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TCD
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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.

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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 infor-

mation 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 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.

Response: We thank the anonymous reviewer #1 for her/his helpful comments and suggestions.

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.

Response: In the next revision, we will show the 10Be flux data on our new timescales and the independent 14C production rate curve as suggested. Because of this 10Be data inclusion, Dr. Kazuho Horiuchi will be a coauthor of the next revision. With regard to your second point, we do not think the B32 ice core dating is necessarily more accurate than the EDML1/EDC3 timescale. For example, examination of Figure 2 of Sigl et al. (JGR 118, 1-19, 2013) shows that there is a discrepancy between the B32 timescale and the timescale of the WAIS Divide ice core, which was recently dated by counting annual layers, of –17 years at AD 200. Chronologies of Law Dome, North-

GRIP, and NEEM cores, presented in the Figure 2 together with B32, do not show such a large discrepancy with the WAIS Divide core chronology. The large discrepancy will be diminished when we consider the EDML1 age, since the EDML1 age is systematically younger than the corresponding B32 age by approximately 10-20 years before AD 1170 (Table 2 and Figure 4b). Hence, we think that inclusion of the DFS2 chronology based on synchronization with the uppermost part of the EDML1 core is meaningful. As we mention later, Dr. Mirko Severi kindly agreed to provide us with unpublished SO4 data from EDML1 for our manuscript, and we will display the synchronization between the DF01 SO4 data and the uppermost part of the EDML1 SO4 data (before AD 187) in a new figure. We could not include such a figure before, because we had only the eruption date information from the EDML1 core (M. Severi, personal communication, 2011, as described in our manuscript). Because of the inclusion of these unpublished data, Dr. Severi will be a coauthor of the next revision.

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.

Response: We will have a native English editor review the entire manuscript to improve the English of the next revision, and we will incorporate the corrections that you suggest above.

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 common event, and apply those differences to the 4 events only visible in the EDML1 core.

Response: As mentioned above, we think the B32 core chronology might not be the most accurate. Therefore, our intention is to include the DFS2 chronology, which is based on synchronization with the uppermost part of the EDML1 core, to show the dating uncertainty in the first millennium. As you suggest, we will include the four additional events from EDML1 as new data (the EDML1 SO4 data mentioned above).

Currently, it is also not absolutely clear from the abstract which sulfate record you used for the deeper part: B32 only, or also EDML?

Response: For the deeper part we used B32 only, except for the information on the four additional eruption dates from EDML1 before AD 187. We could make our DFS2 chronology after AD 187, simply because the corresponding age in the EDML1 dating with the B32 time markers was already published (Ruth et al., Clim. Past, 3, 475, 2007), and we could use the one to one relation between the tie points of the "B32-Year" and the "EDML1-Year" that was given in a supplementary table of Ruth et al. (2007; in the material "Volcanoe-stratigraphic synchronization B32 vs. EDML vs. EDC"). We will take care to be clear about this in the next revision.

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?

Response: We will shorten the first part of our explanation in the Introduction in the next revision by including mainly information that relates to volcanic synchronization for the last 2,000 years, as suggested.

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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.

Response: The error in the accumulation rate was quoted from Kameda et al. (2008, J. Glaciology, 54, 107). This measurement was performed using 36 bamboo stakes (grid of 6*6, placed at 20 m intervals) from 1995 to 2006. The quoted error 1.5 mm water-equivalent yr-1 is the average of 36 bamboo stakes of each standard error (also called 'standard deviation of the mean') for a single-stake that indicates the uncertainty of 68.3% scattering for the mean of the accumulation rate from 1995 to 2006 (e.g., Taylor, 1997*). The error value was given in Table 1 of Kameda et al. (2008). The error value, however, is not essential in our paper and hence in the revision we will just mention that the accumulation rate was observed to be 27.3 mm water-equivalent yr–1 from 1995 to 2006 by a stake method. * Taylor, J.R. 1997. An introduction to error analysis: the study of uncertainties in physical measurements. Second edition Sausalito, CA, University Science Books (quoted from Kameda et al., 2008).

