use the phrase “age gap” to refer to the differences between chronologies; I recommend changing this to either ”age difference” or “age offset”. Similarly, “dating scale” should be “age scale”, throughout.
P408 L5: DFO2006, *a* chronology for the DF core *that strictly follows O2/N2 age constraints*, ...
P408 L14: glaciological *approach that is more weakly constrained * by age markers.

P409 L8, L21: *age* scale

These were changed as suggested.

[R1, #17]
P409 L28: define ACECM

Definition was given as suggested.

[R1, #18]
P410 L12: remove "of WGS84", or change to: ". . .3800 m relative to the WGS84 geoid".
P410 L22: period *of* the past . . .
P411 L4: referred *to* as the..
P411 L6: referred *to* as *the* DF2 ..

These were changed as suggested.

[R1, #19]
P411 L16: Do you have a reference here? Logging practices differ somewhat between countries.

Brief description is found in Fujita et al. (2002a).

[R1, #20]
P411 L23 *hiatuses*
P412 L12: * and surface snow redeposition processes such as sastrugi.

These were changed as suggested.

[R1, #21]
P412 L22: remove "with confidence", or describe what this confidence is based on.
This confidence was based on our procedure to observe pattern of peaks. We explained this in the main text.

P412 L29: again, "confidently" is rather subjective unless you quantify it.
We explained that "triple check of the pattern between DF, EDC and Vostok" was done.

[R1, #22]
P413 L9: or *accumulation hiatuses during* cold periods
P413 L22: difference *between the age scales*
P413 L23: remove "respectively"
P413, section 3.2. The fact that DF ages are older at the last interglacial was already observed by Bazin et al. 2013, figure 7. Please mention that.
P414 L1-2:Here, *positive (negative)* . . .. *older (younger)* ages *than* the AICC2012 chronology

These were changed as suggested.

[R1, #23]
P414 L5-6: "there are tails. . .entire MIS 5". I have no idea what this means! Please clarify or remove.

We modified expressions. Please see end of section 3.2.

[R1, #24]
P414 L6-9: "Over the period . . . 4, 3 and 2". Rewrite this sentence or remove.

We removed this statement. Referee #2 also gave us a caution. Please see [R2 #35].

[R1, #25]
P414 L12: "gradient" should be "slope", or "derivative". (gradient is commonly used when there are more than 1 dimension).

It was changed as suggested.

[R1, #26]
P415, L7-12: this is not completely fair. AICC2012 also uses O_2/N_2 age constraints

It is true that AICC2012 also uses O_2/N_2 age constraints. Our point is that the AICC2012 is a "glaciological chronology". We mentioned that AICC2012 also used O_2/N_2 age constraints.

[R1, #27]
P416 L13-L15: we must examine *firn densification processes as well, which greatly complicates the analysis*. Note that AICC2012 does not technically include firn densification modeling.

We agree with suggested expression. It was changed as suggested.

[R1, #28]
P416 L22: Please explain what we should expect to see here if AICC2012 perfectly respected its age constraints.

AICC2012 is a probabilistic dating using several ice cores. It seems difficult to expect some results of age-marker-based dating simply. 2σ of age marker is larger than ~4kyrs for ice older than 93 kyr BP. Thus, it is not proper just to interpolate them.

[R1, #29]
P416 L26-28: Use consistent age uncertainties for the O_2/N_2 age constraints from EDC and DF. The AICC2012 constraints were chosen at 4ka because of questions regarding phasing with insolation. Either you accept this uncertainty in phasing, and set O_2/N_2 uncertainties at DF to 4ka also, or you reject it, and set them all to 2ka.

Because speleothem record in China agree well with the O_2/N_2 age constraints of DF core except one case at 94.2 kyr BP, we can be confident to use 2 kyr. The case of the 94.2 kyr BP will be discussed in detail in the revised paper.

[R1, #30]
P417 L13: do you mean to say that AICC2012 does not fit its own age markers? Please elaborate.

Our expression was "Thus, the 1–3 kyr gaps are apparently not driven by the age incompatibility between the ice age markers used for establishing the two chronologies". It is not fair to say that AICC2012 does not fit its own age markers, because AICC2012 is the best compromise between age constraints and glaciological ice flow model. We hope to keep present expression. Instead, in the revised version,we discuss glaciological models have error bias of surface mass balance.

[R1, #31]
P417 L19-20: AICC2012 does not include constraints from firn densification modeling. Please rephrase. The AICC Delta-depth approach has many uncertainties also.

We suggest to rephrase as follows. "These numerous gas age makers are linked with the ice age of the AICC2012 through assumptions of firn thicknesses at each site and lock-in depths."(P12. L.45)

[R1, #32]
P418 L10: “this possibility” WHAT possibility??

"the possibility of complex effects from the other ice core's orbital markers and from numerous stratigraphic links with the influence of background scenarios". In the revised paper, we specified this.(P13. L.2-3)

[R1, #33]
P419 L4: age *difference varies, with peak differences* at MIS . . .

This part was changed as suggested.

[R1, #34]
P419 L10: consider moving the discussion on speleothem ages to its own section for clarity.

We put this discussion between previous 4.1 and 4.2.

[R1, #35]
P419 L21, L22 and L23: “is deviated” should be changed to “deviates”. Also on P420 L7
P419 L28 MIS 5c, 5d *and 6*. Remove: “reliability of”
P421 L4: remove “on a time series”

These were changed as suggested.

[R1, #36]
P421 L20: Note that during MIS5a-5c AICC compares really well with the speleothem.

We noted this in the revised manuscript. We understand that it was very important point.

[R1, #37]

P422: Appendix A. Refer to the appendix in the text. I only found out when I got to the end of the paper.

We mentioned it in the revised manuscript.

[R1, #38]
P436, caption: please state what the horizontal axis is. Time? Depth?

Horizontal axis is depth. We stated it in the revised manuscript.

RESPONSE TO COMMENTS OF THE REFEREE #2 (C125–C130, 2015)

General comments:

[R2 #1]
The authors use proxy measurements of volcanic fallout deposits in two deep ice cores from Antarctica to establish stratigraphic tie-points between the two ice cores. Based on this synchronization, age differences between the two respective ice-core timescales and potential causes are discussed. Systematic errors in estimating surface mass balance is considered a major source of age uncertainty in ice cores from Antarctica dated using a glaciological approach.

As for the authors' view about importance of the present work, please see M2 above.

[R2 #2]
Volcanic synchronization is a commonly applied tool in ice core sciences and has been previously used to synchronize ice cores Vostok, Dronning Maud Land, Talos Dome and EDC. The majority of the data sets used have been used for similar studies before.

Present work made synchronization between Dome Fuji ice core with one of these ice cores (the deepest and oldest ice core). Dome Fuji deep ice core has never been synchronized to any of these ice cores before. Even if the data of Vostok, Dronning Maud Land, Talos Dome and EDC have been used before for the other synchronization work, it seems to mean little to our new work. Rather, once DF-EDC synchronization is established, Dome Fuji very deep ice core is then indirectly synchronized to the other ice cores, which is very positive situation for paleoclimatic studies.

[R2 #3]
Due to the large number of different timescales available for these ice cores, the most likely sources contributing to dating errors can be isolated by the authors.

We have studied and examined mainly between common time scales of these four cores (AICC2012) and a chronology of Dome Fuji core (DFO2006). EDC3 and DFGT was used as references. "The large number of different time scales" seems overemphasis.

