

## ***Interactive comment on “Volcanic synchronization of Dome Fuji and Dome C Antarctic deep ice cores over the past 216 kyr” by S. Fujita et al.***

### **Anonymous Referee #1**

Received and published: 4 March 2015

Fujita et al. describe the synchronization of the Dome Fuji and Dome Concordia ice cores using volcanic tiepoints 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.

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.

No direct tephra matches are provided, and volcanic synchronization is based on the matching of ECM patterns. My main (and only) concern is that the manuscript lacks

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a robust evaluation of the volcanic matchpoint identification. No quantitative criterion is given for assigning or rejecting potential tiepoints. 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.

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.

— 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.

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, SO<sub>4</sub> 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?

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

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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.

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.

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?

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.

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  $\Delta z_{DF}$  and  $\Delta z_{EDC}$  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  $\Delta z_{DF} / \Delta z_{EDC}$ .

2) Mention the companion paper (Parrenin 2015) somewhere in the introduction, and

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describe briefly how the two manuscripts are related.

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.

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.

5) For figure 4: I assume there are 2 O<sub>2</sub>/N<sub>2</sub> 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 tiepoints makes this speculative, of course. It may be worth pointing out.

6) Please indicate where the ECM data and volcanic tiepoints can be accessed. The matchpoints (depths) should be included as a supplementary data file. Ideally the same would be done with the ECM data also.

— language/technical —

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 O<sub>2</sub>/N<sub>2</sub> age

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constraints\*, . . . .

P408 L14: glaciological \*approach that is more weakly constrained \* by age markers.

P409 L8, L21: \*age\* scale

P409 L28: define ACECM

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 ..

P411 L16: Do you have a reference here? Logging practices differ somewhat between countries.

P411 L23 \*hiatuses\*

P412 L12: \* and surface snow redeposition processes such as sastrugi.

P412 L22: remove “with confidence”, or describe what this confidence is based on.

P412 L29: again, “confidently” is rather subjective unless you quantify it.

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

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P414 L5-6: “there are tails...entire MIS 5”. I have no idea what this means! Please clarify or remove.

P414 L6-9: “Over the period . . . 4, 3 and 2”. Rewrite this sentence or remove.

P414 L12: “gradient” should be “slope”, or “derivative”. (gradient is commonly used when there are more than 1 dimension).

P415, L7-12: this is not completely fair. AICC2012 also uses O2/N2 age constraints

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.

P416 L22: Please explain what we should expect to see here if AICC2012 perfectly respected its age constraints.

P416 L26-28: Use consistent age uncertainties for the O2/N2 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 O2/N2 uncertainties at DF to 4ka also, or you reject it, and set them all to 2ka.

P417 L13: do you mean to say that AICC2012 does not fit its own age markers? Please elaborate.

P4174 L19-20: AICC2012 does not include constraints from firn densification modeling. Please rephrase. The AICC Delta-depth approach has many uncertainties also.

P418 L10: “this possibility” WHAT possibility??

P419 L4: age \*difference varies, with peak differences\* at MIS . . .

P419 L10: consider moving the discussion on speleothem ages to its own section for clarity.

P419 L21, L22 and L23: “is deviated” should be changed to “deviates”. Also on P420

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L7

P419 L28 MIS 5c, 5d \*and 6\*. Remove: “reliability of”

P421 L4: remove “on a time series”

P421 L20: Note that during MIS5a-5c AICC compares really well with the speleothems

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

P436, caption: please state what the horizontal axis is. Time? Depth?

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Interactive comment on Clim. Past Discuss., 11, 407, 2015.

**CPD**

11, C57–C63, 2015

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