

Again, we have appreciated all the constructive comments on our manuscript. In the following we reply to the main points that the editor has pointed out. Then, a point-to-point response of the detailed point is given.

Best regards,

Shuji Fujita and Frédéric Parrenin on behalf of co-authors

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## **RESPONSE TO MAJOR COMMENTS**

[#1]

**I have two sets of comments. The first is primarily related to language or word choice, or some details of the presentation that I thought would not be clear to the reader. These comments are noted in the attached marked up pdf version of the paper. I do not anticipate any major challenges in addressing these comments. An additional editorial review by a native English speaker would help with some details. There are a few small grammatical errors in the Supplement A text as well, that would be easy to fix.**

*As for the first set of comments, we repaired language and word choice as suggested. Also, some details of the presentation was added, responding to your comments.*

*Because of the limited time before the suggested deadline of upload, we hope to rely on edition of English done in the editorial process of the CP.*

*A few small grammatical errors in the Supplement A text were repaired.*

[#2]

**The second set of comments concerns responses to a few reviewer concerns. For the most the changes in response to reviewers comments have significantly improved the manuscript and again, thanks for considering them so carefully.**

**There are three issues outstanding for me. One is the question of data availability. Several reviewers asked specifically that all of the data be made publically available. Many of the data sets are, but some will only be available on request. I have no reason to doubt that the data will be shared on request, but this does not necessarily guarantee that it could be used by others freely. The impact of the work would be greater, now and in the future, if we could be sure of the public archive now. So I urge the authors to reconsider this aspect of the response.**

*As for the ECM data and AC-ECM data of the DF2 core, I (S. Fujita) promise that I will submit the data used in the present DF-EDC synchronization work to National Centers for Environmental Information (NCEI) as soon as I can complete preparations for it. Hopefully it will be in September 2015. It will never be later than October 2015.*

*As for the sulphate data of the EDC96/99 cores, the lead authors asked to the data holders (some Italian EDC scientists) to reconsider this aspect of the response. They did not agree with submitting the data to a public database. They claim that they have sent the data to everybody who had requested the data to them and that they will continue to do so. Within the international collaboration of this paper 07, the lead authors could not change the unreleased status of the data. Frankly, it is a shame and a pity, in particular, when repeated provide of the unreleased data is related to joining to coauthorship to many papers. Nevertheless, please note again that readers can see **all** of the sulphate data of the EDC96/99 cores in the Supplementary Material A. The graph data are not as flexible as or as useful as the digital text data on public database. However, the data are in public at least to this extent. We hope that the editor understand that the lead authors could not do more.*

[#3]

**A second, small point concerns the response to R4#7. This reviewer was asking about why the match tolerance was a fixed distance rather than a varying distance due to thinning. I did not find a clear response to that one particular point and would appreciate some additional explanation.**

*I (S. Fujita) did not really understand what the referee meant in his/her review before. Now I understand what was meant. [R4 #7] is first copied here, and then our reply is described below.*

[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.**

*First point is about suspicion about "lots of incorrect tie points". Again, we emphasize that our synchronization work was based on pattern matching. Indeed, please see all figures of showing the tie points. From the point of view of the operator who performed the extraction work, I would contend that all the extracted tie points are correct. In our manuscript, very small possibility for the occurrence of small (centimeter scale and random) errors was explained. Once readers inspect the figures by themselves, they will soon understand it is impossible that “lots of incorrect tie points” can happen with the data used.*

*Second and the main point. We wrote in the paper “When we searched for possible candidates of the tie points, we found each pair of candidates in most cases within ~0.1 m of expected depths“. We used the number “~0.1m” as approximate number of our observation. This number was not something like a strict criteria or tolerance. We can see in Figure B3 that the data scatter within the graphs looks similar between figures. To see many figures closely, we think that there are some more reasons that control (or affect) the data scatter. For example, a possibility which we cannot examine easily is depth errors of the ice cores related to various factors; (i) borehole inclinations, (ii) cumulative small errors of ice core logging, (iii) fractures, post-coring relaxations of the core in addition to (iv) surface snow redeposition processes such as sastrugi. In our study, it seems impossible to resolve these because for example, it is very hard to quantify how DF/EDC teams handled carefully/strictly ice core depths in the counterpart sides. Moreover, number of the tie points in unit period of time is highly variable, which may also affect the data scatter. A fact is, when we compare 4 sets of data within DF and 3 sets of data within EDC, we can see scatter of volcanic spike peaks of the order of centimeter. This fact seems to be related to heterogeneous distribution of impurity within ice cores. Further possibility is that the data scatter can increase with increasing depth (See, data scatter is smallest at Holocene). In this case, there may be some physical reasons of heterogeneous deformation that influence the layering in scales of the order of centimeters. If the depth scatter is solely by depositional irregularity, it may decrease with increasing depth. But reality is different as the referee pointed out. But it cannot be a reason to claim presence of lots of incorrect tie points.*