[R2 #4]
These errors are further discussed in a companion paper, and the findings of this study are proposed to be used in envisioned revisions of the AICC2012 timescale. Thus, the manuscript

presented here has a very strong methodological focus on the evaluation of timescales with very limited direct implications for paleoclimatology.

Please see M2 above. We have an objection to the comment "very limited direct implications".

[R2 #5]
In summary, the approach followed in this study is straightforward and the main conclusions made by the authors are well supported by the data.
A major concern I have, however, is that the overall significance of the analysis does appear too limited to justifying a stand-alone publication. If this was written more concise I could easily see these results incorporated into the methods section of the companion paper.

Please see M2 above.

Specific comments:

[R2 #6]
Page 408: L. 1-22: Was your sole motivation to understand the differences between the two timescales? Wouldn't it be expected that they are different?

We have not stated that the authors have sole and narrow motivation like that. Please see M2 above regarding our motivation and meaning of our study.

[R2 #7]
Why does a dating offset of 2-4 ka BP some 120 ka ago matter? Does it limit our understanding of past climate?

Please see M2 again. Both timing and duration are very important to better understand climate of the past.

[R2 #8]
What is the take home message?

Please see abstract, concluding remarks and introduction. Also, please see M2.

[R2 #9]
Should DF2006 be used in the future instead of AICC2012?

In the paper, we did not suggest so. Each of age scale has its own background and approaches.

[R2 #10]
Please clarify your motivation and significance of these kind of evaluation.

As we stated in the manuscript, improving our understanding of ice-core chronologies. Please see introduction. In ice core studies, dating is a central issue that must be studied in order to better constrain the timing, sequence and duration of past climatic events.

[R2 #11]

L. 24-26; Page 409: L. 1-3: Please provide more balance:
The ice cores cited are not the only ones archiving past climate, and the timescales cited are not the only timescales for ice cores in Antarctica. Good age-models are in general important in paleoclimatology, not only for ice cores.

We mention now that good age-models are in general important in paleoclimatology, not only for Antarctic ice cores somewhere at the beginning of introduction.

[R2 #12]
L. 8: How can a timescale for Antarctica have been constrained by annual layer counting in Greenland? I can't see how this should work.

We provided information of a reference paper (Veres et al., 2013). We hope readers to see it.

[R2 #13]
L. 24: Are there conventions in which order ice-core analyses are performed? Please explain.

It seems to us this is not very important in the main context of the paper. Readers can see, for example, Fujita et al.(2002a), an example of ice core processing. In many of ice core projects, electrical conductivity measurements (such as ECM, DEP or ACECM) are performed first.

[R2 #14]
L. 29: What do you mean with profiles? Concentration measurements?

We meant data. We rephrased as "records".

[R2 #15]
Page 410: L. 1: How can you locate an event solely based on electrical properties or sulphate? This is no tephra.

Please see M4, [R1, #2] and M5 above. Electrical properties of ice or sulfate is more robust indicator of large volcanic eruptions. Tephra layers are not very important information for synchronization of East Antarctic ice cores.Rather, there is a danger to use them. Please see new subsection 4.4 on this point.

[R2 #16]
L. 2: Does the eruption take several years? Or the residence time of the fallout products in the atmosphere? This is not the same. I am missing in this section any information how the fallout is incorporated in your proxy? Is it gas, particles; together with snowfall or without? This information becomes important later when you discuss the uncertainties in your tie-points.

No, of course durations of volcanic eruptions depend on each eruption. Fallout of sulfuric acid aerosol is known to occur for one or more years following eruptions due to residence time of it in the atmosphere. We mentioned it in the revised version.

[R2 #17]
L. 5: Why is the number of tie-points in earlier studies so small relative to the numbers you give in the abstract?

It is because earlier studies extracted prominent peak signals. In this study, we extracted even relatively small peaks as many as possible. We hesitate to add a comment on this because it decreases readability. It does not seem very important to readers.

[R2 #18]
L. 21: This is redundant.

This part was revised .

[R2 #19]
L. 28-29: I take it that your main motivation of this study is to provide evidence that the published timescale AICC2012 is imperfect and will need future refinements, but which are not yet proposed in this manuscript? Please replace "the timescale" with AICC2012, unless you want to imply that AICC2012 is the only timescale for Antarctica.

Our main motivation is not like that, please see M2 as for meaning of this study.
For clarity, we change as follows. Two time scales → two age scales (DFO2006 and AICC2012)

[R2 #20]
Page 411: L. 15-16: This sentence can be deleted. I believe measuring the length of ice cores is well established.

The referee #1's view [R1, #19] is that logging practices differ somewhat between countries. In addition, true depth and true length is different. We must tell to readers that the depth is determined by ice core logging.

[R2 #21]
L. 20: Why did you limit yourself to finding a tie-point only every 5 meter? Wouldn't it be better to find as many as possible?

We assume that the referee wrote this comment before reading sentences just 10 lines below L20. It is not realistic that we extract as many as possible tie points from the beginning. As we stated in the manuscript, major tie points were extracted first at least each 5m depth. And then, based on the initial ~650 major tie points, as many plausible minor tie point peaks as possible were extracted. Please see P412 L1-4 of the CPD paper.

[R2 #22]
L. 21: How can a tie-point convince you? With which arguments? If it can't convince you, I believe it does not become a tie-point. Please clarify.

Please see M3 above.

[R2 #23]
L. 22: How can a volcanic signal be lost, and why should that occur frequently? What do you mean with smaller accumulation rate? Smaller than which reference? If you are drilling at sites subject to potential accumulation bias doesn't that also affect your glaciological dating approaches? I can imagine those are assuming some constant

(or at least non-zero) accumulation rates.

Please see M3. Some papers (Barnes et al., 2006; Kameda et al., 2008; Wolff et al., 2005) are useful to better understand the temporally and spatially heterogeneous deposition on the ice sheet.

[R2 #24]
Page 412: L. 1-5: I believe most people would use some computer-aided interfaces for this kind of methods. So the screenshot of your specific interface (Fig. 2) is probably not of immediate general interest. For better readability please consider to show a regular time series plot instead. In addition to a section where you are very confident in the matching consider to also show a section where you are less confident (e.g., in a cold period) to visualize the full range of uncertainties associated with the synchronization.

Showing the PC interface seems necessary either in the main text or in appendix because we suggested extraordinarily a lot of tie point as compared with earlier studies. We provide the Supplementary material A for readers to see our choices of the tie points.

[R2 #24]
L. 5: I don't understand how you can identify the shape, size and synchronicity of tie-points, if the shape and size of the signals are often disturbed, and the synchronicity is achieved by the synchronization itself. Please clarify.

Please see M3. We explained how tie points were identified.

[R2 #25]
L. 6: Do you mean to say that ECM, DEP and sulphate can be used fully interchangeable? Are there no other acidic species in ice other than H2SO4? Or vise-versa, are there no sulphate species present in ice that are not from volcanic eruptions? What about volcanic HCl and marine DMS? All previous synchronizations cited in this manuscript have been done using sulphate. Is ECM and DEP the better parameter to use? Is this the reason why you identify more volcanic tie-points now relative to earlier studies? If so why would that be? I find it surprising that an electric proxy measurement should be equally reliable to detect volcanic events than the direct measurement of the sulphate fallout. I would expect the opposite. For example doesn't the high (alkaline) dust loading during the glacial affect the electrical measurements by neutralizing volcanic acids present in snow?

