Review to: "Atmospheric ^{10}Be from Talos Dome (East Antarctic) ice core records geomagnetic dipole intensity from 170 to 270 ka BP"

Comments from reviewer are in black / answers to the comments are in blue / suggested modified sections are in orange, with the specific modifications in **bold**

Review 1:

The revised manuscript is significantly improved compared to the submitted version. Most of the suggestions have been implemented, and for a small number of suggestions regarding minor modifications, the authors provided arguments for keeping the original wording.

The manuscript is suitable for publication after addressing the minor issues listed below.

The authors could one more time check the text in the response letter, listed under 'revision' and the text in the revised manuscript (with track changes), because sometimes they do not correspond and the revised manuscript doesn't contain the changes listed in the letter. For example, the text in the response letter on the first page starting with "After removing the …" contains sentences in bold that are not in the manuscript.

We thank Sanja Panovska and Pablo Rivera Perez for their valuable feedbacks. We acknowledge the several mistakes we have made in the previous submission. We have paid more attention to correctly report changes proposed in the letter into the manuscript.

Revision:

L299: "**4.3. ^{10}Be background flux**

After removing the **^{10}Be minima associated with major ion maxima**, rolling averages can be calculated to smooth the record and obtain the first-order variations, which are likely to result. Test 1 ka, 3ka and 5 ka rolling averages (Figure S1) illustrate the trade-off between noise reduction and signal preservation. Given the mean resolution of our record (300 a) we selected a 3 ka rolling average as a practical compromise. This choice provides stable background trends while preserving the amplitude of GDM-related variations. The resulting 3-ka averaged ^{10}Be flux record can be compared to geomagnetic reconstructions, including the Dome Fuji ice core data (Figure 4, Horiuchi et al., 2016), authigenic ^{10}Be/^{9}Be records from marine sediment cores (Figure 5, Simon et al., 2016) and ^{10}Be production (Figure 5, Poluianov et al., 2016) calculated from RPI-based VADM (Channell et al., 2009). Flux enhancement during geomagnetic excursions or events depends strongly on the choice of background taken as reference. **When discussing paleomagnetism, the background should be defined as the ^{10}Be profile without minima.** Here, we consider either the full 170–270 ka BP interval ^{10}Be flux mean average ($(1.36 \pm 0.26) \times 10^5$ at cm^{-2} a^{-1}) or a fixed baseline of 1.1×10^5 at cm^{-2} a^{-1} as **background**. The latter could be considered as representative of the long-term background ^{10}Be flux, which is close to the minimum values the 3 ka rolling average around 178 ka BP and 223 ka BP. Depending on the chosen baseline, the flux enhancement factors during specific events are as follows: for the 190 ka BP event (IBE), the ^{10}Be flux is 1.59 or 2.08 times the background; for the 205–215 ka BP event (PFE), the increase is 1.24 or 1.62 on average; and for the 240 ka BP event (ME), the peak flux reaches 1.25 or 1.63 times the reference value. **It is evident that defining a clear background value would have been preferable; however, this was not possible due to the limitations inherent in our profile and our**

understanding of the polar bias. We therefore advocate the necessity of having this dual scenario, mean or fixed values, with further work being necessary to refine this question.”

L273: we have modified figure 2.

In general, the addition of a separate subsection of the background flux contributes to the clarification of the different enhancement factors.

The use of the terms: background, baseline, reference, are all these the same?

For example, the sentence on line 320:

“Depending on the chosen baseline, the flux enhancement factors during specific events are as follows: for the 190 ka BP event (IBE), the 10Be flux is 1.59 or 2.08 times the background; for the 205–215 ka BP event (PFE), the increase is 1.24 or 1.62 on average; and for the 240 ka BP event (ME), the peak flux reaches 1.25 or 1.63 times the reference value.”

What does it mean: ‘baseline’,‘times the background’, ‘on average’ and ‘times the reference value’? Are the reported factors for PFE an average over the period, and the peak flux is 1.25 is reported for ME? These enhancement factors should have the same meaning and always be expressed with respect to the two backgrounds listed before in the text. Thus, is the following simplified sentence correct: “Depending on the chosen baseline, the flux enhancement factors during specific events are as follows: 1.59 or 2.08 for the 190 ka BP event (IBE); 1.24 or 1.62 for the 205–215 ka BP event (PFE); and 1.25 or 1.63 for the 240 ka BP event (ME).”

We thank the reviewers for raising this point. We have now corrected all mentions to simply refer to background. We also clarified the sentence L312, as suggested by the reviewers.

The number of the identified 10Be minima has been changed from 52 to 40, but 52 is still listed in the abstract and the conclusion sections.

We are sorry for this mistake and have now corrected it.

Including Table S1 that lists studies reporting the geomagnetic excursions in the Supplementary material is much appreciated.

We agree with the reviewers. We have revised this Table as suggested by Reviewer 2.

The response letter lists two revisions of the caption of Figure 1 (slightly different, on page 2 and 17). The one in the revised manuscript contains the ME excursion, but ME is not reported in Table S1.

We acknowledge a mistake in the letter, not updating the caption in Page 2. Indeed, the caption mentions the Mamaku Excursion, to clearly show two small intensity events are recorded. However, we realise that our sentence is misleading when compared to Table S1. Our initial Table S1 reflects literature as PFE and ME excursions are most of the time mixed up, because of sample resolution, chronology uncertainty, and their temporal proximity. We now explicitely brought this point to the attention of the read:

Revision:

“Table S1: list of studies reporting at the IBE, PFE alone, and IBE, PFE, and ME. The confusion between PFE and ME events are clarified in section 2.2.”

We also added ME in the Table S1.

The good overall agreement with the model of Lanci et al., 2008 is difficult to judge, as it is not shown. In that case, better to remove this part of the sentence.

We removed the sentence accordingly.

If the ratio is unitless, then does at at^-1 cancel out and it is not written, or it is written anyway?

In the community working on cosmogenic nuclides, it is common to report “at at^{-1}” units. We agree that the ratio is unitless, we have removed the at at^{-1} unit for clarity.

Line 252, reference to Fig 5 was replaced to Fig 1, but the sentence doesn’t refer to Fig 1. Please remove.

We are sorry for the mistake which led to a misunderstanding. The correct figure was Figure 2, in which we show the uncertainty of ^{10}Be flux.

Eq 1, as written, is not expressed in (%), right? Because the accumulation units cancels out and the factor is 0.2 (from the 20% uncertainty, as explained)

We thank the reviewer for raising, we have revised Eq.1 to clarify it is relative uncertainty.

Fig 2 in the response letter is updated, but not in the manuscript.

We thank the reviewer for raising this mistake. We have now corrected this.

Typo on line 327: macima => maxima

This was corrected in the manuscript. We are sorry for this mistake

Sanja Panovska and Pablo Rivera Perez

Reviewer 2:

Comments from round 1 are in black / answers to round 1 in blue / comments from reviewer from round 2 are in grey / answers are in green / suggested modified sections are in orange, with the specific modifications in **bold**

1. Earlier studies' findings are disregarded

Comprehensive research on the IBE period (170–200 ka BP) has already been conducted by Horiuchi et al. (2016). They presented unprecedented, high-resolution ^{10}Be data from the Antarctic Dome Fuji (DF) ice core and western equatorial Pacific sediments. They also discovered the following: (i) a 7-kyr plateau of the ^{10}Be maximum at the IBE, (ii) a twofold enhancement in ^{10}Be production (i.e. cosmic ray intensity), (iii) an asymmetric pattern of the ^{10}Be peak that is opposite to that of geomagnetic reversals, and (iv) an apparent age offset of several kyr between the ice core and the marine sediments, mainly due to uncertainty in the chronology of the sediments. I found that all of these findings are confirmed using independent data sets by Lamothe et al. in this preprint. This is truly wonderful. However, this preprint does not refer to the earlier findings. It should properly indicate what is known from the earlier research and what new findings were obtained in this study.