*We just show our observation that the data scatter was of the order of 0.1m. From a view point of the operator, it is never be caused by wrong tie points. When I did the synchronization work, the choice of the volcanic acid peak was almost always unique; there were almost no rooms for errors. We provided sufficient (all) figures for the readers to verify this point.*

[#4]

**A third point is the inclusion of the DF vs DC phase lag between the deuterium records. The result is interesting, but not well enough described (see my comments in the text) to be evaluated. This may have been included at the request of a**

**reviewer and at face value seems like a nice idea. However, my opinion is that it either needs to be expanded or not included, and I am comfortable with either choice.**

*We choose to expand. We add new figures 7 and 8 to provide more information. Please see [#25] below.*

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— detailed comments —

**[#5] Page 2, Lines 8-10**

**A reader may be confused about this sentence because it is not clear what these deuterium signals lead (what variable are they being compared to?). Also can I suggest using a more formal isotopic term like D/H ratio of ice or dD of ice?**

*We revised this part as follows. “Additionally, we found that the deuterium signals of ice,  $\delta D_{ice}$  (‰, VSMOW), at DF tends to lead the one at EDC, with the DF lead being more pronounced during cold periods. The lead of DF is by +710 years (maximum) at MIS 5d, -230 years (minimum) at MIS 7a and +60-+126 years on average.”*

*We used an expression  $\delta D_{ice}$  (‰, VSMOW) in the manuscript.*

**[#6] Page 2, Lines 18-20**

**I suggest revision here. "In addition, good ice core age models are generally important, because ice core chronologies are often used in other types of paleoclimatic studies"**

*This part was revised as suggested.*

**[#7] Page 3, Line 7**

**Suggest reword: "In ice core studies electrical conductivity studies are usually ....."**

*This part was revised as suggested.*

**[#8] Page 4, Lines 23-26**

**Will need to update this sentence to reflect new plans for the Parrenin paper.**

*This part was removed to reflect the new plans.*

**[#9] Page 5, Lines 19-21**

**This part of this sentence may be confusing. The use of the word "often" suggests to me that sometimes the signals were not common, but then one would ask why they were used as tie points. If I understand correctly you first found 650 obvious matches, and then could use them to guide the pattern matching in more detail. In any case, I suggest that this could be reworded to be clearer.**

**Page 5, Line 21**

**Does this refer to the 650 tie points mentioned above or additional tie points found after those first 650 were identified? This may not be clear to the reader.**

*We remove "often". This part was revised as follows. "At an initial stage, ~650 tie points were extracted down to a depth of ~2180 m for both cores, using prominent peaks common between ice core signals from different ice cores. The ~650 tie points were found as patterns of appearance in ice core signals versus depth and they provided initial hints to recognize further matching patterns of tie points."*

**[#10] Page 5, Line 32**

**To clarify, for the reader - this includes the original 650, correct?**

*This part was revised as follows to clarify the point. “Using the PC interface, 1401 tie points, including the original ~650 points, were extracted. “*

**[#11] Page 5, Line 32**

**To clarify, for the reader - this includes the original 650, correct?**

*This part was revised to clarify the point.*

**[#12] Page 6, Lines 18-19**

**Again, to clarify, 800 points common between both cores, or a total of 800 (some between DF and Vostok and some between DF and Vostok).**

*This part was revised as follows to clarify the point.*

*“Between DF and Vostok, and between EDC and Vostok, for each pair of ice cores, we identified more than 800 tie points covering the past 140 kyr. “*