Please see M4 and M5 above.

[R2 #26]
L. 17: Using the height for what? I am also confused that you don't use the height anymore here, while just above you used the shape and size of the signal for synchronization. Please clarify.

Our statement was not necessarily correct. Please see M3 above for details.

[R2 #27]
L. 20: What are "patterns of data fluctuations"

It is, "Locations of multiple peaks of signals in terms of relative depth".

[R2 #28]
L. 27: Please explain why you find much more tie-points now than previously, which is surprising given that in all cases you used EDC sulphate as the reference for synchronization. Are you less conservative now in your selection?

Our principle to find the tie points were "as many as possible" while the earlier studies often saw some peak height criteria or criteria of acceptable timing. However, we think that we were not necessarily "less conservative" because we were always very careful to see patterns of the data fluctuations. Another reason is that we made the PC interface suitable to find the tie points as many as possible.

[R2 #29]
Is ECM better suited than sulphate? Or are Dome Fuji measurements of higher quality than others?

It is a question to which we do not have a simple answer. Please see M4 as for relations between various signals in terms of volcanic synchronization.

[R2 #30]
Page 413: L. 9: Even with "zero" accumulation the volcanic fallout will still be removed from the atmosphere, and thus end up on the ice-sheets. It does not disappear only because snowfall rates are low. There must be other arguments that you find less tie-points in the cold periods than low accumulation rates.

Please see M3 and [R1, #12]. We suggest that during low accumulation periods the snow surface gets reworked by wind scouring etc, which removes the distinct volcanic layers.

[R2 #31]
L. 14-15: I don't understand this sentence. What is surface mass balance and how is it measured in ice cores?

The surface mass balance is the sum of surface accumulation and surface ablation. Please see Cogley et al. (2001) and/or Cuffey and Paterson (2010) for details of this terminology. We can measure it in ice cores only when layers are dated absolutely or relatively. Our companion paper (Parrenin et al. 2015) studied relative ratio of surface mass balance between two core sites. When two layers within an ice core are dated and when ice thinning between them can be estimated, we can estimate average surface mass balance over a period between the two layers.

[R2 #32]
L. 16: Write relative large numbers (relative to Talos, Vostok, . . .). Overall, 10-20 tie-points every 1,000 years is not many compared to other examples of volcanic synchronization performed for Greenland and Antarctica, or relative to the frequency of major volcanic eruptions from other databases (e.g., http://www.volcano.si.edu/) .

It does not seem very meaningful to be strict to the usage of general qualitative expression "large number of". We do not study ice core close to Iceland volcanoes. We are not comparing our tie

points with synchronization between Greenland ice cores or with database of historical volcanic eruptions. If we start such comparisons, paragraphs for that is necessary, which is not a scope of the present study.

[R2 #33]
L.18: How large must an eruption be to get recorded in the ice? VEI=5, VEI=6? Can you estimate this based on the historic eruptions? What has the atmospheric circulation to do with your ability to detect volcanic fallout? Or the SMB?

These points are beyond the scope of this paper. These points are interesting, but not essential for the discussion of our paper. An important point is that we often find sulfuric acid signals at common timings within ice core in very wide area in Antarctica. Please see M4 and M5, also.

[R2 #33]
Why should the signal diffuse in the ice? These are no measurements of stable isotopes of water.

In case of sulfuric acid, diffusion can occur in liquid phase (e.g., Barnes et al., 2003). We mentioned this in the revised paper. Please see P.5,L.26 and P.7, L16.

[R2 #34]
Page 414: L. 5: What are "tails of the profile"?

Please see [R1, #23].

[R2 #35]
L. 7: Deducing a "periodicity" from two apparent cycles seems risky to me.

We removed this statement. Referee #1 gave use the same comment at [R1, #24].

[R2 #36]
L. 13: What are these "climatic events"? Do they have a name? Why are they important? Please be more specific.

We meant generally climatic stages such as MIS or AIM. We rewrote this sentence as "We also investigated the difference in durations of climatic stages (of various time scales) between DFO2006 and AICC2012". Please see P.8 L.8-9.

[R2 #37]
L. 11-27: I don't really understand this entire section: Of course one would expect if the individual timescales deviate that the duration calculated between the age of timescale agreement and the age of maximum timescale offset will be different as well. Where is the added value in this analysis of duration? Fig. 4 is in my opinion fully sufficient to make the point. Section 3.3 and Table 3 could be easily deleted or at least significantly shortened to make the manuscript more concise.

Importance of correct understanding of durations in paleoclimatology is given in M2. Please see.

[R2 #38]
Page 415: L. 7: What is interpolation of age markers?

We rewrote as "The age scale for the DF core, DFO2006, is interpolation between the O_2/N_2 age markers using glaciological ice flow modeling". Please see P.9 L.1-2.

[R2 #39]
L.12-18: Unclear sentence. Please reword.

970 *Our suggestion is at P.9 L.7-13.*

[R2 #40]
Page 416: This is a very detailed description of a fact that becomes already quite obvious just from Fig. 4: The age offset between the timescales are within the error bounds of AICC2012 but outside the narrower constraints of DF2006. Maybe try to combine this section with Section 3.2.

It would also be interesting if you could summarize why the O_2/N_2 measurements from Dome Fuji are so much more precise than those from Vostok? Is it due to improved methods? Or
980 **due to the sampling sites? If it is common that O_2/N_2 measurements are more precise than TAC measurements why have these measurements not been performed previously on one of the AICC2012 ice cores?**

As for a possibility to combine this discussion (section 4.1) and results (section 3.2), we hope to keep present sections. We hope to separate between direct results of our synchronization and further analysis.
As for uncertainty of the O_2/N_2 age markers, please see [R1, #29].

[R2 #41]
990 **Page 418: L. 1-2: What parameters in the ice form the isochrones visible in the radar? Dust? How do you then link the O_2/N_2 age markers to the radar profiles? Do the radar soundings have the resolution and dating accuracy to detect the "climate events" discussed in the manuscript? How are they matched to the ice cores?**

Please see M6.

[R2 #42]
L. 26: As errors in estimating SMB are proposed as the most likely candidate to explain the dating discrepancies it becomes important to summarize how SMB is measured in ice cores?
1000 **Which measurements are used and which parameters?**

Please see [R2 #31]. Also please see our companion paper (Parrenin et al., in review).

[R2 #43]
Page 419: L. 13: How are speleo-ages determined?

Please see ^{230}Th dating technique in cited paper Cheng et al. (2009). It was stated in the revised manuscript. Please see P.9 L.22.

[R2 #44]
Page 420: L. 3: What makes O_2/N_2 an absolute age marker? Haven't they been orbitally tuned?

Please see Bender (2002) and Kawamura et al. (2007). Variation of O_2/N_2 is orbitally tuned.

[R2 #45]
L.14: Duration of what?

Duration of a period from late stage of MIS 6 until MIS 5b. We mentioned it in the revised manuscript. Please see P.14 L3-4.

[R2 #46]
L. 22: What have water isotopes to do with SMB?

A link between SMB and water isotope ratio is assumed. Please see Parrenin et al. (in review). If assumed link (SMB as a function of water isotope ratio) has errors, SMB used in glaciological modeling will have errors.

[R2 #47]
Page 421: L. 3: A speleothem record. There is probably more than one.

It was changed as "speleothem records".

[R2 #48]
L. 5: Redundant use of "time"

It was changed as "heterogeneously on a time-series".

