The response from the authors and the revised manuscript: Atmospheric ^{10}Be from Talos Dome (East Antarctic) ice core records geomagnetic dipole intensity from 170 to 270 ka BP by Lamothe et al.

The revised version of the manuscript is significantly improved. I have respect for the authors for revising the manuscript in such a short time and for adding new information on this subject. Nevertheless, I believe that several issues remain. Moreover, the revised manuscript has raised new concerns, particularly regarding consistency with the δ^{18}O chronostratigraphy. In my opinion, these issues should be addressed before this work can be published in GChron.

Further comments and questions regarding the authors' responses:

Comments on the original preprint are in green. Answers from the authors to these comments are in blue. Comments on the answers or new comments raised by the revised manuscripts are in black. Note: Not all previous comments and responses are included; only those related to new questions are included.

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 $^{10}Be/^{9}Be$ 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 post-depositional 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 post-depositional remanent magnetization acquisition in marine sediments”; “but extend the comparison to ice core ^{10}Be fluxes, which are not affected by magnetization processes”; and “Unlike comparisons involving RPI, a phase shift between ice core and oceanic ^{10}Be records cannot be attributed to magnetic lock-in effects.”).

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 ^{10}Be analysis, or are there other reasons why none were taken? Please clarify.

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 10Be 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.

Revision:
L240: “The outlier analysis was intentionally focused on minima, as low ^{10}Be values are far more frequent and have a strong impact on the rolling average than high values.”

I don't completely agree with this argument — I still think it would be better to delete only the ^{10}Be 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.

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 ^{10}Be 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 ^{10}Be 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.

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 ^{10}Be 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 ^{10}Be 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 ^{10}Be 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.

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 $\delta^{18}O$ 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 ×105 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 ^{10}Be 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 ^{10}Be 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.

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 10Be 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 $\delta^{18}O$-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?

<u>I think that this is an important point for this work to pass the review process</u>. The PISO-1500 record includes its original $\delta^{18}O$ stack, accompanied by the paleointensity stack. The authors should plot the $\delta^{18}O$ 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 $\delta^{18}O$ chronostratigraphy seems necessary.

<u>**Comments and questions regarding (mainly) the revised part of the manuscript.**</u>
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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 ^{10}Be sampling resolution, a low-concentration interval extending over 40 cm is counted as two distinct minima)

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

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

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

Lines 421–424. I know that Jouzel et al. (2026) made a such discussion based (at least partly) on the earlier argument with ^{10}Be 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.

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

Revised Fig. 4. Does the ^{10}Be 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.

Lines 476–479. Change the sentence as follows (the points are in bold): 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** by Horiuchi et al., (2016).

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

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.

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

Lines 610–612. The ^{10}Be stack by Frank et al. (1997) is based on $^{230}Th_{ex}$-normalized ^{10}Be flux records rather than authigenic $^{10}Be/^{9}Be$ 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 $\delta^{18}O$ chronostratigraphy and other methods that are considered less reliable from a modern perspective.

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

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

<u>The following are suggestions for technical corrections:</u>

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.

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

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

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