13-14: If the chemistry is so different from sea-salt why do you later perform a sea-salt correction based on average seawater chemistry?

Response: We consider the ion contents observed at Dome Fuji to be a mixture of stratospheric and tropospheric ions. We used the sea-salt correction to remove the tropospheric component from the SO4 measurement data. To clarify this, we will include a more explicit explanation of the composition of the precipitation recorded in the DF01 core in the Introduction.

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:

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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?

Response: In the next revision, we will improve our manuscript by properly taking into account these suggestions and comments.

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.

Response: We consider that the B32 timescale, though very useful, is not the best one. As noted above, Sigl et al. (2013, JGL, 118, 1) show (their figure 2) that the B32 chronology certainly differs from the WAIS Divide core (WEC06A) chronology, by as much as –17 years at AD 200. Chronologies of Law Dome, NorthGRIP, and NEEM cores, presented in the figure 2 together with B32, do not show such a large discrepancy with the WAIS Divide core chronology. We understand that the top part of EDML1 may also have a large uncertainty, as you suggest, but the discrepancy between the EDML1 chronology and the WAIS Divide core chronology in the first millennium is considered to be smaller than that for B32, because the EDML1 age is consistently younger than the corresponding B32 age by approximately 10-20 years before AD 1170 (Table 2 and Figure 4b). Furthermore, as Dr. Mirko Severi has provided us with unpublished SO4 data from EDML1 for the uppermost part, in the next revision we will be able to add a figure showing the synchronization between EDML1 and DF01.

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”

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Response: We will explain the errors in our revision and improve our manuscript as you suggest.

Page 776: Line 1-13: Please give at least one reference reporting in more detail sampling processes, detection limits, measures of quality control, blank, reproducibility of results at RIKEN using IC instrumentation.

Response: Sampling processes will be described in more detail in the present paper in the next revision. The maximum analytical errors for Na+, Cl–, and SO42– were 2% and the detection limit was 1 μg/L, and blank levels of these species were less than the detection limit. Furthermore, during every after completing each set of analyses, we checked the linearity of the calibration lines of all chemical species and confirmed that the error limits of the slopes were less than 2% for Na+ and less than 1% for Cl–, and SO42–. We will add this information to our present paper. In addition, we would like to refer to our recently completed paper in the next revision âĂŤ Yuko Motizuki, Hideaki Motoyama, Yoichi Nakai, Keisuke Suzuki, Yoshinori Iizuka, Kazuya Takahashi: "Chemical composition and unique characteristics of shallow ice core samples from a Dome Fuji core (Antarctica) drilled in 2001 âĂŤ which has been submitted to the International Journal of Environmental Analytical Chemistry. In this paper, we showed our typical chromatograph of anions and cations, related to the measurement precision.

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.

Response: We will address these problems in the revision in order to improve our manuscript.

20: Use better verbs than "sending" and "rising" maybe injecting or emitting? 24: Better: This deposition results in high concentrations. . .

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Response: We will take into account these suggestions to improve our manuscript.

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.

Response: We used nssSO4 data, not total SO4 data, for our discussion partly because in our based B32 core paper (Traufetter et al., Journal of Glaciology, 50, 137, 2004; see their figure 2), nssSO4 data were discussed. We requested SO4 and Na data by a personal communication with Drs. Hans Oerter and Rolf Weller (2011) and obtained the B32 data on SO4 and Cl but not Na. Drs. Oerter and Weller requested us to use Cl- instead of Na+ to derive nssSO4 for the B32 core. Because of this, we compared our nssSO4 concentration record derived by using the Cl- data with those derived by using Na+ data, and obtained almost the same result as described in the text (p. 778, Line 13). We will add a figure for the nssSO4 data obtained from equations 1 and 2 according to a comment by the referee #2. We also showed in the Motizuki et al. paper submitted to the International Journal of Environmental Analytical Chemistry (mentioned above) the depth profiles of the Na+ and Cl- concentrations and their correlation. The Na+ and Cl- concentrations in the DF01 core are not almost constant but there are peaks (spikes). We furthermore note that, in preceding studies, no volcanoes are known to have produced HCl in their past eruptions and to have affected Antarctica. We thus believe that even if such HCl-rich eruptions (in the south hemisphere) occurred and even if they were involved in our data, overall, the HCl effect would have been negligibly small in our analyses. Therefore, we think, as you suggest above, that the conclusion is the same whether total SO4 or just nssSO4 is used, but we here used nssSO4 just to make sure.