[R2 #49]
L. 6-8: Are these differences still within the dating uncertainties for AICC2012? If so, will it be necessary at all to update the timescale? If no, are you suggesting to better use DFO2006 as chronology for Antarctica in the future?

This point is now discussed in the revised paper. Causes of the age differences between AICC2012 and DFO2006 is a bit complex. It is very likely that major causes of error at MIS 5b and MIS 5d are in DFO2006 and in AICC2012, respectively. We developed discussions on this point.

[R2 #50]
L. 24: How will these new insights be used? Which approach should be taken in the future? Is there potential to improve the precision of age markers for AICC2012 ice cores? Do you suggest in general to putting more weight on orbital tuning than on the glaciological approach?

Like AICC2012, chronology should be the best compromise between a background chronology (based on modeling of the SMB, and snow densification into ice and ice flow) and observations (absolute ages or certain reference horizons, and stratigraphic links among several cores and

orbital ages). If we can use age markers with smaller uncertainty, we can constrain chronologies accordingly.

[R2 #51]
Page 422: Volcanic events are actually much more frequent than 1 in 154 years. Write volcanic signal frequency in our proxy records instead.

At (P422 L25 in the CPD paper and P19 L6 in the revised manuscript), we rephrased "volcanic events" as " volcanic signal frequency in our proxy records".

[R2 #52]
Page 423: What makes an observation significant? Do you use some objective criteria to define an ice-core signal as "volcanic"? A certain deviation from a threshold? A minimum length of the signal?

Please see M3.

[R2 #53]
Page 424: Are the datasets used in this study already submitted to data repository?

Please see M1.

[R2 #54]
Technical corrections: I strongly encourage the English speaking co-author to double-check and edit language and grammar of this manuscript.

Please see M7.

RESPONSE TO COMMENTS OF THE REFEREE #3 (C136–C145, 2015)

The paper presents a synchronization of the Dome Fuji and Dome C ice cores, and a comparison of their chronologies (DFO2006, AICC2012). Furthermore, the authors compare to other chronologies for these two cores, and to a speleothem record from China. A companion paper describes the conclusions gained about past changes in surface mass balance (SMB) ratio.

General comments:

[R3 #1]
Synchronizing ice cores is an important task, and the synchronization performed here will be of benefit for ice core science. However, I feel that this paper doesn't stand on its own very well, and it fails to reach any substantial conclusions. I therefore suggest the authors to combine this submission with its companion paper (inference about previous changes in SMB based on this synchronization).

Please see M2 above.

[R3 #2]
If the authors decide to keep this paper as a separate manuscript, I recommend them to 1) expand the scope of the paper (see below), as well as 2) better explain the conclusions regarding past changes in SMB as derived in the companion paper. Suggestions for possible ways to extend the scope of the paper:

- Employ the synchronization for purposes other than estimates of past SMB. This could be a detailed comparison of the isotope records from the two cores on a synchronized timescale.

- Include the synchronization to Vostok already constructed as part of this work. This would also allow a comparison to Vostok timescales.

Please see M2 above.

Our scope or aim of this paper is not estimates of past SMB. Of course, SMB is one of very important subjects in polar science. Our purpose in this paper is to better understand chronologies of paleoclimate records. In this paper, we specifically studied two very deep ice cores from East Antarctica. Based on the DF-EDC synchronization in this paper, time scales of the paleoclimatic records were examined in detail, which is a major step toward improving our understanding of their chronology. We hope readers to understand that past history of SMB is closely related to aim of this paper but our focus is better understanding of chronologies.

We now provide preliminary comparison of the isotope records in the revised paper. However, detailed comprehensive discussions of the isotope records should be done in the near future. It requires another set of quite focused and specialized presentations and discussions. Authorship will be different, too. We believe very detailed discussions are beyond the scope of this paper focusing on synchronization.
We presented here the big size task, synchronization of the two very deep cores. In the companion paper, we focus on one of applications, surface mass balance. Detailed comparison of the isotope records and related discussions should be done after that. It seems to us that such a development of sound and "step by step" progress of scientific efforts.

Including here the Vostok records will also introduce complexity seriously. This was discussed by the authors when we discussed publication plan. Vostok site is not located at the dome summit. Origin of Vostok ice core is ~250-km-long flow line from Vostok station to a direction of Ridge B. The chronology is very much influenced by the spatially/temporally variable depositional conditions. In this paper, we deal with two dome summit cores. Vostok should be discussed in separate paper, in which we will discuss ice flow and temporal/spatial variability of depositional conditions along the flow line toward the Vostok ice core site. In addition, we should examine relation between quality of O_2/N_2 record and TAC records generated along such variable depositional environment. This is another topic that should require thoughtful discussions. Moreover, ECM data for Vostok ice core is available only in the time interval 0–145 kyr BP. This limited interval will also introduce "unfocused" condition of the assumed combined paper.

We really hope readers to see importance and significance of this paper. This paper alone has a big impact on our understanding of paleoclimate chronologies, in particular for MIS 5.

[R3 #3]

I also wonder why the authors choose to stop the analysis at 2250m/216 ka, and leave the synchronization of the lower part of the core to a future study. What is the reason to stop here? Does the synchronization get more difficult? If the goal is to use the synchronization for inferring past changes in SMB in relation to glacial/interglacial cycles (as in the companion paper) it would make sense to perform the synchronization over more than just the last few glacial cycles.

There are limitations for volcanic synchronization due to evolution of chemical peak shapes (e.g., Barnes et al., 2003: Fujita et al., 2002b and 2002c). For ice older 216 ka, there are still surviving ECM/DEP peaks to depths down to ~2,400 m. But number of detectable peaks decreases with increasing depths. Moreover, below about ~2,400 m it seems almost impossible to identify peak-to-peak volcanic links between two ice cores. Then, acidity peaks from volcanic origin are not available to synchronize ice core. Then we need some different methodology for ice core synchronization such as dust or water isotopes. Further synchronization is a big and difficult task which should be attempted in future. We mentioned it shortly in the revised paper (P5 L24-27).

[R3 #4]
I would like to see also a discussion (incl. figure) of the obtained annual layer thickness profiles in the manuscript.

We included it in the manuscript as Figure 4e. Indeed, based on this comment, we performed analysis of annual layer thickness, and found that a feature of data to support that one of the O_2/N_2 age constraints has significant error. We now provide discussions about it.

[R3 #5]
I suggest the authors to redo the analysis of relative durations based on the new volcanic synchronization (instead of using the O2N2 markers), and show the results in a figure. This higher resolution analysis may show if there is a general pattern in age duration differences between e.g. warm and cold period, which is not clear from the lower-resolution comparison.

We made such a figure, as in new Figure 4b. We agree with the comment that we discuss duration based on this figure, instead of using the O_2/N_2 markers. Readers can observe the relative durations in higher time resolution.

[R3 #6]
The paper needs editing for language as well as organizational changes to obtain a better flow. It would benefit from being more to-the-point. In the same manner, I suggest to combine figures 4-6 into a single figure, and not display the same data multiple times.

With such editing, I also hope that the conclusions of this paper will become much more apparent. Currently, my main conclusion from reading the paper is that the two timescales are different, but that we don't know why.

As for a problem of language, please see M7 above. We combined previous Figures 4-6 and a new figure of the duration ratio, as new Figures 4 and 5.