**[#13] Page 6, Lines 30-31**

**The reader may not know why you do this - is it just to show that the two signals do go together? I suggest adding a sentence to explain why you mention this.**

*We revised the last 5 lines of the Section 2 as follows.*

*“We also note that tephra matches were not used in the synchronization work, because tephra layers that identified from the same origins (eruptions) were rare among deep ice cores from East Antarctica (see Narcisi et al., 2005). In this paper, instead of using tephra matches in the synchronization work, we used it as an a posterior test of the synchronization work. “*

**[#14] Page 7, Lines 30-31 (Last sentence of the Section 3.1)**

**The reader may be curious to know why this is a "speculation" - is this because the automated algorithms are more likely to find a matching pattern if they have more cores to look at? This is a small point but you could elaborate on this slightly.**

*We revised this part as follows. "We deduce that this situation limited the number of identified tie points; we generally find more tie points when we have more sets of ice core data to look at. "*

**[#15] Page 8, Line 4**

**It is not clear what "at timings of" refers to. Do you mean the onset of these two periods?**

*It does not mean the onset. We simplify the expression as "at".*

**[#16] Page 8, Line 10**

**It is not so clear to me what is meant by an "event" or "climate stage" here. As I understand it 4b shows the ratios of durations between tie points. To me those intervals between tie points are not "events" or "stages" in the normal meaning of the word.**

**If my interpretation is correct then I would suggest some rewording of this first paragraph.**

*We removed the words event or climatic stages. We simply express as "durations". This was applied to the section title of 3.3.*

**[#17] Page 8, Lines 20-21**



**This sentence is not really needed but if you feel you want to highlight this observation it is fine.**

*We hope to keep this sentence because at MIS 5 and 6 dating error seems large and complex.*

**[#18] Page 8, Lines 22-26**

**I have some trouble following this sentence. Would this be correct:**

**"In addition, the duration ratio between intervals defined by the O<sub>2</sub>/N<sub>2</sub> age markers (Table 3), which occur on precessional (9-14 kyr) time scales, was examined." Then, I think you need to discuss the result in terms of duration ratio (because that is the term in the first sentence) or change the first sentence so it refers to duration difference.**

*We revised this part as follows. "In addition, the duration ratio between intervals defined by the O<sub>2</sub>/N<sub>2</sub> age markers (Table 3), which occur on precessional (9-14 kyr) time scales, was examined. In intervals of the precessional cycles of the O<sub>2</sub>/N<sub>2</sub> age markers, the difference in durations ranges approximately within  $\pm 3$  kyr. As a result, the duration ratio ranges between 0.75 and 1.25."*

**[#19] Page 10, Lines 16-17**

**I do not feel that this sentence is strictly needed.**

*We removed this sentence.*

**[#20] Page 10, Lines 21-23**

**This sentence might be clearer if it did not refer to modelling, and instead just referred to the need for anomalous flow if this result is real.**

*As suggested, we simply express this part. “Such a step needs anomalous flow if this result is real. “*

**[#21] Page 11, Line 14**

**Is there a difference between an ice age of a marker and an ice age of a chronology? This may not be clear.**

*To clarify this point, we add at page 11, line 12-14 in the revised MS as follows.*

*“Note that there is a difference between an ice age of a marker and an ice age of a chronology in cases of glaciological chronology such as AICC2012. “*

**[#22] Page 12, Lines 23-25 (Last three lines of the section 4.2.1)**

**Need to revise this sentence.**

*We removed this part to avoid citation of the paper 08. To compensate for this removal, we added two new papers to citation, We can see the isochronal layers at Dome Fuji (Steinhage et al., 2013) and those at Dome C (Cavitte et al., 2013). These papers also show nice isochronal features that were mentioned in this section.*

**[#23] Page 13, Lines 21-24**

**This sentence needs some revision, I am not completely sure of the meaning so it is hard to give advice.**

*This part was rewritten as follows. We hope that these are understandable for readers.*

*“DFGT2006 is a time scale based on a sedimentation model, with sedimentation parameters being constrained using some dated horizon. It is not strictly constrained to dated horizons, as DFO2006 is.*

