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Interactive comment on “Dating of a Dome Fuji (Antarctica) shallow ice core by volcanic signal synchronization with B32 and EDML1 chronologies” by Y. Motizuki et al.

Anonymous Referee #1

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1. General Comments The main goal in this manuscript is to provide a timescale for a short ice and firn core (Dome Fuji) from Antarctica. This is achieved by volcanic synchronization using the sulfate record from another ice core (B32) over the last 2,000 years. The main conclusion of their analysis is, that synchronization is possible using 31 volcanic eruptions and that mean accumulation rates calculated between the used tie-points is fairly constant and thus confirming the initial synchronization. Accurate timescales are essential for palaeo-climate studies, synchronized timescales allow to compare different records from different sites and allow to retrieve some spatial information on past climate change. This study certainly provides a better timescale than was available previously for this site at Dome Fuji. The suggested synchronization to

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B32, appears robust and is very valuable for the ice core community as it allows to linking other Dome Fuji records with various ice core records that have been synchronized to EDML or EDC earlier. This manuscript, however, could take more use of this new timescale and/or synchronization to advance our understanding in past climate variability. I suggest that the authors include at least one application that is based on the new synchronized timescale to demonstrate the potential of this ice core using this new dating in reconstructing palaeo climate. Showing and discussing the existing Dome Fuji 10Be record that is already mentioned in the manuscript on the new timescale could be such a first application. If more space is required for such an application, I suggest to excluding the synchronization with the EDML1/EDC3 timescale, which is currently not adding much information, as the annual dated B32 chronology is presumably more accurate. It will also make the manuscript and figures easier to read with only one timescale to display. The main conclusions are supported by the data. However, some details need to be reevaluated and the language could need some improvement. Overall, this manuscript should be published after the raised concerns have been fully addressed.

2. Specific Comments The manuscript could strongly benefit from language and grammar corrections by a native speaker if that was possible. Not being a native speaker myself I can only recognize some language related issues but not provide the necessary corrections. Page 770: Line 2: You can perform synchronization or you can find correlation. 2: Look for alternatives for “extremely good”! Maybe tight synchronization? 7: Remove “careful”. I assume you did it careful. 14-21: Relative confusing and too much detailed information. I would suggest to only discussing ONE synchronization (with the annually dated B32 ice core). The difference between DFS01 and DFS02 are only the differences between an annual layer dated ice core B32 from DML and a flow model based timescale from Dome C. One can surely assume the B32 chronology is more accurate than the one from EDC! If you used the second DFS02 chronology only to have ages for the entire 2kyrs, including the 4 additional events from EDML, I would suggest to calculating the difference between B32 and EDML1 for the last

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common event, and apply those differences to the 4 events only visible in the EDML1 core. Currently, it is also not absolutely clear from the abstract which sulfate record you used for the deeper part: B32 only, or also EDML? Page 771: Line 1-13: Very long. In short: You need the short ice cores to calibrating the long deep ice cores against recent observations. 21-24: Please give references which timescale was using which stratigraphic constraints. Not every timescale was using all possible tie-points. And many of the constraints used were for very different time periods than the one discussed in your paper (e.g. orbital tuning, Laschamp event). Which ones have been used during the last 2,000 years? Page 772: Line 5: What is the error? The quantification uncertainty for a single year? Or the standard deviation for the annual mean of the 12 annual values? If it is the latter, than this shows a surprisingly low inter-annual variability. 13-14: If the chemistry is so different from sea-salt why do you later perform a sea-salt correction based on average sea water chemistry? Page 773: Line 4: The counting of annual layer signals would not be that difficult if they could only be resolved. But as you are having 0.7-1yr resolution you just cannot resolve them. Please change wording accordingly. 7: The probability of zero accumulation of 9% is in contradiction with the low variability in MAR as described before (27 ± 1.5)! Please check again what the error really shows! Page 774: Line 2: What is “correct” and how would you know? Please use either “precise” or “accurate”. 3: “manually prepared”? Does that mean discrete samples? 5: ...insufficient to resolve/identify/interpret annual layers. Delete “To overcome the problems and ...” 16: Better: ...transfer the annual-layer counted timescale? 18-26: Why do you need the EDML1 chronology? This timescale is not based on annual layer counting at EDML (but comes from EDC). It would be much more consistent and the manuscript easier to read if you decide for one timescale for a reference chronology. Especially, as you are not showing the nssSO₄ record from the main EDML ice core in your manuscript. Page 775: Line 2: Do you mean synchronization errors or the errors in the absolute ages of EDML1 and B32 which you imported during your synchronization process? 15: Remove “in fact” 22: Remove “unfortunately” Page 776: Line 1-13: Please give at least one reference reporting in more detail sam-