It was known in the paper of Bazin et al. (2013) that two chronologies had a major difference, in particular at around MIS 5. Based on our volcanic synchronization, difference in detail in time-

series (or depth) is clarified. Elements in the background (such as age markers, modeling of the SMB, ice flow, and stratigraphic links among several cores an so on) were examined isolating possible causes of the differences. It seems to us that a series of the data analysis and discussions are sufficiently valuable for researchers of paleoclimate community and in Antarctic Glaciology. It is very important to know nature of chronologies, what can be potential errors, how much it is, and why it plausibly occurred. It is also important to know what we should do next to improve chronologies. In the revised version, we attempted to better explain these aspects to readers. Please read M3, too.

Specific comments:

[R3 #7]
Since this paper is about the synchronization of the two cores, I would like the authors to expand on how this was done, by showing synchronized data sections with selected age markers indicated - preferably both for an "easy" and a "difficult" section. This will allow readers to judge for themselves the difficulty of this task, and the corresponding confidence of the resulting synchronization. Such figures should also show which tie points were extracted manually, and which were extracted automatically, so that the performance of the manual vs. automatic routines can be assessed.

First, please see M1, M3 and the appendix.
Manually extracted tie points and automatically extracted tie points were ~650 and ~1400, respectively. Thus, number of the tie point became nearly double. Please understand that we do not make distinction in figures because there seems small benefit to do it.

[R3 #8]
There is always some ambiguity when synchronizing ice cores based on volcanic marker horizons. The sulfate/acidic peaks corresponding to a specific eruption do not necessarily appear similar in the two records (which is noted by the authors).
However, a main – and serious – uncertainty associated with any given synchronization is that whole ice core sections may be incorrectly aligned. Such misalignment over longer sections can be hard to spot, especially when focusing on individual eruptions rather than patterns of eruptions, and such mistakes will not necessarily be picked up from looking at relative depth distances between volcanic horizons in the two cores. I would like to see a discussion of this topic in the paper, along with a more general discussion of how the confidence associated with selected tie points is estimated.

As stated in the paper and previously in this reply, matching was done using the pattern of many peaks in a particular section of core, respecting particularly the relative depth differences between them (which tends to be preserved with an uncertainty similar only to the roughness of the snow surface). We agree that matching individual peaks in isolation can lead to misalignment but this was not done.

[R3 #9]
The authors e.g. mention that they use the Vostok data to do crosschecks. How were these cross-checks performed?

Vostok core was synchronized together with DF cores and EDC cores, as we can see in the fifth row

from the top in Figure A1. Approximately 800 tie points were found between DF and VK, and between EDC and VK, respectively. When patterns for appearance of volcanic peaks in the VK core were 1similar to the DF and EDC core, the identified tie points are very convincing. It is the cross-check. A weak point of VK core is that we had only one set of data, that is, ECM. For DF and EDC, we had four sets of data and three sets of data, respectively. If we have, for example, DEP data and/or sulfate data for VK core, we would find much more tie points.

[R3 #10]
The authors also mention that some of the tie points were ambiguous; in a data file of the depth of the volcanic marker horizons, I hope that they will include this information in the file.

Our statement was that almost all tie points were determined without ambiguity because of the pattern matching (P423 L12-13 in the CPD paper). However, in cold stages, it was difficult to find confident tie points. Matching pattern cannot be found. In such difficult cases, no tie points were chosen.

[R3 #11]
I much hope that the authors are planning to release the employed sulfate, ECM and DEP data with the paper, as it is impossible to evaluate the quality of the synchronization without having access to these data sets.

Please see M1.

[R3 #12]
The authors abstain from including gas stratigraphic markers when comparing marker horizons from the two cores, due to the added complexity when dealing with Delta-age. However, Delta-age calculations have already been published for both cores (Kawamura, 2007, Bazin 2013, Veres 2013). Using these published values for Delta-age, it should not be a major effort to include these age markers in the analysis.

Considering uncertainty of the gas stratigraphic markers such as $\delta^{18}O_{atm}$ is as large as ~6 ka, introducing them in discussion can cause complexity rather than limiting uncertainty. We hope to limit our discussions to the most direct volcanic synchronization link.

[R3 #14]
The authors compare the ice core records with the (absolute) ages determined from a "speleothem record from China". It appears from the referenced literature, that this must be the record from Sanbao cave. Please expand on why this particular cave record was selected for comparison, and how well we know the ages from this cave record. Are other cave records available for this time period that could be included in the analysis?

We base our analysis on a link between EDC3 chronology and the speleothem age (Sanbao Cave) given by Barker et al. (2011). A reason for this comparison was that it is the only one available speleothem records that are linked to chronologies of the deep Antarctic ice core more or less directly. Estimate of the uncertainty also relies on analysis by Barker et al. (2011). They gave the measured age uncertainties from this record over this time period in their Table S1. Absolute speleo error ranges between 0.08 – 2.60 kyr over a period of time to 216 kyr BP. Errors for tuning between

EDC3 scale and the speleo age ranges between 0.11 to 1.57 kyr. Combined uncertainty ranges between 0.13 to 3.01 kyr. Considering readability of the paper, we hesitate to add some of these to the paper.

[R3 #15]

The authors seem to conclude that the O_2/N_2 age markers (albeit not consistent with the TAC age marker at 90 ka, and not consistent with the speleothem age markers during this period either) should be considered the most reliable age markers, and that therefore the DFO2006 timescale is likely to be more accurate than AICC2012. However, over the last 100 ka (MIS 5c), AICC2012 shows indeed very good agreement with the cave record. The authors' conclusion about the MIS 5a-5c periods likely being of too short duration in the AICC2012 timescale, and how this may be caused by errors in estimation of past SMB in AICC2012, seems therefore not very well supported by the timescale comparisons in the paper.

We agree with cautions by the referee #3. We reconsidered all the data. At MIS 5b, AICC2012 agrees very well with the speleothem data. At MIS 5d, DFO2006 agrees well with the speleothem data. Thus, over the last 100 ka, AICC2012 seems most reliable. At MIS 5b, DFO2006 seems most reliable. We described these aspects in the revised paper.

Technical corrections:

[R3 #16]

The authors use the word "age gap" throughout the text and figures. 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". Similarly, the word "dating scale" should be changed to "age scale" or "timescale". 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.

We now use "age difference" and "age scale" as suggested. We added information of AIM to MIS in some figures.

[R3 #17]

P. 408, Line 5-6: Strange sentence. Perhaps change: "Characterized by strong constraining by the O_2/N_2" -> "strongly constrained by O_2/N_2"

We revised this sentence as suggested.

[R3 #18]

P. 408, Line 16-17: "This leads us to hypothesis . . . approaches": Please reword.

We revised this sentence as suggested.

[R3 #19]

P. 408, Line 22: "compatibility .. assessed": Which age markers are referred to here?

We intended to say generally all markers that we used, O_2/N_2, TAC and speleothem markers. If they are not compatible with each other, it means that errors are somewhere there.

[R3 #20]
P. 409, Line 7: I think it is worth adding a line noting here that the ice cores in the AICC2012 timescale are themselves linked via volcanic and other marker horizons.

It was already noted at Line 11.

[R3 #21]
P. 410, line 8: Please add that Bazin et al also used gas stratigraphic markers.

We added this just after that.

[R3 #22]
P. 410, line 25: Add a sentence or two about the difference between ice age and gas age, so that a non-expert can understand the distinction between these.