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

Response: We will correct these problems and properly improve our manuscript, taking into account your suggestions above.

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?

Response: First, the DF01 core includes only the period before 1900 AD (the records of the most recent 100 years are missing, as we described in the manuscript). For this, 5 volcanic signals from Traufetter et al. (2004) for the B32 core were not used. Also, 2 volcanic events couldn't be confirmed in the provided B32 core data (1889 and 1886 events as described in Table 1). The volcanic nssSO4 signals of other events in both the B32 and DF01 cores are small (as in the cases of 1762 and 1542 events; Figure 3) and we did not dare to synchronize those signals. Overall, the annual-scale DF01 nssSO4 concentration spikes are the same level or larger than the annual-scale B32 nssSO4 concentration spikes as shown in Figure 3.

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? 11: Remove: "One sees..." 13: Remove: "It is mentioned here" 17: Typo: Kohnen 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 plus/minus 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.

Response: By "Volcanic activity" we meant both magnitude of eruptions and the frequency. In our revision, we will take care all of these problems and will properly improve our manuscript by taking into account your suggestions above.

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?

Response: The EDML1 papers were cited but all the SO4 data from the EDML have not been released. In this opportunity, Dr. Mirko Severi agreed to provide us with unpublished SO4 data from EDML1 for our manuscript, and we will display the synchronization between the DF01 SO4 data and the uppermost part of the EDML1 SO4 data (before AD 187) in a new figure. We could not include such a figure before, because we had only the eruption date information from the EDML1 core (M. Severi, personal communication 2011, as described in our manuscript). Because of the inclusion of these unpublished data, Dr. Severi will be a coauthor of the next revision.

Page 782: Line 14: Assuming the mentioned "features" in 10Be and14C 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.?

Response: The one to one relation between the tie points of the "B32-Year" and the "EDML1-Year" that was given in a supplementary table of Ruth et al. (2007; in the material "Volcanoe-stratigraphic synchronization B32 vs. EDML vs. EDC"), but the errors of the tie points in EDML1 were not given there. On the other hand, Parrenin et al. (Clim. Past. 3, 485, 2007) showed the age markers used for the construction of the EDC3 age scale in their Table 1. We could find from their Table 1 that the time marker of EDC3 just before 1171 AD (0.779 kyr BP in Table 1) is 765 BC (2.716 kyr BP in Table 1), and the estimated error for the 765 BC time maker was given as plus/minus 50 years in the Table 1. Then assuming the constant precipitation rate between 1171 AD (fixed) and 715 (=765-50) BC and also that between 1171 AD (fixed) and 815 (=765+50) BC, we can estimate the age shift of the tie points of EDML1. These we regard as rough error estimates in the EDML1 age and presented in the fifth column of our Table 2 (p. 796), since the errors in the volcanic horizons in the EDML1 time scale are based on those in the EDC3 chronology as described in the text (p. 782, L8). We will take care to be clear about this in the next revision.

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?

Response: Based on “suggested” ages of volcanic eruptions given in earlier ice core studies, Watanabe et al. (1997) allocate the ages to some of the ECM spikes. The word “known” in Line 22 was not appropriate and will be deleted in our next revision. We also found a mistake in their estimate of a MAR in Watanabe et al. (1997). This will be commented concisely in our revision.

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?

Response: The 10Be profile with 14C curve using our DFS1 and DFS2 timescales will appear in the next revision as suggested.