We thank the reviewer for this comment. We agree that our results are in excellent agreement with Horiuchi et al., 2016, which therefore would support more mentions of Horiuchi et al., 2016. We also notice that while many of the results discussed in our work are discussed in Horiuchi et al., 2016 dataset, we propose new elements like the twofold enhancement with respect to polar bias, the influence of marine age model uncertainties on the 3 ka delay, or the different asymmetric patterns between excursions and inversions. We have corrected our manuscript to better show these elements and what was already present and discussed in Horiuchi et al., 2016.

I would like to thank the authors for appropriately incorporating most of my comments into the revised manuscript. However, I must point out that Horiuchi et al. (2016) do not attribute the age offset between the ice core and the sediments to the magnetic lock-in depth. The discussion of the lock-in depth, performed in Section 4.1 of Horiuchi et al. (2016), only addresses the offset between the RPI and the 10Be/9Be ratio of the sedimentary KR cores. Then, the age offsets between the ice core and sediment records were synchronized using a simple cross-correlation procedure that assumed differences in their original chronologies (see Fig. 5 and Sec. 4.3). Although the DFO-2006 chronology was adopted for stacking all records (Sec. 4.4) because it was the only one based on direct orbital tuning, the discussion does not address whether the difference is primarily caused by uncertainty in the chronology of the sediments or the ice core. I think a new and excellent element of this manuscript (Lamothe et al.) is specifying the main cause of the age offset as the uncertainty of sediment chronology. In any case, descriptions such as "interpreted it primarily in terms of magnetic lock-in depth associated with postdepositional remanent magnetization acquisition in marine sediments." are misleading. Those should be either deleted or corrected appropriately (Lines 580–587: “and interpreted it primarily in terms of magnetic lock-in depth associated with postdepositional remanent magnetization acquisition in marine sediments”; “but extend the comparison to ice core 10Be fluxes, which are not affected by magnetization processes”; and “Unlike comparisons involving RPI, a phase shift between ice core and oceanic 10Be records cannot be attributed to magnetic lock-in effects.”).

We thank the reviewer for this important clarification. We agree that our previous wording was misleading. Horiuchi et al. (2016) do not attribute the age offset between ice-core and sedimentary records primarily to magnetic lock-in depth. Their discussion of lock-in depth is restricted to offsets between RPI and ^{10}Be within sediment cores, while the synchronization between sediment and ice-core records is performed using a cross-correlation approach without explicitly identifying the dominant source of the offset. We have therefore revised the manuscript to remove this ambiguity and avoid attributing this interpretation to Horiuchi et al. (2016). In contrast, we emphasize that one of the novel contributions of our study is to further investigate the origin of this offset and to show that it is most likely dominated by uncertainties in sediment chronologies rather than magnetization processes.

Revision:

L555: “A similar 3 to 4.5 ka offset between oceanic and Dome F ice core records was previously reported by Horiuchi et al., (2016), **without explicitly identifying the dominant cause of the age offset.** When comparing authigenic ^{10}Be/^{9}Be from core MD05-2920 to the TALDICE ^{10}Be flux, the best correlation is obtained when the oceanic record is shifted 3 ka younger ($R^2 = 0.37$, Figure S4), which remains within the uncertainties of the marine core age model (Tachikawa et al., 2014). **Because this comparison relies solely on ^{10}Be-based proxies, it is not directly affected by magnetic lock-in processes that influence RPI records.** Potential physical causes would instead involve atmospheric or oceanic transport and mixing processes. Nevertheless, does this 3-ka offset result from age model uncertainties or reflect a physical lag in the system, thereby limiting the possibility to use paleomagnetic events as chronostratigraphic horizons?”

Lines 162–163. Why is there a sampling gap between 1499 and 1505 m? Please clarify.

We did not sample this section. We clarified this in the manuscript.

My question is, "Why did the authors not take samples from this interval?" Are there no ice cores or samples available for the 10Be analysis, or are there other reasons why none were taken? Please clarify.

The interval between 1499 and 1505 m was not sampled as part of a targeted sampling strategy focusing on depth intervals corresponding to the expected timing of the IBE, PFE and ME excursions, based on existing age models and marine sediment records. The absence of samples in this interval does not reflect any limitation in the availability or quality of the ice core.

Revision:

L166: “The ice core was continuously sampled in sections of 20 cm (when possible) between 1470 m and 1499 m and between 1505 m and 1531 m. **The interval between 1499 and 1505 m was not sampled as part of a targeted sampling strategy focusing on depth intervals corresponding to the expected timing of the IBE, PFE and ME excursions, based on existing age models and marine sediment records.**”

Lines 239–241, and 280. If the authors used objective statistical criteria, why did they focus only on low values and not high ones? If the reason is related to concurrent peaks of certain ions, why were low ^{10}Be values unrelated to those peaks excluded from the final rolling average? Please clarify.

We thank the reviewer for this comment and for the opportunity to clarify this point. Although objective statistical criteria were used to identify anomalous values, the analysis focused primarily on low ^{10}Be values because they represent the dominant and recurrent feature affecting the rolling-average signal. Sharp and isolated minima are frequent in the ^{10}Be concentration record and can significantly distort the rolling average, particularly when they are related to post-depositional processes rather than to changes in ^{10}Be production.

In contrast, high ^{10}Be values do not display the same behavior. Apart from a single isolated high value at 1473.2 m, very few maxima satisfy the same statistical criteria applied to minima. Applying an equivalent detection procedure to high values identifies only one additional case to 1473.2 m, at 1521.2 m, which corresponds to a broader, multi-sample Gaussian-shaped feature rather than an isolated outlier. Such smooth maxima are unlikely to result from contamination or depositional artifacts and therefore do not significantly bias the rolling average.

For this reason, the analysis focused on identifying and evaluating the origin of low ^{10}Be values. Low values that were not associated with concurrent peaks in major ion concentrations were excluded from the final rolling average because they are interpreted as non-climatic artifacts that disproportionately affect the smoothed signal. In contrast, maxima were retained, as they represent features for which no clear mechanisms can support to discard them.

I don't completely agree with this argument — I still think it would be better to delete only the 10Be minima that occurs with the ion maxima — but I understand the authors' reasoning. With this in mind, I recommend adding the explanation written above rather than the simple description in the revision. A detailed description of the results of the empirical examination would be helpful for future work on the "oldest" ice core projects.

We thank the reviewer for this constructive suggestion. We realise there was a misunderstanding in our previous reply as we understood the reviewer was asking to remove 10Be maxima (‘not the high ones’). We now clearly understand, and agree, that 10Be minima removal should only be done when concomitant maxima are observed in major ions. Following this recommendation, we have revised the 10Be flux, the Figures, and the text.

The results and interpretations remain largely unchanged, as the excluded minima represent only a small fraction of the data and do not significantly affect the reconstructed 10Be flux variations.