We inserted a statement."Note that, gas is trapped in polar ice sheets at ~50–120 m below the surface and gas age is therefore younger than age of the surrounding ice (ice age)."

[R3 #23]
P. 411, line 24: I assume that 2250m depth corresponds to an age of 216 ka? In both cores? In general, the relationship between age and depth is not obvious from the paper. It would be very helpful for the readers if the authors included references to ages as well as depths throughout the paper.

We corrected the depth as ~2170-2180 m. This depth approximately corresponds to an age of 216 ka in both cores. Please see Figure 3 for the relationship between age and depth. In addition, in revised paper, we provided a list of tie points with information of age scales of AICC2012 and DFO2006.

[R3 #24]
P. 412, line 2: It is not clear from text and illustration in figure 2 how the interface for synchronization works.

Please see [R1 #8].

[R3 #25]
P. 412, line 6: "We note that there are no uncertainties associated with the use of the different proxy records . . . for the identification of the volcanic events". It is unclear what the authors mean by this sentence. There is always some uncertainty associated with picking common marker horizons for two cores (as also noted by the authors later in the paper), and since the proxy records used for synchronization also register signals other than volcanic activity, there is uncertainty associated with assigning many peaks in these records to volcanic events.

This part was replaced by explanations of possible uncertainties.

[R3 #26]
P. 412, line 17-18: "We did not use the height of peaks. . . ice sheet". I assume that the authors here are referring to the synchronization work. Please make this explicit in text.

This part was replaced by more detailed explanations.

[R3 #27]
P. 412, line 21: "one or more" -> "two or more" *Corrected.*

[R3 #28]
P. 412, line 24-27: The Vostok and EDC ice cores have previously been synchronized (102 tie points) for the interval 0-145 ka (Parrenin, 2012). I assume that these volcanic tie points provide the basis for the more-detailed simultaneous synchronization to Vostok made here? If so, this should be made clear from text.

Present work was done independently without referring the previous work done by Parrenin (2012). Later, we checked that the independent outputs agreed with each other. In the present paper, we do not hope to give very detail of the synchronization work with Vostok ice core. Please see [R3 #2] about our reason for it.

[R3 #29]
P. 412, line 28: How were these crosschecks made?
Please see [R3 #9].

[R3 #30]
P. 413, line 1: "Supplement" -> "Appendix" *Corrected.*

[R3 #31]
P. 413, line 8-10 + 18-20: It also seems that it became harder to find tie points in the deeper part of the cores. Please comment on why this may be (thinner layers? increasing disturbances in layering with depth?).

We provided explanations for it in section 2.2 and at the end of section 3.1.

[R3 #32]
P 413, line 12: "Characterized by" -> "developed based on" *OK.*

[R3 #33]
P. 413, line 14-15: Include a plot of gradient on the figure. This gradient not only shows "variable SMB multiplied by thinning", it also simply shows the mean layer thicknesses within any given interval. This is also valuable information that deserves a mention.

Please see [R3 #4].

[R3 #34]
P. 414, line 5: What is the meaning of the sentence: "there are tails . . . entire MIS5"?

We rephrased. Please see [R1, #23].

[R3 #35]
P. 414, line 7, 8, 9: "difference for x kyr" -> "difference over a period of x kyr" (repeated 3

times). *These sentences were removed from the revised version.*

[R3 #36]
P. 414, line 17-19: "we use these... EDC cores". I find it strange that the authors here decide to use the O_2/N_2 markers, instead of the new volcanic markers that form the basis of the work in this paper. Both are ice markers, but due to its event-like nature, the volcanic synchronization has much smaller uncertainty in the depth assignment. I suggest the authors to redo this analysis based on the volcanic markers, and show the result in a figure.

We agree with the comment. Please see [R3 #5].

[R3 #37]
P. 414, line 22, 26: Please refer to age instead of ID values. *OK.*

[R3 #38]
P. 414, line 25, 28: Where does the uncertainty value of 2.7 ka come from? The uncertainty of the O_2/N_2 markers is stated to be between 2-4 ka.

It is propagation of errors in both addition and subtraction. We calculated time span between two age markers, that is subtraction.
$2\sigma_{1\text{-}2}=((2\sigma_1)^2+(2\sigma_2)^2)^{0.5}$
Here, $2\sigma_1$ *and* $2\sigma_2$ *are from Table 2. Then* $2\sigma_{1\text{-}2}=2.7$*. Now we removed the related statements in the revised text.*

[R3 #39]
P. 415, line 1-5: This paragraph is very hard to read.
This was removed.

[R3 #40]
P. 415, line 6 (start): The discussion section needs to be re-arranged to make it easier to read, and to avoid repetitions. I suggest the following: First: An introductory section, describing what may potentially cause the age differences. Then make individual section in which each of these topics are described, e.g. non-compatible tie points, thinning function, influence by links from other cores, SMB, etc.

Many thanks for this suggestion. We rearranged the section 4 based on this suggestion.

[R3 #41]
P. 416, line 11: What are these "some other ice markers", apart from 10Be markers? If referring only to 10Be markers, then write this instead.

There are various markers such as ^{10}*Be, ACR-Holocene transition and Mt. Berlin tephra. Thus we expressed as some other age markers. We wrote them in the revised manuscript.*

[R3 #42]
P. 416, line 7 -417, line 20: This section is very hard to follow. Please rewrite.
P. 417, line 14-20: This is almost a repetition of section from P. 415, line 26 – P. 416, line 5. Please combine these two sections.

We revised Section 4 largely.

[R3 #43]
P. 417, line 23- P. 418, line 2: The disregard of any errors in the thinning functions warrants a little more attention. To completely "exclude the possibility of errors in the thinning function" seems like a major disregard, especially since the paper end up concluding that changes in surface mass balance must be driving errors in the glaciologically-derived timescale – which by itself will influence the thinning function. However, the important question is not whether the thinning function is erroneous, but rather how much effect different thinning function effects the resulting timescale. I'd like to see some sensitivity studies on the resulting timescale when using slightly different thinning functions, although this topic might fit better into the companion paper. At the very least, this section should here be reworded to reflect these uncertainties.

Our views are as follows. There should be some errors. However, such errors are not main cause of the age disagreement. We find no reason that such errors can occur especially at MIS 5d. We described such a comment in the text. Please see Section 4.2.1. Also, more discussion was provided in the companion paper (Parrenin et al., in revision).

[R3 #44]
P. 418, line 1-2: I have a hard time believing that simply by looking at the shape of internal isochrones over these large distances, one would be able to infer whether or not the two employed thinning functions are correct.

Please see M6. Also, more discussion was given in the companion paper (Parrenin et al., in review).

[R3 #45]
P. 419, line 12: Introduce the speleothem record that is referenced (Sanbao cave?). What are the measured age uncertainties from this record over this time period? What is the reason to pick this cave record? Are other cave records available that could be included?

Please see [R3 #14].

[R3 #46]
P. 419, line 15: New results (Buizert, in press) show that there is a lag between abrupt changes in Greenland temperature, and the inflection points in the Antarctic temperature record. However, the lag is of the order of 200 years, and so does not explain the large age differences observed here.

Thanks for the information. If the lag is of the order of 200 years, then, deviation from the speleothem age is probably a real error. It seems that we must carefully see reliability of age markers (O_2/N_2, TAC and speleothem markers), as we gave a statement in the concluding remarks. Indeed, we now believe that the O_2/N_2, marker at 94.2 kyr BP has an error of ~3 kyr toward older direction. In the revised manuscript, it is discussed.

