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Climate of the Past 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 #2**

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General comments: 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. 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. 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. 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 palaeo-climatology. 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.

Specific comments: 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? Why does a dating offset of 2-4 ka BP some 120 ka ago matter? Does it limit our understanding of past climate? What is the take home message? Should DF2006 be used in the future instead of AICC2012? Please clarify your motivation and significance of these kind of evaluation. 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. 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. L. 24: Are there conventions in which order ice-core analyses are performed? Please explain. L. 29: What do you mean with profiles? Concentration measurements? Page 410: L. 1: How can you locate an event solely based on electrical properties or sulphate? This is no tephra. 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. L. 5: Why is the number of tie-points in earlier studies so small relative to the numbers you give in the abstract? L. 21: This is redundant. 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. Page 411: L. 15-16: This sentence can be deleted. I believe measuring the length of ice cores is well established. 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? 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. 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. 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. 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. 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? 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. L. 20: What are “patterns of data fluctuations” 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? Is ECM better suited than sulphate? Or are Dome Fuji measurements of higher quality than others? 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. L. 14-15: I don’t understand this sentence. What is surface mass balance and how is it measured in ice cores? 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/) 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? Why should the signal diffuse in the ice? These are no measurements of stable isotopes of water. Page 414: L. 5: What are “tails of the profile”? L. 7: Deducing a “periodicity” from two apparent cycles seems risky to me. L. 13: What are these “climatic events”? Do they have a name? Why are they important? Please be more specific. 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. Page 415: L. 7: What is interpolation of age markers? L. 12-18: Unclear sentence. Please reword. 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 O2/N2 measurements from Dome Fuji are so much more precise than those from Vostok? Is it due to improved methods? Or due to the sampling sites? If it is common that O2/N2 measurements are more precise than TAC measurements why have these measurements not been performed previously on one of the AICC2012 ice cores? Page 418: L. 1-2: What parameters in the ice form the isochrones visible in the radar? Dust? How do you then link the O2/N2 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? 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? Which measurements are used and which parameters? Page 419: L. 13: How are speleo-ages determined? Page 420: L. 3: What makes O2/N2 an absolute age marker? Haven't they been orbitally tuned? L. 14: Duration of what? L. 22: What have water isotopes to do with SMB? Page 421: L. 3: A speleothem record. There is probably more than one. L. 5: Redundant use of "time" 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? 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? Page 422: Volcanic events are actually much more frequent than 1 in 154 years. Write volcanic signal frequency in our proxy records instead. 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? Page 424: Are the datasets used in this study already submitted to data repository?

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

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