Revision:

L253: “To identify anomalously low ^{10}Be fluxes, which could bias the interpretation of geomagnetic intensity, we applied an objective statistical criterion. These ^{10}Be minima were identified when the concentration fell below the mean minus one standard deviation, both calculated using a 3-ka rolling window. **The corrected ^{10}Be flux was calculated removing the ^{10}Be minima associated with a maximum in major ion concentrations.**”

L300: “After removing the **^{10}Be minima associated with major ion maxima**, rolling mean averages can be calculated to smooth the record and obtain the first-order variations, which are likely to result. Testing 1 ka, 3ka, and 5 ka rolling mean averages (Figure S1) illustrate the trade-off between noise reduction and signal preservation. Given the mean resolution of our record (300 a) we selected a 3-ka rolling mean average as a practical compromise. This choice provides stable background trends while preserving the amplitude of GDM-related variations. The resulting 3-ka averaged ^{10}Be flux record can be compared to geomagnetic reconstructions, including the Dome Fuji ice core data (Horiuchi et al., 2016) in Figure 4, and authigenic ^{10}Be/^{9}Be records from marine sediment cores (Simon et al., 2016) and the ^{10}Be production (Poluianov et al., 2016) calculated from RPI-based VADM (Channell et al., 2009) in Figure 5. Flux enhancement during geomagnetic excursions or events depends strongly on the choice of **the** background **value**. **When discussing paleomagnetism, the background should be defined as the ^{10}Be profile without minima.** Here, we consider either the full 170–270 ka BP interval ^{10}Be flux mean average**, after removing ^{10}Be minima (See section 5.1) but includes the geomagnetic events,** of (1.**44** ± 0.26) × 10^5 at cm^{-2} a^{-1} or a fixed **value** of 1.1 × 10^5 at cm^{-2} a^{-1} as **background**. The latter could be considered as representative of the long-term background ^{10}Be flux, which is close to the minimum values the 3 ka rolling mean average around 178 ka BP and 223 ka BP. Depending on the chosen background, the flux enhancement factors during specific events are as follows: **1.55 or 2.02 for the 190 ka BP event (IBE); 1.23 or 1.62 for the 205–215 ka BP event (PFE); and 1.17 or 1.53 for the 240 ka BP event (ME).** As a general idea, the flux enhancement factor for IBE represents 1.**4** times the modern value of ≈1.64 10^5 at cm^{-2} a^{-1} at Dome C (Jouzel et al., 2026). **It is evident that defining a clear background value would have been preferable; however, this was not possible due to the limitations inherent in our profile and our understanding of the polar bias. We therefore advocate the necessity of having this dual scenario, mean or fixed values, with further work being necessary to refine this question**."

L431: "The Iceland Basin Excursion (IBE), around 190 ka BP, stands out as one of the most prominent geomagnetic events during the Bruhnes chron (Simon et al., 2016). Based on the TALDICE ^{10}Be flux data, a flux enhancement factor of 1.**55** to 2.**02** is observed, depending on the **chosen** background (see section 4. Results), similar to the twofold enhancement reported in Dome F (Horiuchi et al., 2016)."

L452: "Accordingly, the observed twofold (2.**02**) flux increase at TALDICE would imply a complete depletion of the geomagnetic dipole moment, consistent with VADM reconstructions."

L500: "In addition to the IBE, the TALDICE ^{10}Be record supports the existence of two distinct geomagnetic events for the period 205-270 ka BP (Figure 5). The identification of lower-amplitude geomagnetic excursions in this interval requires a cautious approach (see section 2.2 and Channell et al., 2020). Unlike more prominent events, these excursions often lack clear and well-dated signatures. The first event, occurring between (206.0 ± 0.8) ka BP and (218.5 ± 1.9) ka BP, shows a flux enhancement factor of 1.**23** or 1.**62**, depending on the **background**, and is here associated with the Pringle Falls Excursion (PFE). The second, centred at (242.0 ± 0.3) ka BP, shows a peak enhancement factor of 1.**17** or 1.**53**, and is tentatively identified as the Mamaku Excursion (ME)."

L600: "Because of this broad global agreement, a comparison of ^{10}Be production calculated from RPI-based VADM and authigenic ^{10}Be/^{9}Be with ice core ^{10}Be fluxes is possible. Using the VADM reconstruction PISO-1500 (Channell et al., 2009) and assuming a constant solar modulation potential of 650 MV, ^{10}Be production was calculated following Poluianov et al. (2016). Between 170 and 270 ka BP, ^{10}Be production was at a mean of 10.5×10^5 at cm^{-2} a^{-1} (min = 7.4 ; max = 15.9). Compared with the 1.**44** × 10^5 at cm^{-2} a^{-1} mean flux in TALDICE (removing ^{10}Be minima), this reveals a mean scaling factor of **7.3 (min = 5.1; max = 11.0**). This scaling factor reflects the combined influence of geographic production patterns, including hemispheric asymmetries in production (Panovska et al., 2023) and atmospheric transport and deposition (Delaygue and Bard, 2011; Golubenko et al., 2024; Heikkilä et al., 2009). Consequently, quantitative interpretation of these ratios remains difficult, which highlights the need for continued model/data integration to constrain transport and deposition processes."

L624: "The Iceland Basin Excursion (IBE) is well recorded, with a flux enhancement factor of **1.55–2.02** and a clearly defined low-dipole field interval between (192.0 ± 1.4) ka BP and (185.6 ± 1.4) ka BP."

L632: "In addition, the TALDICE ^{10}Be record captures two lower-amplitude geomagnetic excursions: a long-lasting Pringle Falls Excursion (PFE), from (218.5 ± 1.9) to (206.0 ± 0.8) ka BP, and a brief Mamaku Excursion (ME) at (242.0 ± 0.3) ka BP, associated with flux enhancement factors of **1.23–1.62 and 1.17–1.53**, respectively."

Figure S1, 4 and 5 have been revised, although this cannot be observed as the changes in fluxes are negligible.

Figure S1:

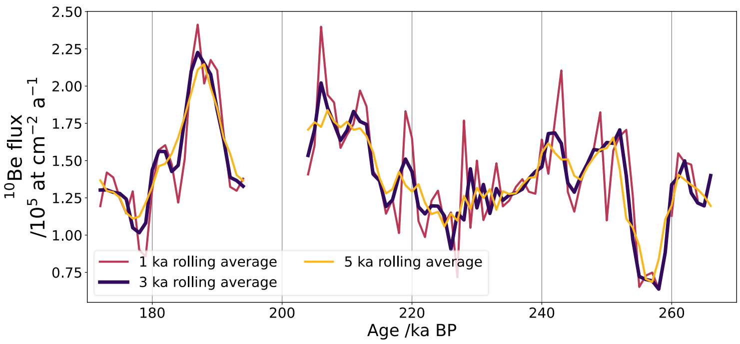


Figure 4:

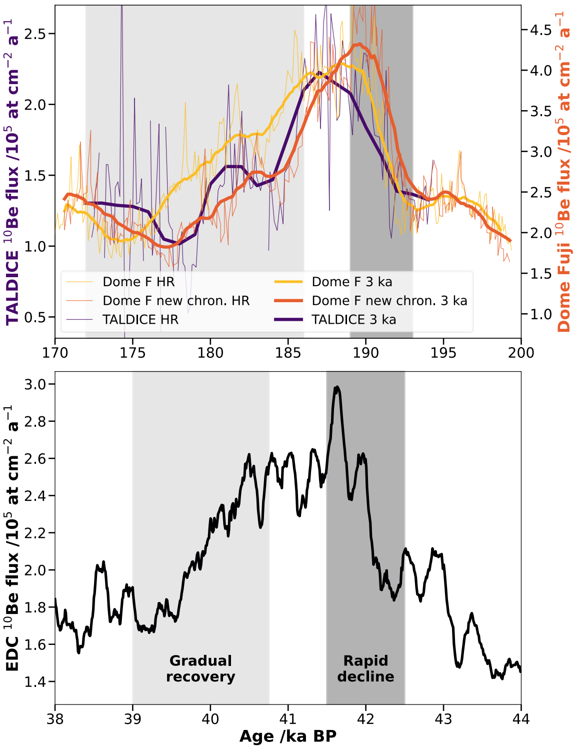
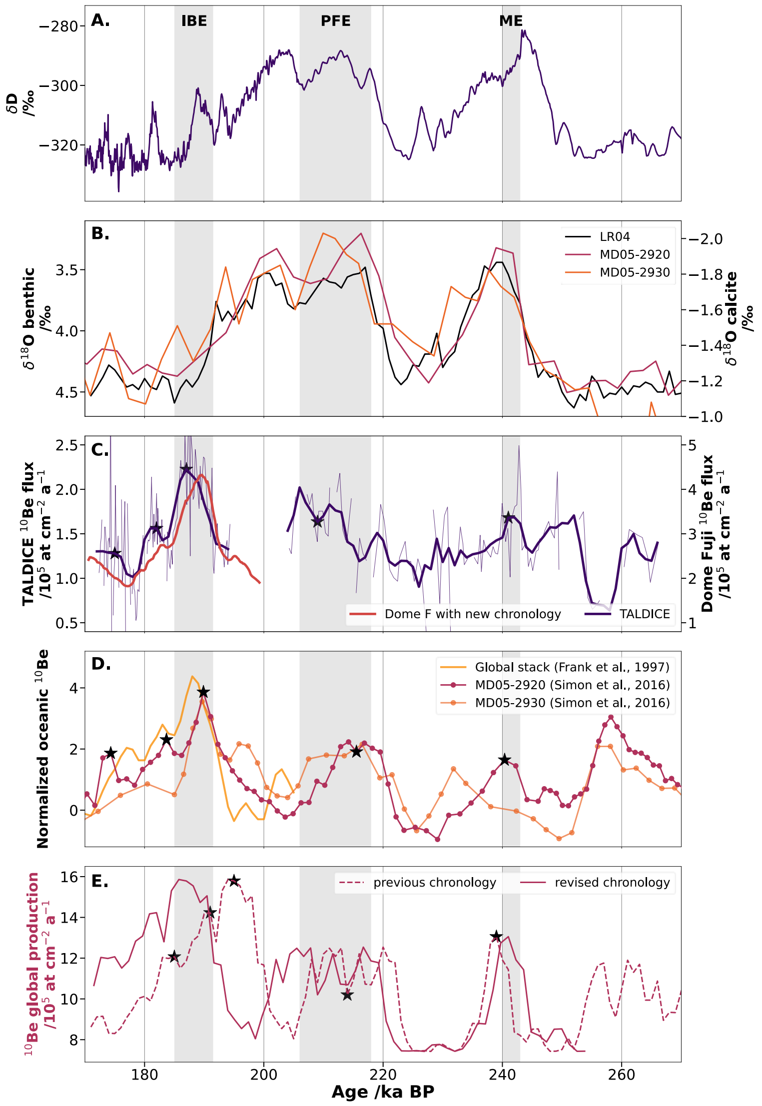


Figure 5:



Lines 264–265. Please describe the details about the statistical analysis in either the Materials and Methods or the Supplemental Information.

We thank the reviewer for this comment. The statistical tests applied in this section are standard non-parametric methods commonly used to compare distributions and proportions in paleoclimate studies. While the paper is not focused on statistical methodology, we agree that the rationale for using these tests should be made more explicit. We have therefore clarified in the manuscript how each test was applied and what hypothesis it was designed to evaluate. Additional details on the statistical approach are now provided in a new sub-section of the methodology.

My question was only for lines 264–265, that is "This association is statistically significant (permutation test's p-value = 0.0001)". Specifically, what variables were included in the permutation test? Was a binary table (or contingency table?) made with the minima of 10Be and the maxima of major ions? If so, how many major ion maxima did the authors identify within the interval of interest (1470-1499 and 1505-1531)? Was other permutation test conducted, such as one based on the rank of the concentrations of 10Be and ion? I understand the other tests described in the manuscript, such as the Mann-Whitney U test, because the text provided some information about them. However, I am unclear about only the permutation test because I have little information about the procedure.

We thank the reviewer for pointing out the lack of detail regarding the permutation test. The test was performed on binary event series, where each depth point was assigned a value of 1 if a ^{10}Be minimum (or ion maximum) was identified, and 0 otherwise. The test statistic corresponds to the number of coincident events between the two series. Therefore, we did not test the concentration value. To evaluate the significance of this synchrony, we compare the total number of measurement (259), with the number of ^{10}Be minima (40), the number of major ion maxima (23), and the number of coincident events (16), for 10,000 iterations. The p-value is calculated as the proportion of permutations yielding a number of coincidences equal to or greater than the observed value. This approach tests whether the observed synchrony of ion maxima/^{10}Be minima exceeds random expectations, without making assumptions about the underlying distributions. It is therefore distinct from rank-based tests (such as Mann–Whitney U) and is conceptually equivalent to a randomization test applied to a contingency structure. We clarified this in the manuscript. However, we realized we made a mistake as the p-value is not = 0.0001, but is < 0.0001.

Revision:

L281: "A total of 40 minima in ^{10}Be concentration were identified across the TALDICE record studied in this work. Because minima are defined at the ^{10}Be sampling resolution, a low-concentration interval extending over 40 cm is counted as two distinct minima. The ^{10}Be minima appear to coincide with maxima in the concentrations of major ions **(16 coincidences out of the 23 maxima observed in major ions**, Figure 3). This association is statistically significant (permutation test's *p*-value **<** 0.0001), though no direct quantitative relationship can be established."

Figs. 4 and 5. In these figures, the ^{10}Be flux profile of the DF ice core appears to have been smoothed using a 5-point rolling average or a similar procedure. I recommend showing the data at its original resolution. In any case, the authors should clearly indicate in the captions and text that the profile is smoothed.

We thank the reviewer for this comment. The Dome Fuji ^{10}Be flux record shown in Figures 4 and 5 is indeed smoothed using a 3 ka rolling average to be consistent with other 3 ka-averaged records, improve readability, and facilitate comparison with other records. We have now explicitly stated the smoothing procedure in the figure captions. We now also show the high-resolution DF ^{10}Be flux in Figure 4.

Last time, I made two suggestions: 1) the DF 10Be data should be shown at its original time (≈100 yr) resolution, and 2) if a smoothed curve is presented, it should be appropriately described in the figure caption and text. Because the post depositional alteration is not observed in the DF record, smoothing is unnecessary or even misleading. In particular, the revised Figure 4 compares the IBE records with the ice core record of 10Be from the Laschamp excursion with a resolution of some hundred years. This is an excellent attempt, and a close inspection of the detailed profiles would help to clarify the similarities between the two excursions representative of the Brunhes epoch. Therefore, I recommend that the authors do not degrade the information by smoothing the DF record in this figure.

Regarding Fig. 5, it may not be necessary to display the DF data at its original time resolution, given that the time resolutions of the other cosmogenic and paleomagnetic records are one order of magnitude lower. Nevertheless, the 1-ka rolling average is preferable for the DF record because the PISO-1500 and the 10Be flux stack compiled by Frank et al. (1998) have a resolution of 1 ka. This will show more similarities than the 3 kyr rolling average data and support the authors' argument in this manuscript.

We thank the reviewer for this detailed comment and for emphasizing the high-resolution quality of the Dome Fuji record. We agree that the original resolution of the DF record contains valuable information and, as suggested in the previous round, we have included the high-resolution record in Figure 4.

However, the primary objective of Figures 4 and 5 is not to highlight the internal variability of the DF record, but to compare ^{10}Be variations across multiple archives (ice cores alone (Fig 4) and ice cores and marine sediments (Fig 5)). All these records have different temporal resolutions and chronological uncertainties. In this context, smoothing the DF record using a 3 ka rolling average similar to TALDICE ensures solid comparison, especially when compared to age uncertainties (about 2 ka). This avoids over-interpreting centennial-scale variability that cannot be robustly aligned between archives.

