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Discussions

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 #3

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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 speleoterm record from China. A companion paper describes the conclusions gained about past changes in surface mass balance (SMB) ratio.

General comments:

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).

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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.

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.

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

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.

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.

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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.

Specific comments:

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 tiepoints were extracted manually, and which were extracted automatically, so that the performance of the manual vs. automatic routines can be assessed.

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 tiepoints is estimated. The authors e.g. mention that they use the Vostok data to do crosschecks. How were these cross-checks performed? The authors also mention that some of the tiepoints 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.

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 synchroniza-

tion without having access to these data sets.

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.

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?

The authors seem to conclude that the O2N2 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.

Technical corrections:

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

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Isotope Stages.

P. 408, Line 5-6: Strange sentence. Perhaps change: "Characterized by strong constraining by the O2/N2" -> "strongly constrained by O2/N2"

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

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

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.

P. 410, line 8: Please add that Bazin et al also used gas stratigraphic markers.

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.

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.

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

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.

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.

P. 412, line 21: "one or more" -> "two or more"

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

P. 412, line 28: How were these crosschecks made?

P. 413, line 1: "Supplement" -> "Appendix"

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

P 413, line 12: "Characterized by" -> "developed based on"

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.

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

P. 414, line 7, 8, 9: "difference for x kyr" -> "difference over a period of x kyr" (repeated 3 times).

P. 414, line 17-19: "we use these... EDC cores". I find it strange that the authors here decide to use the O2N2 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.

P. 414, line 22, 26: Please refer to age instead of ID values.

P. 414, line 25, 28: Where does the uncertainty value of 2.7 ka come from? The uncertainty of the O2N2 markers is stated to be between 2-4 ka.

P. 415, line 1-5: This paragraph is very hard to read.

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 tiepoints, thinning function, influence by links from other cores, SMB, etc.

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.

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.

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.

P. 418, line 1-2: I have a hard time believing that simply by looking at the shape of internal isocrones over these large distances, one would be able to infer whether or not

the two employed thinning functions are correct.

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

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.

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 O2N2 age markers are in error in the section around 90 ka. 2) All other O2N2 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 tiepoints are correct within their associated uncertainties.

P. 420, line 9: I assume the authors here are talking about the AICC2012 agescale.

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.

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.

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

P. 421, line 13: "driven . . . mainly" -> "not driven"

P. 421, line 18: The authors ought to specify that this is not the case for the section 5a-5c.

P. 422, line 10: Most of the TAC age markers and O2N2 age markers are compatible. Only the marker with ID 6 is incompatible.

P. 422, line 27: Just because the tiepoints 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.

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 tiepoints are highly correlated: If one tiepoint happens to be chosen wrongly there is a high chance that the next tiepoint 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.

P. 424, line 2: How can the value of 0.1 be derived from figure A3?

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 tiepoints, and can be removed. Is the blue dot a preliminary hand-picked tiepoint? Which would then be the automatically picked tiepoints? This figure focuses on showing the automated interface for picking tiepoints. 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.

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.

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.

Table 2-4: It is confusing that age markers from both DF and EDC are simply labelled 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.

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?

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