**[#24] Page 14, Lines 11-12**

**In figure 5 d18O is plotted. For consistency could you plot dD in Figure 4, or is there a good reason not to?**

*We repaired this point. We plotted  $\delta D$  instead of d18O in Figures 3, 4 and 8.*

**[#25] Page 14, Lines 20-22**

**This is obviously a very interesting result but it should be more quantitatively reported, for example, the value of r and how sensitive it is to the time shifting used to find the maximum. I think the reader needs to see more details.**

*In order to show data in more details, we add two figures. We show a figure with the correlation coefficient as a function of the shift for the entire time span. In addition, we show another figure with the value of the phasing as a function of time. We think that to show these figures make the paper more interesting and informative. We added explanations and short discussions on these at bottom 14 lines at page 14.*

*In addition, in the concluding remarks, 15 lines of item (ix) was added to highlight importance of this result.*

**[#26] Page 15, Lines 7-10**

**Can you provide information about how well they are confirmed? In other words, can you quantify, briefly, how consistent they are?**

*In the revised manuscript, at 4th-11th lines in the section 4.4, we provide information. We quantitatively explained.*

*“We confirm that three of the links (DF 1361.89/EDC1265.1 m, DF 1849.55/EDC 1796.3 m, DF 2170.18/EDC 2150.9 m) are consistent with the matches we have made using the pattern of volcanic marker peaks in this study; deviation of these links from*

*the track of the DF/EDC volcanic match links is within 0.08 m. Thus these three of the links independently support the matches we have proposed at these depths. The fourth one (DF 2117.75/EDC 2086.6 m) is not consistent with our synchronization; deviation of this link from the track of the DF/EDC volcanic match links is approximately 2 m. “*

**[#27] Item (viii) in conclusion**

*Mention to the paper 08 was removed.*

**[#28] Page 19, Lines 4-5**

**This sentence is hard to understand.**

*We revised this part as follows.*

*“Along the sequence of the irregularly distributed tie points, deviation of each tie point from an interpolated track of the surrounding tie points is in most cases within 0.1 m, as we discuss below.”*

**[#29] Page 19, Line 10**

**The meaning of this sentence is also difficult for me.**

*This sentence was simply removed.*

**[#30] Page 35, Line 9**

**Interesting point - can you mark the spot on the figure?**

*We revised this part as follows. “This set of examples contains a plausible Toba super eruption that occurred sometime at ~74 kyr BP, studied by Svensson et al. (2012). Tie points with ID numbers 513, 515, 517 and 518 (shown as letters T1-T4 in (e)) were*

*discussed by Svensson et al. (2012) (see their Figure 8) as tie points of the Toba super eruption “.*

*In Figure 2(e), these tie points were indicated.*

**[#31] Page 36, Line 6 and Page 38, Line 8**

**Best to specifically indicate that this is d18O<sub>ice</sub> and the standard reference material (VSMOW?).**

**Please add that this is d18O of the DF ice. In caption or somewhere also indicate that it is relative to V-SMOW (I assume this is the case).**

*Please see #24. We replaced  $\delta^{18}O$  by  $\delta D_{ice}$ . In figures and in the main text, we used  $\delta D_{ice}$  (‰, VSMOW) or simply  $\delta D_{ice}$ .*

**[#32] Page 37, Line 4**

**Need to replace this word as it implies that you are uncertain about which axis to use. Is the point that it does not really matter which axis is at top and bottom? In that case I would recommend deleting "tentatively."**

*We removed this word as suggested.*

**[#33] Page 38, Lines 11-13**

**For me it is slightly confusing to have DF-speleo then speleo-AICC2012 but if there is a good reason for not subtracting speleo from both I accept the difference.**

*In the revised manuscript, we added a sentence. This was the reason.*

*“Note that a reason for not subtracting speleo from both is to make comparison between Figure 4a and 4d easier at MIS5. “*

**[#34] Page 39, Line 13**

**should indicate that the red line from 4a is reproduced in Figure 4b.**

*We explained in the main text as suggested.*

*Other changes*

*In the abstract and in the item (ii) in the concluding remarks, description about the phasing was revised based on addition of Figures 7 and 8*

*In the Supplementary Information A, the last page (80) was lacking by a careless mistake. We add a page 80.*

*We removed all descriptions citing the paper 08.*