Presenting the DF record at its original resolution alone would therefore create an imbalance in the comparison and could be misleading, as it would suggest a level of synchronicity that is not supported by the chronological constraints. For this reason, we maintain the 3 ka smoothing for intercomparison purposes, while also providing the high-resolution data in Figure 4 to preserve the full information content of the DF record.

Regarding Figure 5, we acknowledge the reviewer's suggestion of a 1 ka smoothing. However, similar to the comparison with TALDICE (Fig 4), given the resolution and uncertainties of the marine records, we consider that a 3 ka smoothing remains more appropriate to ensure consistency across datasets.

We did not make any revision.

Lines 236–238. Correcting the previously published ^{10}Be flux data based on the most recent DF chronology (DF2021) is an excellent attempt. However, this preprint incorrectly uses the DF2 depth for SAR estimation (I verified this by recalculating the updated 10Be flux myself.). Since the DF2021 chronology is associated with the DF1 ice core, the equivalent DF1 depth (see Horiuchi et al.'s (2016) supplementary data file) must be used instead of the DF2 depth. Additionally, it appears that the previous chronology (DFO-2006) is still being used for the age model in this preprint (Figs. 4 and 5). To maintain consistency, I recommend that the authors use the DF2021 chronology for the age model of the corrected ^{10}Be flux of DF. As a result, the r-squared values shown in lines 355–356 (and the relevant discussion?) will change.

Lines 363–365. The ^{10}Be record from the DF ice core over the last millennium (Horiuchi et al., 2008) was normalized using the previous nominal value of the ICN ^{10}Be standard. Additionally, the ^{10}Be flux was calculated using an earlier SAR estimation based on the simple empirical relationship between SAR and d^{18}O in surface snowpacks (Satow et al., 1999) (for more details, see Horiuchi et al., 2008). Then, the average of the last millennium's ^{10}Be flux was updated in Horiuchi et al. (2016) using a revised standard value and the formulation of Parrenin et al. (2007) (i.e. using the same methodology as the published ^{10}Be record for the IBE) (see the Supplementary Material of Horiuchi et al. (2016)). Although it is still just about 1.3 times higher, the updated value of 2.07×10^5 at cm^{-2} a^{-1} should be compared to the EDC value.

Lines 368–370. As mentioned above, the DF ^{10}Be flux is not twice as high as the EDC ones, but rather, just 1.3 times higher for the last millennium. Therefore, the difference of two times between the DF and TALDICE is not persistent, but has been observed (so far) only during the IBE. Although this seems enigmatic, I agree with the authors that data from other cores is necessary to resolve this issue.

We thank the reviewer for this detailed and very helpful comment. We acknowledge that, in the previous version of the manuscript, the description of the Dome Fuji ^{10}Be flux recalculation was not sufficiently clear and may have led to confusion. We confirm that the revised flux has now been recalculated using the DF1 depth scale consistently associated with the DF2021 chronology (Oyabu et al., 2022). The resulting flux have been corrected accordingly.

We also revised Figures 4 and 5 using the recalculated Dome Fuji flux based on the DF2021 age model. The revised comparison yields an improved agreement between TALDICE and Dome Fuji when using the DF2021 chronology ($R^2 = 0.44$) compared to the older DFO-2006 chronology ($R^2 = 0.37$). The relevant text has been updated accordingly.

Regarding the comparison with last-millennium values, we have substantially modified this discussion. We note that a recent independent compilation combining measurements and atmospheric modelling (Jouzel et al., 2026) confirms that Dome Fuji exhibits systematically higher ^{10}Be fluxes than EPICA Dome C (by 67 %) over the last millennium, supporting the conclusion that the Dome Fuji enhancement is not an artefact of accumulation or standardisation choices. We therefore clarify that the larger Dome Fuji / TALDICE contrast observed during the IBE is specific to that interval, but occurs within a broader context of persistent inter-site differences across East Antarctica. We also now discuss this difference in relationship with climate variations during MIS7, and show that this difference does not differ between glacial and inter-glacial conditions.

Finally, we emphasize that this systematic offset does not affect the interpretation of geomagnetic dipole moment variations, which relies exclusively on relative changes in ^{10}Be flux within each archive.

I thank the authors for their appropriate correction of the DF 10Be flux profile. I also understand the arguments in the revised manuscript regarding the comparison to the value from the last millennium. However, I still believe it is preferable to use the updated value of 2.07×10^5 at cm–2 a–1 provided by Horiuchi et al. (2016) for long-term comparisons to maintain consistency in SAR estimation. On the other hand, the SAR estimated using the empirical relationship (Satow et al., 1999) cannot be said to be inaccurate for the most recent period. Moreover, the original 10Be flux values published in Horiuchi et al. (2008) are used in discussions of comprehensive research such as Jouzel et al. (2026). To reconcile this discrepancy, I suggest using the data in Table 2 of Jouzel et al. (2026). The value 88.56 at m–2 s–1 (2.79×10^5 at cm–2 a–1) has been corrected for the old standard value, at least. As a result, the "the last millennium" interval would be from 237 to 985 yr BP.

We thank the reviewer for this clarification. We agree that consistency in the reference values used for the last millennium is important. Following the reviewer’s suggestion, we now use the value reported in Jouzel et al. (2026). We made the following revision.

Revision:

L398: “This pattern persists over the last millennium, between **965** and **1713** CE, the mean ^{10}Be fluxes were **1.69×10^5 at cm^{-2} a^{-1} and 2.79×10^5 at cm^{-2} a^{-1} at Dome C and Dome F, respectively (Jouzel et al., 2026). These values may be the results of the occurrence of solar minima during this interval corresponding to enhanced ^{10}Be production (Wolf, 1280 – 1350 CE; Spörer, 1420 – 1570 CE; Maunder 1645 – 1715 CE; Dalton 1790 – 1830 CE, Bard et al., 2000; Berggren et al., 2009; Horiuchi et al., 2008).”**

Lines 485–486. What is the authors' opinion on the clear maximum observed around 232 ka in the MD05-2930 record? Please clarify.

We thank the reviewer for drawing attention to the specific pattern around ca. 232 ka BP in the MD05-2930 record. We agree that this feature is noticeable. However, it is not observed consistently in other marine records nor in the ice core ^{10}Be fluxes, which prevents a robust attribution at this stage.

Several explanations should be considered, including uncertainties in the marine age model, which could potentially shift this feature toward the age of the Mamaku Excursion, although such a reinterpretation would affect the δ^{18}O-based alignment to the LR04 stack. Alternatively, this maximum may reflect local depositional or sedimentary processes, transient perturbations of the authigenic ^{10}Be signal, or changes in sediment circulation or scavenging efficiency.

Given the absence of corroborating evidence from independent archives, we consider this feature as tentative and do not interpret it further. Additional high-resolution marine records would be required to assess its origin and potential geomagnetic significance.

I understand that the authors believe the maximum at ca. 232 ka BP in the MD05-2930 record is unlikely to correspond to the Mamaku Excursion because shifting the age from 232 ka BP to 242 ka BP would affect the δ18O-based alignment of the MD05-2930 core to the LR04 stack (Tachikawa et al., 2014). Yes, this consideration would be reasonable.

However, if so, how do the authors think about the PISO-1500? Its age model is also based on the oxygen isotope chronology with the LR04 stack. Therefore, the PISO-1500's shift of about 10 kyr (half of the precession cycle) between 170 and 200 ka (Fig. 5E) must also affect its alignment to the LR04 stack. Is this acceptable?