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pling processes, detection limits, measures of quality control, blank, reproducibility of results at RIKEN using IC instrumentation. Page 777: Line 15-17: What does “confirmed” mean? Did the two models agree within 1 year, 10 years, 100 years? During which time periods? You would expect that a timescale based on 1-D flow modeling is at least giving similar results than one based on a 3-D model, as they probably share a lot of common input data. 20: Use better verbs than “sending” and “rising” maybe injecting or emitting? 24: Better: This deposition results in high concentrations. . . Page 778: Line 1-17: This is a minor point. But, I am not convinced that a correction for sea-salt contribution is necessary for your study and if chloride would be the parameter of choice to use for correction. The sea-salt contribution is low (8%) and probably fairly constant at such a high inland site. Every large eruption should be detectable even before this minor correction was applied. Chloride is a by-product for some volcanic eruptions (HCl) as shown in Greenland ice cores. If you assume all Chloride is from sea-salt you might potentially bias your results for HCl rich signals (if they exist) in Antarctica. 22: You can’t see a synchronization in Fig. 2. You can maybe see that using the suggested (initial) scaling factor to account for the difference in mean accumulation rate at both sites the SO₄ records look very much alike and that the sulfate peaks probably reflect the same volcanic events. Page 779: Line 14-16: remove “annually resolved” or write approximately annually resolved 24: If you find 94 events and Traufetter et al. find 49 events in B32, why do you only use 31 events for synchronization? Does it mean Traufetter et al. have found additional events that do not show up at Dome Fuji? 27: Please define the alpha value when first mentioning it. Page 780: Line 1: remove “accurate” 3: remove “it is clear” 15: remove “as mentioned in section 1” 26: How do you define “volcanic activity”? Magnitude of eruptions? Frequency? Combination of both? Page 781: Line 1-4: It would certainly be good not only to have the dates but also to be able to show the according sulfate record. The publications showing this record is cited in this manuscript (Severi et al. 2007, Ruth et al. 2007). Why is the SO₄ record not be shown? 11: Remove: “One sees. . .” 13: Remove: “It is mentioned here” 17: Typo: Kohnen Page 782: Line 14: Assuming the mentioned “features” in 10Be and

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14C are indeed for the same event your approach gives an absolute age uncertainty range for the year 765 BC. But how do you estimate the uncertainty for your time period 1-2000 C.E.? 18: remove “insisted”, use “found” or “suggested” instead 20: “matching reference”? Better maybe: reference ice core, reference chronology 25: There is already a newer date than those references: “We present here a new accurate and precise eruption date of ad 232 ± 5 (1718 ± 5 cal. BP) for the Taupo event” (Hogg et al., 2012 22: 439 The Holocene). I would also be careful with the attribution of Taupo to the signal in 221 ± 22 , because your other signal in 250 ± 22 also is within the age uncertainty not different from the new (14C wiggle-match) age. Accordingly, I suggest to add the second possible candidate for Taupo in Table 1. Page 783: Line 22: Which are the “known” eruptions? Known from what? Earlier ice core studies? Other proxies? And even if the eruptions were known, what evidence was in the ice to attribute the ECM spike to the very event? Clearly, Watanabe et al., 1997 must have had very different MAR in the 1st millennium relative to more recent values, with ages younger by almost 200 years. Any comments on this? Page 784: Line 14-20: Can you directly show the 10Be from Dome Fuji on your volcanic synchronized timescale against the radiocarbon curve, in addition or instead of only the age differences in the two dating methods? 24-25: How was synchronization achieved between these ice core records? Page 785: Line 3: Is there a reference for the magnitude of the “536 event”? Was it really bigger than Tambora? It does not look like in your figures. 1-5: Two large eruptions are recorded in Greenland according to Larsen et al. (2008). How would they know which one was tropical if all ice cores in Antarctica had uncertainties of >10years? 5-9: There must be some errors in Table 4: You say DFS02 ages are consistently younger than DFS01 ages, but you have an age of 515 in DFS02 for which you give 543 in DFS01. And why do you have for certain events only ages on one timescale but not on the other? Are not both timescales based on the same synchronization between B32 and DF01 and differences of DFS01 and DFS02 only due to the timescales in the reference chronology? Furthermore, there seems to be some synchronization error between DFS01/02 and WAIS Divide/Law Dome between 300-400 A.D. For ages after