[R3 #47]

P. 420, line 3-5: I think it is important also to note the following:
1) In the interval 0-100 ka, the AICC2012 and speleothem ages agree very well. Thus, from this comparison, it seems likely that the O_2/N_2 age markers are in error in the section around 90 ka.
2) All other O_2/N_2 markers (except for the section around 200 ka, where there is reason to believe the speleothem ages to be off) are within 2 ka of the speleothem ages, consequently there is reason to believe that the remaining of these tie points are correct within their associated uncertainties.

We agree with these two points.This was addressed to the revised version.

[R3 #48][R3
P. 420, line 9: I assume the authors here are talking about the AICC2012 age scale.

We repaird this part to clarify the subject.

[R3 #48]
P. 420, line 16: Yet, in the section MIS 5a-5c, the AICC2012 timescale is in very good agreement with the speleothem ages, which seems to suggest that there is not any "strain compensation" in this section.

We now agree with the comment. Indeed, we now believe that the O_2/N_2, marker at 94.2 kyr BP has an error of ~3 kyr toward the older direction. In the revised manuscript, it is discussed.

[R3 #49]
P. 420, line 15: Here the authors mention that errors in the thinning function caused by previous SMB changes relative to the model are the cause of some of these age scale differences. However, the authors previously rejected errors in the thinning function as reason for timescale differences.

This sentence was removed. Please see [R1, #24] and [R2 #35].

[R3 #50]
P. 420, line 27: "because methods for establishing a chronology are consistent". I don't understand?

This sentence was removed. Same as [R3 #50].

[R3 #51]
P. 421, line 13: "driven . . . mainly" -> "not driven" *Corrected.*

[R3 #52]
P. 421, line 18: The authors ought to specify that this is not the case for the section 5a-5c. *Corrected.*

[R3 #53]
P. 422, line 10: Most of the TAC age markers and O_2/N_2 age markers are compatible. Only the

marker with ID 6 is incompatible.

We agree with it, in terms of uncertainty range. Behind our statement, we had in mind that dominant spectrum component is different between TAC and O_2/N_2. In addition, uncertainty is also different. The published records of O_2/N_2 show that their spectral signature is dominated by the precession in case of O_2/N_2 (Kawamura, 2001; Bender, 2002; Kawamura et al., 2007; Suwa and Bender, 2008a). However, in case of TAC, it is dominated by the precession but there are additional effects from the obliquity (Kawamura, 2001; Raynaud et al., 2007). This difference is not yet explained.

We do not mention aspect above in the revised manuscript not to disturb readability.

[R3 #54]
P. 422, line 27: Just because the tie points are found 150 years away from each other on average does not mean that there is only a volcanic eruption happening every 150 years. This period is only between eruptions that are sufficiently prominent and distinct to be identified in both cores.

Please see [R2 #51].

[R3 #55]
P. 423, line 3-10: This description neglects the possibility that volcanic signal 1 may only be found in ice core 1, and volcanic signal 2 only be found in ice core 2. This situation may happen more frequently than one would want. It should be noted that the uncertainties related to selecting adjacent tie points are highly correlated: If one tie point happens to be chosen wrongly there is a high chance that the next tie point will also be wrong, since the depth scales would be shifted relative to each other. The only way to avoid this is to look at patterns of peaks, not just the relative depth of volcanic peaks, which seems to be how the automated interface deals with the data. Please discuss this issue.

Please see M3. Looking at patterns of peaks were very important. We now emphasized this point in the revised manuscript.

[R3 #56]
P. 424, line 2: How can the value of 0.1 be derived from figure A3?
Please see [R1, #10].

[R3 #57]
Figure 2: Why do some of the panels have black background? The right-hand side of the figure (with all the buttons) is not important for understanding the data and/or process of picking tie points, and can be removed. Is the blue dot a preliminary handpicked tie point? Which would then be the automatically picked tie points? This figure focuses on showing the automated interface for picking tie points. However, I think it is much more important to show some data section with the final layer picks, so that the reader can judge the confidence level in these picks.

We agree with the criticism. Demonstration of the interface has secondary importance.
To improve the situations on this aspect, we provided figures showing the tie points in the

Supplementary material A. Also, we moved the information of the PC interface to the Appendix A. Please see M1 and M3 for closely related discussions.

[R3 #58]
Figure 3: I suggest to add a plot of the gradient of depth/age profiles for the two cores. This gives information on the changes in layer thickness down the core.

We added suggested profiles in Figure 4e. The figure let us recognize that there was a step at F4, which is presumably caused by an error in one of the O_2/N_2 constraints. The figure played important role in the discussion.

[R3 #59]
Figure 4-6: I suggest to merge figures 4-6 into a single figure. In all figures, the isotope record on the two timescales is repeated (which also in shown in figure 3, although here only on the DFO2006 timescale), so the only new information is in the upper panels in figure 4a, 4b, 5, and 6. Furthermore, the information in these figures will be easier to compare if in a single figure.

We modified the figures as suggested.

[R3 #60]
Table 2-4: It is confusing that age markers from both DF and EDC are simply labeled 1,2,3, so that one ID corresponds to two very different ages (ID 1 has ages 12.3 ka and 7.3 ka in the two cores, respectively). I suggest giving the ID values a core-specific label.
Table 3: Add to table the ages corresponding to start/end of each section.

We modified the tables as suggested.

[R3 #61]
Table 4: ID 4 and 5 have the same age (but different ID values). Yet, the synchronized depth on DF1 is not exactly the same, and consequently neither is the corresponding DFO2006 age. How can this be if the three cores are synchronized simultaneously?

We have investigated this point. As for the DF-EDC-VK volcanic synchronization inspected in this work, we confirmed that the results were the same as previous EDC-VK volcanic synchronization done by Parrenin et al. (2012). Therefore, it is unlikely that the inconsistency (as large as ~10 m in DF depth) is caused by errors in volcanic synchronization. We also investigated if authors of the ^{10}Be records used Vostok 5G core or some other core. If different core is used, it can be a cause of the errors. In Raisbeck et al. (2007) or Bazin et al.(2012), we could not confirm it. We speculate that the cause of the inconsistency is in determined depth of the Laschamp geomagnetic excursion in two different ice cores of EDC and VK. Indeed, the Laschamp geomagnetic excursion has a broad peak with width of ~100 m in deep ice cores. It seems natural that depth determination has some uncertainty.

RESPONSE TO COMMENTS OF THE REFEREE#4 (C157–C162, 2015)

[R4 #1]

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.

As for relations between two papers, please see M2 above.

[R4 #2]
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.

As for robustness of the tie points, please see M3 above. In addition, we provided many examples of the tie points in the Supplementary material A.

[R4 #3]
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.

Our views are as follows. Because availability of data sets depended on depth range, number of tie points for each time span does not indicate occurrence frequency of large volcanic eruptions on the earth. From the ice sheet surface to a depth close to 1000 m, no data set from DF2 core was available for the synchronization. We believe that this situation limited the number of identification of the tie points. In addition, considering temporal variation of the depositional environment, we cannot simply deduce past occurrence frequency of the volcanic eruptions. We now provided such information at P7 L19-24.

[R4 #4]
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".

For more detailed description, please see M3 above.

[R4 #5]
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.

As for ^{10}Be Laschamp event, please see [R3 #61]. It is not very useful. As for volcanic tephra, please see [R1, #2] and new subsection 4.4.