I think that this is an important point for this work to pass the review process. The PISO-1500 record includes its original d18O stack, accompanied by the paleointensity stack. The authors should plot the δ18O stack against the revised chronology in Fig. 4B to verify its alignment with the LR04 stack. In any case, a proper discussion on the consistency of the revised chronology for the PISO-1500 with the δ18O chronostratigraphy seems necessary.

We thank the reviewer for this important and insightful comment. We agree that the apparent discrepancy between the ^{10}Be maximum observed around 232 ka BP in MD05-2930 and the expected timing of the Mamaku Excursion raises questions regarding the consistency of age models based on δ^{18}O alignment to the LR04 stack.

Following the reviewer's comment, we have revised Figure 5 to include the δ^{18}O record from MD05-2930. This comparison shows that the overall alignment of the core with the LR04 stack remains robust. However, the ^{10}Be maximum at 232 ka coincides with a local feature in the δ^{18}O record.

While we cannot fully exclude age uncertainties, this observation also questions the possibility of local oceanographic processes affecting both authigenic ^{10}Be deposition and δ^{18}O signals, such as changes in circulation, sedimentation dynamics or bioturbation.

Regarding the comparison with the PISO-1500 stack, we acknowledge that similar chronological challenges may arise when aligning different archives to a common δ^{18}O reference framework. However, the overall agreement between independent records at the multi-millennial scale suggests that such discrepancies remain limited and do not affect the main conclusions of this study. Ultimately, this highlights the need for cautious interpretation of isolated features in single marine records, as well as overly relying on stacks as chronological constraints.

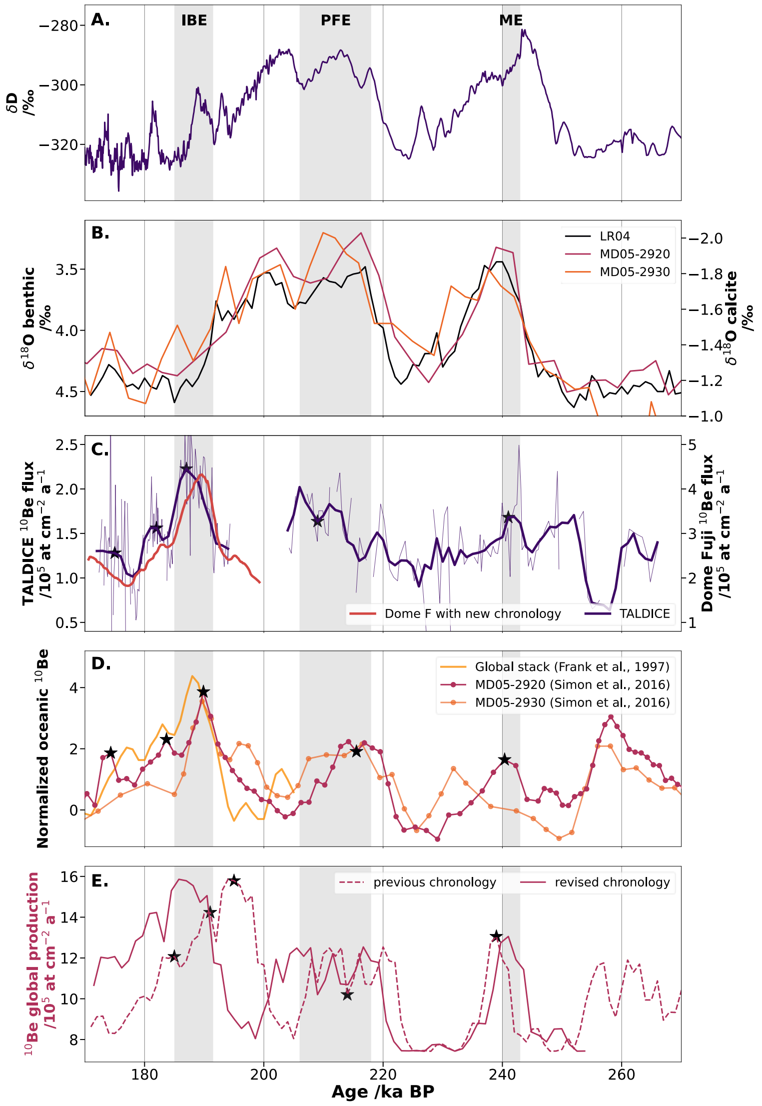
We have added a discussion of these points in the revised manuscript to clarify the interpretation and limitations associated with this feature.

Revision:

L589 : "**A distinct ^{10}Be maximum is observed at ≈232 ka in the MD05-2930 record, for which no clear counterpart is identified in other archives. To further investigate this feature, we examined the δ^{18}O record of planktonic calcite from the same core (Regoli et al., 2015), using its recently revised chronology (Bowman et al., 2024). Interestingly, the ^{10}Be maximum coincides with a local δ^{18}O peak. This raises two possible interpretations: either an incorrect identification of the MIS 7.5 interval in this portion of the record, or the influence of local or regional oceanographic processes affecting both authigenic ^{10}Be and δ^{18}O signals, such as changes in circulation, sedimentation dynamics, or**

bioturbation. This observation highlights that small-amplitude or isolated features in single marine records should be interpreted with caution and cannot be systematically integrated into global stacks without independent validation. Nevertheless, the overall agreement between ice core and marine records at multi-millennial timescales indicates that such discrepancies remain limited and do not affect the main conclusions of this study. More broadly, this emphasizes the need for cautious use of stacked records as chronological constraints, particularly when relying on ^{10}Be alone."

We have revised Figure 5 to include d18O calcite



Lines 290. I recommend adding the same note as in the caption of Fig. 3 just after "40 minima": (Because minima are defined at the 10Be sampling resolution, a low-concentration interval extending over 40 cm is counted as two distinct minima)

We added this as suggested.

Line 324, etc. In the strict sense, "c." (circa) should only be used for age or date. Replace it with "approx." or something similar.

We modified c. by ≈, when not referring to ages

Lines 402. Is it really 210%? I see it is about 170%.

We acknowledge a mistake and corrected accordingly.

Lines 416–418. I don't think this sentence makes sense anymore.

We have clarified this section.

L398: “This pattern persists over the last millennium, between **965** and **1713** CE, the mean ^{10}Be fluxes were **1.69 × 10^{5} at cm^{-2} a^{-1} and 2.79 × 10^{5} at cm^{-2} a^{-1} at Dome C and Dome F, respectively (Jouzel et al., 2026). These values may be the results of the occurrence of solar minima during this interval corresponding to enhanced ^{10}Be production (Wolf, 1280 – 1350 CE; Spörer, 1420 – 1570 CE; Maunder 1645 – 1715 CE; Dalton 1790 – 1830 CE, Bard et al., 2000; Berggren et al., 2009; Horiuchi et al., 2008).** Although no ^{10}Be measurements are available for the last millennium at Talos Dome, comparison of recent century data shows similar mean fluxes between Talos Dome (1.49 × 10^{5} at cm^{-2} a^{-1}, Supplementary Table 1) and Dome C (1.69 × 10^{5} at cm^{-2} a^{-1}, Jouzel et al., 2026). Independent evidence from the last millennium further indicates that Dome Fuji generally exhibits higher ^{10}Be fluxes than other East Antarctic sites. A recent multi-site compilation and modelling study (Jouzel et al., 2026) reports that measured ^{10}Be fluxes at Dome Fuji exceed those at EPICA Dome C by ≈70 %, consistent with earlier observations. These results suggest that persistent differences in ^{10}Be flux over Antarctica likely reflect regional **atmospheric and depositional processes,** rather than artefacts related to accumulation estimates **derived from age models or methodological biases**.”