24-25: How was synchronization achieved between these ice core records?

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Response: The actual synchronization was not performed using SO4 data from the WAIS Divide, Law Dome, and the GICC05 chronologies; just compared with the dates of the tie points within errors.

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?

Response: The suggestion that the 536 event was larger than Tambora was found in Larsen et al. (2008). But as you suggested, our data do not support this. We will remove the three Green land dates (Larsen et al., 2008) from Table 4 in our revision to avoid complexity; this will not affect our purpose to present Table 4 to see whether DFS1 or DFS2 was more consistent with other chronologies.

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 after400 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.

Response: We admit that Table 4 in its present form contains mistakes as you suggested. We will improve Table 4 by taking into account all your observations given above, although we now better understand that synchronization using only dates is not robust.

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?

Response: We rather consider that the change in MAR from the preceded value may be meaningful. This is because Fujita et al. (The Cryosphere, 5, 1057, 2011) found that the accumulation rate in the second half of the 20th century in the eastern part of DML including Dome Fuji was found to be higher by approximately 15 % than averages over longer periods of 722 yr or 7.9 kyr before AD 2008. We thus discussed the sudden jump in the MAR from 22.2 to 28.9 mm yr-1 after the Tambora eruption. We will also point out that 22.2 mm yr-1 between 1810 and 1815 is exceptionally low as suggested; the reason for this at this moment is unknown. After a very large eruption, precipitation may be increased as a result of increasing sulfate aerosol particles, because clouds tend to be induced. We will clarify these in our revision with proper references.

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”.

Response: We will improve our manuscript by taking into account these comments and suggestions.

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.

Response: The allocation of the Cerro Bravo eruption was followed Traufetter et al. (2004). However, in our revision, we will properly improve Table 1 by taking into account your suggestions above.

Table 2: Here the ages of EDML1 are consistently younger than B32 (unlike in Table4). How is the absolute dating error of 1171 estimated?

Response: The 1171 tie point (‘1171’ is based on Ruth et al. 2007) is the last common event between the B32 and EDML1 chronologies, although it was given as ‘1170’ in Traufetter et al. (2004). So the dating error of 1171 was quoted as 6 years, from the work by Traufetter et al. We will mention this in our revision.

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 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.

Response: The synchronization in the EDML1 age in Table 3 are just followed the one to one relation between the tie points of the “B32-Year” and the “EDML1-Year” that was given in a supplementary table of Ruth et al. (2007; in the material “Volcanoe-stratigraphic synchronization B32 vs. EDML vs. EDC”). If the synchronization between the B32 and the EDML1 given by Ruth et al. is reliable, then the accumulation rates in EDML1 are as we show in Table 3 and Figure 6. We have double-checked and confirmed the accumulation rates. We will keep in mind this problem and will comment this in our manuscript in the next revision.

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.

Response: We will improve Table 4 by taking into account your suggestions above.

Fig. 2: Was the synchronization between DF01 and B32 done using the B32 volcanic sulfate or the B32 total sulfate records?

Response: The synchronization was done using the B32 volcanic sulfate, nssSO4.

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?

Response: We will flip the age axis in Figures 3, 5, and 6 in our revision as suggested.

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.

Response: Figure 4(b) will remain the same as it is, because Table 3 is correct if we believe the one-to-one relation between the B32 and EDML1 tie points given by Ruth et al. (2007), as explained above.

Fig 5: In addition to showing the age difference to Horiuchi et al., can you consider showing the 10Be data on your new timescale and the independent 14C 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 14C.

Response: We will show the 10Be flux data on our new timescales and the independent

14C production rate curve as an application in a new figure in the next revision as suggested.

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.

Response: Figure 6 will remain the same as it is, because Table 3 is correct if we believe the one-to-one relation between the B32 and EDML1 tie points given by Ruth et al. (2007), as described above. We will comment, however, this problem in our manuscript in the next revision.

Interactive comment on The Cryosphere Discuss., 8, 769, 2014.