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400 A.D. DFS01 ages are consistently (on average 12-15 years) younger than Law Dome and WAIS Divide dates. Before 300 A.D. they are suddenly and consistently (10-18 yrs) older than Law Dome and WAIS Divide. If that synchronization was correct, that would suggest that within 130 yrs those two timescales have 30 years less annual layers (-25%). I would suggest, to double check those synchronizations and eventually to moving down all volcanic events before 300 by one position. This would result in age differences between the various timescales that are not abruptly changing. With the table as it is, the good agreement between DFS02 and WAIS Divide/Law Dome is only because the synchronization error is compensated by the absolute age difference between EDC3/EDM01 and WAIS Divide/Law Dome, and should not be seen as measure of the accuracy of the EDC3/EDML01 timescale. 14: 28.9 mm yr-1 after Tambora seems not to be “clearly increased” with respect to recent values 27.3 mm yr-1 or the long-term mean. I would rather argue that the value of 22.2 mm yr-1 between 1810 and 1815 is exceptionally low. And why should MAR be increased after volcanic eruptions? Is there any mechanism involved? 27: Remove “extremely good” Page 786: Line 1-10: Consider to shorten this section. It is quite obvious that averages over longer time periods do not vary as much as averages over shorter time periods. 17: High-resolution is a strong statement given that there are no tie-points between 1170 and 690. I would call it with high confidence between 1-700, 1100 and 2000 and give a maximum interpolation error for the time without tight synchronization (around 900) based on your 15% interpolation error. 23: It would have been nice to give an example for an application already within this manuscript, which is in its current version a pure “dating paper”.

Table 1: I don't see how an attribution of Cerro Bravo to the signal in 1342 can be made, given an error of 75 yrs. Age of Taupo is 232 ± 5 years, and this age can be attributed to 2 signals in B32 based on dating errors Table 2: Here the ages of EDML1 are consistently younger than B32 (unlike in Table4) How is the absolute dating error of 1171 estimated? Table 3: The abrupt changes in MAR for DFS2 during the 6th century from 27 to 20 to 32 and back to 27 mm yr-1 that are absent in the synchronization of

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B32 (DFS1) strongly suggest some error in the synchronization or in the EDML1 ages at 566. You might want to double check the tie-points and ages in the table and also in Fig. 06. Table 4: Please double check synchronization of B32/DFS01 and Law-Dome/WAIS Divide before 300 AD as discussed above and attribution of Taupo232. And correct DFS01 and DFS02 ages according to Table 2. Fig. 2: Was the synchronization between DF01 and B32 done using the B32 volcanic sulfate or the B32 total sulfate records? Fig. 3: Would it be possible to show all time-series (Fig.3,5,6) with the timescale axis flipped, so the youngest part is on the right to be consistent with other climate reconstructions? Fig. 4: Part of this plot (b) only shows the difference between EDC3 and B32 dating. Of interest are, however, the rapid changes at around 66m (500 AD), probably indicating the same issues as discussed for Table 3. Fig 5: In addition to showing the age difference to Horiuchi et al., can you consider showing the ^{10}Be data on your new timescale and the independent ^{14}C curve? This would be interesting especially during the time period in which your synchronization is not using tie-points from volcanoes 700-1170 AD but which do show some strong variations in ^{14}C . Fig 6: Please check again the tie-point at 567 AD for DFS2. If the synchronization is based for both timescales on B32 and DF01 the resulting MAR cannot be that different! A flow model based timescale (EDC3/EDML01) does not have accumulation rates changing by 50% within a few decades as suggested by this figure.

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