Lines 421–424. I know that Jouzel et al. (2026) made a such discussion based (at least partly) on the earlier argument with 10Be data obtained in the 59th JARE traverse (Horiuchi et al., 2022). However, I don’t think they made a strong argument for it. So, I suggest replacing the sentence as follows (the changing points are in bold): Jouzel et al. (2026) further suggest that this persistent contrast **may** reflect regional atmospheric and depositional processes specific to the high-elevation interior of East Antarctica, including a transition from predominantly wet deposition north of 75°S to dry-dominated deposition south of this boundary **(Horiuchi et al., 2022),** as well as enhanced stratosphere-troposphere exchanges over the highest Antarctic domes.

We revised accordingly.

Lines 427–433. I think that this is an important contribution of this work!

We acknowledge that this should be raised in the conclusion.

Revision:

L639: “The strong agreement between TALDICE and marine authigenic ^{10}Be/^{9}Be records reinforces the potential of ^{10}Be as a robust tool for synchronizing marine and ice core archives. Our results reveal that the oceanic records (MD05-2920 and MD05-2930) precede the ice core by ca. 3 ka, a discrepancy attributable to uncertainties in the age model rather than a genuine phase shift. Crucially, no systematic differences were observed between glacial and interglacial intervals, which further validates the limited oceanic-atmospheric differences. **In addition, a ≈2 fold enhancement is observed by 10Be deposition in Dome Fuji relative to TALDICE, which may result from regional deposition differences. However, this difference is not modulated by climate variability, which suggests that atmospheric deposition likely remained unchanged over Antarctica between glacial and interglacial periods. Consequently, while absolute fluxes differ between sites, their temporal variations primarily record changes in cosmogenic production, reinforcing the use of Antarctic ice cores as robust archives for reconstructing geomagnetic dipole moment variations, after accounting for polar bias.**

Overall, this multi-archive consistency not only strengthens the fidelity of reconstructions of past geomagnetic dipole moment variations, but also offers a promising avenue for refining chronologies and exploring climate-magnetic field interactions during critical intervals such as the Mid-Pleistocene Transition."

Revised Fig. 4. Does the 10Be profile of the Laschamp interval represent the original Raisbeck et al. (2017) profile or a recalculated profile using the AICC2022 age model? Please clarify this. Additionally, the details of smoothing for the Laschamp profile should be described in the caption.

We thank the reviewer for raising this point. We have clarified this in the document.

Revision:

L426: "For comparison, the same periods can be identified during the Laschamps excursion in the ^{10}Be flux record from EPICA Dome C ice core (**19 point rolling average, ≈175 a,** Raisbeck et al., 2017**, recalculated on the AICC2023 chronology (Bouchet et al., 2023)**)"

Lines 476–479. Change the sentence as follows (the points are in bold): An asymmetric pattern, characterized by a rapid increase in 10Be flux associated with dipole collapse followed by a slow and three-step dipole moment recovery was already identified in the Dome Fuji ice core record **(Figure 4) and western equatorial Pacific sediment records** by Horiuchi et al., (2016).

We revised accordingly.

Lines 505–507. From this sentence, the readers expect to see the detailed 10Be records from both ice and sediment cores for the IBE in Figure 4. However, the revised Fig. 4 does not include sedimentary 10Be profiles. So, delete the words "as well as oceanic cores".

We thank the reviewer for this comment. We agree that the wording could be misleading with respect to the content of Figure 4. Our intention was not to imply that sedimentary ^{10}Be records are shown in this figure, but rather to highlight that similar asymmetric dynamics have also been reported in both ice core and sedimentary archives in previous studies. We have therefore revised the sentence to clarify this point and avoid any confusion.

Revision:

L457: "The structure of the ^{10}Be flux anomaly during IBE is also noteworthy. An asymmetric pattern, characterized by a rapid increase in ^{10}Be flux associated with dipole collapse followed by a slow and three-step dipole moment recovery was already identified in the Dome Fuji ice core record (Figure 4) **and western equatorial Pacific sediment records (Horiuchi et al., 2016)**. Similar asymmetric dynamics have also been reported for the Laschamps excursion in ^{10}Be records from both ice cores (Muscheler et al., 2005; Raisbeck et al., 2017, Figure 4) and**, independently, in** sediment cores (Ménabréaz et al., 2012; Simon et al., 2016, 2020). »

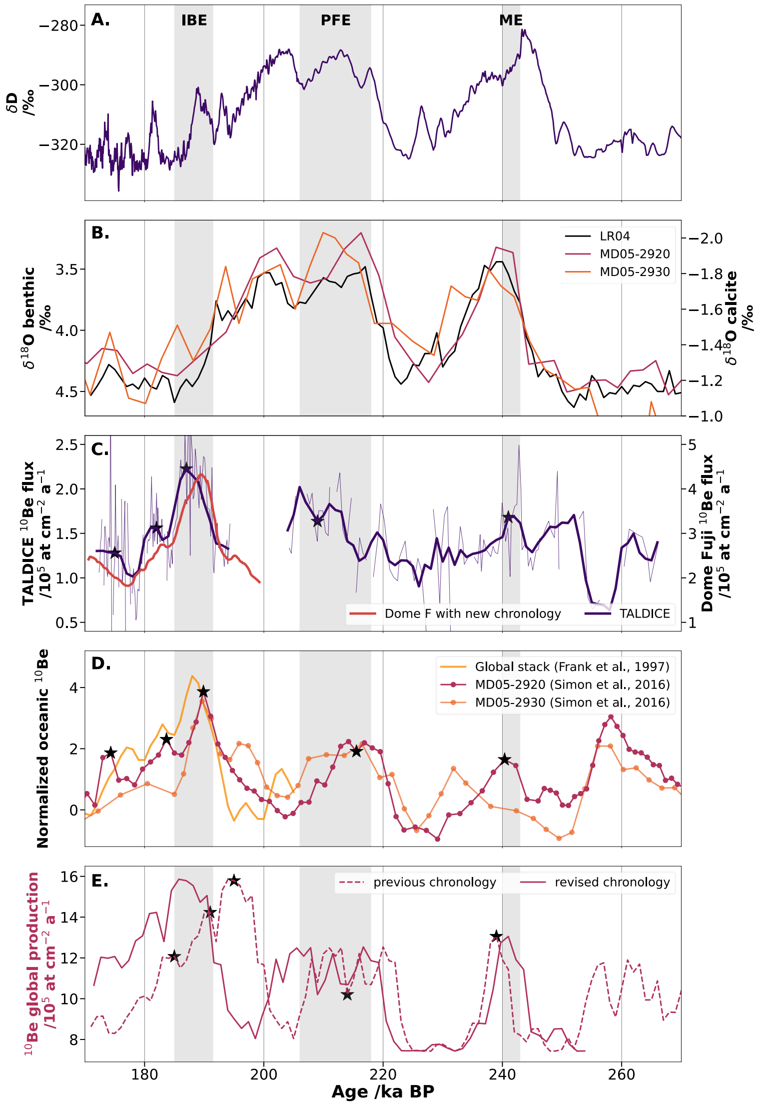
Lines 508–510. This sentence should be moved to Sec. 5.3.2. The present section (Sec. 5.3.1) is dedicated to writing about the IBE. Revised Fig. 5. I see that the black stars have been deleted from Fig. 5c, which shows the profile of the MD05-2920 core. As a result, potential readers may not understand Fig. S4c. Please address this issue appropriately.