[R4 #6]
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.

Please see M5 above.

[R4 #7]
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.

Please see the examples in the Supplementary material A. When the patterns of data fluctuations agreed between one or more sets of data at DF and EDC, they were extracted as tie points. Probability for accidental occurrence of the signal peaks at the same timing in the ice cores are extremely small. In our present case of the volcanic synchronization, such sets of the synchronous peaks constitute sequential chains, which further ensures robustness of the tie points. Pattern recognition was very important. Thus, depth window to investigate the tie points were often 10-20 m in which multiple peaks of signals were found.

[R4 #8]
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.

Such a discussion would require discussions on original locations of volcanoes on the earth, atmospheric circulation and depositional environment at multiple ice core sites, all together. It is not scope of our discussions in this paper. We would like to point out, as we use larger number sets of core data, number of tie point candidates increases.

[R4 #9]
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.

Pattern of the peak distributions were very important to confidently determine the candidates of the tie points. Please see examples of tie points in the supplementary material.

[R4 #10]
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.

As for release of data, please see M1 above.

[R4 #11]
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.

Readability will be improved by a planned action for [R1, #13], for example.
The referee shed a light on a need for a review of ice core dating. However, we feel that such a kind of review section in this paper will cut flow of descriptions. Instead of making a new section, we added more words and explanations both in introduction and discussions.

[R4 #12]
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.

In the revised manuscript, we discussed possible causes of errors of DFO2006 age at MIS 5b and errors of AICC2012 age at MIS 5d. The revised version provide age uncertainty of these timescales.

[R4 #13]
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.

This was replaced by "age differences".

The other comments from the referee #4 is for a paper Parrenin, Fujita et al. (2015).

RESPONSE TO COMMENTS OF THE REFEREE #5 (C223–C224, 2015)

The work presents a synchronization of two deep ice cores, the Dome Fuji (DF) and EPICA Dome C (EDC) cores. The results are rather technical and of interest only to a rather limited readership, but on the other hand, having good timescales for the two ice cores is an objective of considerable importance and of general interest that cannot be met without publishing the nitty-gritty details going into a time scale. Ways to make the manuscript more significant, and thus more strongly justify publication as a separate manuscript rather than as a technical section of another paper of, could be

[R5 #1]
- to make sure that both the data used and the synchronization tool is made accessible upon publication. Without the data, the reader cannot check the validity of the synchronization, and if the tool is not made available, it makes little sense to introduce it.

As for the publication of data, please see M1 above. As for the tool for the synchronization, we provide the code and explanation as Supplementary material B. Please see also [R1 #8].

[R5 #2]
- to extend the synchronization to the entire length of the cores

Please see [R3 #3].

[R5 #3]
- to analyze the synchronized records

Please see [R3 #2]. Comparison of the synchronized ice core records (isotope records) is now provided in the revised paper.

I will not go into details as the manuscript has already been reviewed thoroughly by several reviewers, but here some issues are mentioned:

[R5 #4]
- The manuscript would benefit greatly from thorough language revisions by a native speaker.
Please see M7 above.

[R5 #5]
- "Age gap" is misleading. "Age difference" or "age offset" seems more appropriate
Corrected.

[R5 #6]
- If you want to use the marine-based MIS nomenclature to refer to ice core time intervals, which is rather illogical but also convenient, please define which ice-core interval you assign to each MIS, either in a table or by marking the boundaries between the different MIS on fig. 3.

It does not seem very necessary to define exact boundaries between stages in this study. We hope to assign each stage to peaks and troughs of water isotope simply. We add information of AIM.

[R5 #7]
- The authors seem biased towards mostly referencing their own work. While this is perfectly justified for the more specialized studies, the introduction would benefit from a broader selection of references.

For data that authors measured and for coring sites that authors investigated, it seems natural to cite such papers. It was not authors' intent to apply some bias of citation. When we revised the paper, we checked this point.

[R5 #8]
- Section 2.2 is rather long but contain almost no quantitative information. It should either be shortened or (better) extended with quantitative details so that the workings of the synchronization tool is explained.

Based on comments from several referees, we providea a set of figures showing many tie points inside them (Please see Figure 2). At the same time, we sent the figure of the PC interface to Appendix A. We revised the text of the section 2.2 with such replacement of figures.

REFERENCES:

Azuma, N., Y. Wang, Y. Yoshida, H. Narita, T. Hondoh, H. Shoji and O. Watanabe 2000. Crystallographic analysis of the Dome Fuji ice core. Physics of ice core records, Sapporo, Hokkaido University Press, 45-61.

Barnes, P.R.F., E.W. Wolff, H.M. Mader, R. Udisti, E. Castellano and R. Röthlisberger 2003. Evolution of chemical peak shapes in the Dome C, Antarctica, ice core. Journal of Geophysical Research-Atmospheres, 108(D3).

Cogley, J.G., R. Hock, L.A. Rasmussen, A.A. Arendt, A. Bauder, R.J. Braithwaite, P. Jansson, G. Kaser, M. Möller, L. Nicholson and M. Zemp, 2011, Glossary of Glacier Mass Balance and Related Terms, IHP-VII Technical Documents in Hydrology No. 86, IACS Contribution No. 2, UNESCO-IHP, Paris.

Cole-Dai, J.H., E. Mosley-Thompson, S.P. Wight and L.G. Thompson 2000. A 4100-year record of explosive volcanism from an East Antarctica ice core. Journal of Geophysical Research-Atmospheres, 105(D19): 24431-24441.

Cuffey, K.M. and W.S.B. Paterson 2010. The Physics of Glaciers. Fourth ed., Academic Press; 4 edition (May 17, 2010).

Fujita, S., P. Holmlund, K. Matsuoka, H. Enomoto, K. Fukui, F. Nakazawa, S. Sugiyama and S. Surdyk 2012. Radar diagnosis of the subglacial conditions in Dronning Maud Land, East Antarctica. The Cryosphere, 6(5): 1203-1219.

Fujita, S., M. Hirabayashi, K. Goto-Azuma, R. Dallmayr, K. Satow, J. Zheng and D. Dahl-Jensen 2014. Densification of layered firn of the ice sheet at NEEM, Greenland. J. Glaciol., 60(223): 905-921.

Hammer, C.U. 1980. Acidity of polar ice cores in relation to absolute dating, past volcanism, and radio echoes. J. Glaciol., 25(93): 359-372.

Hammer, C.U., H.B. Clausen and C.C.J. Lnagway 1997. 50,000 Years of Recorded Global Volcanism. Climatic Change, 35: 1-15.

Koerner, R.M. 1971. A stratigraphic method of determining the snow accumulation rate at Plateau Station, Antarctica, and application to South Pole-Queen Maud Land Traverse 2, 1965-1966. In Crary, A.P., *ed. Antarctic ice studies II*, Washington D.C., American Geophysical Union, 225-238.

Narcisi, B., J.R. Petit, B. Delmonte, I. Basile-Doelsch and V. Maggi 2005. Characteristics and sources of tephra layers in the EPICA-Dome C ice record (East Antarctica): Implications for past atmospheric circulation and ice core stratigraphic correlations. *Earth and Planetary Science Letters*, 239(3-4): 253-265.

Raisbeck, G.M., F. Yiou, J. Jouzel and T.F. Stocker 2007. Direct north-south synchronization of abrupt climate change record in ice cores using Beryllium 10. Clim. Past, 3(3): 541-547.