We thank the reviewer for this suggestion. We agree that this sentence relates to accumulation rate variations; however, we consider it directly relevant in this section, as it supports the interpretation of ^{10}Be flux variations for the PFE and ME events. Specifically, it provides context on the climatic conditions (interstadial periods with elevated accumulation rates) that could influence the flux estimates. For this reason, we have retained the sentence in this section but clarified its role in linking accumulation changes to the interpretation of the flux signal.

Regarding Figure 5, we thank the reviewer for pointing this out. The black stars have now been reintroduced in panel (c) to ensure consistency with Figure S4 and improve clarity for the reader.

Revisions:

L516: "Nevertheless, both events occur during interstadial periods (MIS 7.3 for PFE and MIS 7.5 for ME) marked by elevated accumulation rates, approximately 8 cm a^{-1} (Figure 2), **which influence ^{10}Be concentrations and thus supports the consideration of flux.**"



Line 576. Replace "(Horiuchi et al., 2016)" with "(Fig. 5 of Horiuchi et al. [2016])" to add information.

We thank the reviewer for this suggestion. We have modified the reference accordingly.

Lines 610–612. The 10Be stack by Frank et al. (1997) is based on 230Thex-normalized 10Be flux records rather than authigenic 10Be/9Be records. Correct the description appropriately. Additionally, it may be noted that the age model of the stack for the interval of interest is based on the classical SPECMAP d18O chronostratigraphy and other methods that are considered less reliable from a modern perspective.

We thank the reviewer for raising this point. We corrected the Figure 5 D label and the caption.

Revision:

L491: "***Figure 5****: Comparison of ^{10}Be and climate records from ice cores and marine sediments over 170–270 ka BP with: A. TALDICE δD (/‰), B. benthic δ^{18}O (/‰) from LR04 stack (Lisiecki and Raymo, 2005) and MD05-2920 (Tachikawa et al., 2014), C. TALDICE and Dome Fuji ^{10}Be (in purple and red respectively, /at cm^{-2} a^{-1}), D. authigenic ^{10}Be/^{9}Be from global stack reconstruction MD05-2920 (red), and MD05-2932 (purple) (Simon et al., 2016)* ***and ^{230}Th-corrected ^{10}Be (yellow, Frank et al., 1997),*** *and E. the ^{10}Be global production (/at cm^{-2} a^{-1}; Poluianov et al., 2016) calculated from RPI-based VADM (Channell et al., 2009). Grey bars highlight the main geomagnetic excursions discussed in the text (IBE, PFE, ME). Timing of identifiable geomagnetic features (black stars in TALDICE and RPI-based VADM) is used to obtain a revised chronology of RPI-based VADM used for calculating the ^{10}Be production, noting though a lower confidence on the star around 210 ka BP.*"

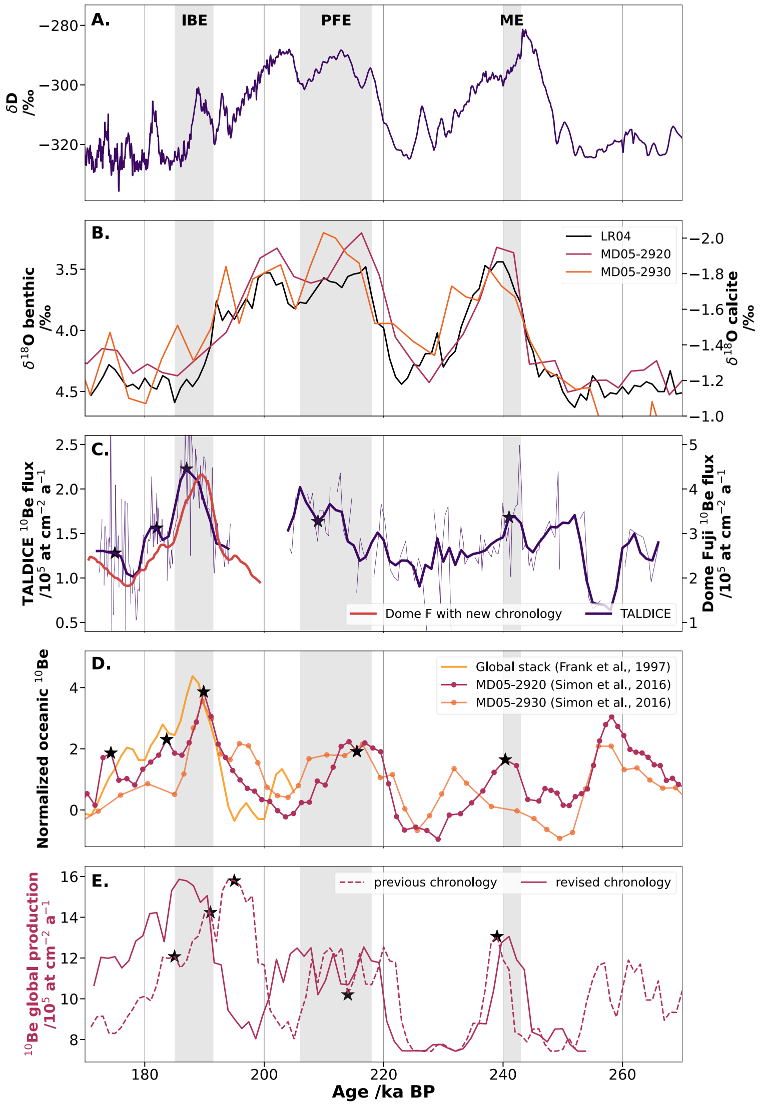


Table S1. Please include information on the KR0515-PC2 and -PC4 sediment cores (Yamazaki et al., 2008; Horiuchi et al., 2016).

We revised this accordingly.

Data disponibility (TALDICE_10Be_1470_1531_m.xlsx): Please indicate which 10Be data were excluded from the rolling average and show the "total" uncertainty (measurement uncertainty + variation uncertainty) of the 10Be flux.

We thank the reviewer for this suggestion. The identification of excluded ^{10}Be values is fully reproducible from the criteria described in the manuscript (LXX: “These ^{10}Be minima were identified when the concentration fell below the mean minus one standard deviation, both calculated using a 3-ka rolling window. The corrected ^{10}Be flux was calculated removing the ^{10}Be minima associated with a maximum in major ion concentrations.”), where minima associated with ion maxima are defined and removed prior to calculating the rolling average.

Regarding uncertainties, both components are already provided in the dataset. The measurement uncertainties are reported in the dataset. The accumulation uncertainty is directly taken in AICC2023 publication (Bouchet et al., 2023), however, we also included them in the shared dataset. The total uncertainty can therefore be calculated by combining these components.

To improve clarity, we have added a short description in the data availability section explaining how excluded values and total uncertainties can be identified and calculated from the provided dataset.

Revision:

L654: “Sheet 1: Depth top, depth bottom, Age top from AICC2023, age bottom, age uncertainty, accumulation, accumulation uncertainty, ^{10}Be concentration, ^{10}Be concentration uncertainty, and ^{10}Be flux. **We raise attention that AICC2023 was the most up to date chronology at the time of the submission, but updated chronology should be considered, especially to calculate accumulation, flux and their associated uncertainties. As described in section 3.3, ^{10}Be minima can be identified when the concentration fell below the mean minus one standard deviation, both calculated using a 3-ka rolling window.**”

The following are suggestions for technical corrections:

As for the English text, the revised parts of the manuscript are inferior to the original. I recommend having a native English speaker proofread the revised parts.

We revised the English using AI.

Line 253. Replace "Figure 1" with "Figure 2".

We revised this accordingly.

Line 327. Replace "macima" with "maxima".

We revised this accordingly.

Line 453. Replace "between" with "of".

We did not find the problematic ‘between’ the reviewer is referring to.