Peer Review File

Late Permian core-surface-magnetosphere conditions revealed by single crystal paleointensities from the Emeishan large igneous province

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This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
Review of the "Late Permian core, surface and magnetosphere conditions revealed from the Emeishan traps" manuscript by Huang, Tarduno, et al.

Review of the "Late Permian core, surface and magnetosphere conditions revealed from the Emeishan traps" manuscript by Huang, Tarduno, et al.
The presented study examines absolute geomagnetic paleointensity measured from single plagioclase crystals extracted from mafic (gabbroic) intrusions associated with the Emeishan Traps, a large igneous province. The authors report an average virtual dipole moment VDM = 4.1±0.7 x 1022 Am/m2 measured from ~ twenty individual crystals and argue for the robustness of these paleointensity estimates. The authors utilize the fact that the studied rocks cooled slowly to support further their argument that the obtained paleointensity represents the time-averaged geomagnetic field and is of high quality. Using the obtained paleofield strength value, the authors speculate about the magnetopause standoff, surface conditions – specifically whether emplacement of large igneous provinces and associated degassing could have triggered mass extinctions, the relationship between the paleofield strength and geomagnetic reversal frequency, and, finally, about changes in subduction flux that may affect the heat flux across the core-mantle boundary. The presented paper was not the easiest to review. But I will try to keep my review short.
First things first.
Although the reported paleointensity results are likely to be of exceptionally high technical quality, the scientific value and meaning of these data remain questionable. My primary concerns about the presented manuscript are:
1. The authors argue for the primary nature of natural remanent magnetization preserved in the samples studied. However, there is no evidence for this. No bulk-rock paleomagnetic study has been conducted on these rocks. It would be nice to see that the paleomagnetic signal recorded by these gabbros is consistent with published paleomagnetic results for the Emeishan large igneous province. The argument that similar rocks sampled some 40-50 km away from the studied location yielded typical (not so) Emeishan paleomagnetic directions does not stand criticism. Every paleomagnetist knows that the paleomagnetic signal may vary from "ideal" to unstable and non-interpretable even within a single lava flow or shallow intrusion.
2. Providing evidence for the slow cooling of the studied rocks, necessary to obtain the time-averaged paleomagnetic signal and to demonstrate that natural magnetic remanence has not been reset, the authors estimated that the gabbro intrusion cooled down to above 120 ºC by 35 Ma. Suggesting that it took 224 Myrs to reach the estimated temperature. Unfortunately, this is fatal to all authors' interpretations. Such prolonged cooling begs a question: what is the age of magnetization in the studied rocks? The timing of magnetization blocking is crucial in studying slowly cooled rocks. Blocking (and unblocking) temperatures depend on a variety of factors, including the chemistry of magnetic carriers and domain states. For example, magnetic remanence in magnetite, blocked at approximately 400 degrees, will completely reset if subjected to ambient heat of ~250 ºC for a sufficiently long time. In contrast, magnetization blocked at the Curie point is practically time-insensitive and would withstand even higher temperatures for more extended periods (Pulliah et al., 1975). Brown and McEnroe (2012) have shown that approximately 570 °C would be a reasonable estimate for the blocking temperature of single-domain oxyexsolved magnetite. How long did it take to cool the studied rocks from closure temperatures of the U-Pb system in zircon (950 degrees??) at 259 Ma (crystallization age, as indicated by U-Pb geochronology) to, say, 570-560 ºC, given that it took over 220 Myr to reach temperatures above 120 ºC? Using a crude estimate and assumption of conductive heat transfer, I

arrived at a somewhat expected number - the magnetic remanence would be blocked some 50-55 Myrs after gabbro's emplacement. I realize that such an estimate ultimately depends on the selected boundary conditions. However, blocking of magnetic remanence in slowly cooled massive intrusions MUST post-date crystallization and U-Pb closure in zircons. In addition, Triassic and Jurassic Ar-Ar ages have also been reported, which may suggest a more complex thermal history of the area. Unfortunately, blocking temperatures and the age of magnetization were not discussed whatsoever, which I am particularly and unpleasantly surprised by.
3. All parts of the discussion ultimately depend on the age assigned to the reported paleointensity value. The unknown magnetization age renders the study questionable at best.
4. I would like to point out the lack of novelty. All parts of the discussion, solely based on the assumption of primary and stable magnetization in the studied rocks, provide nothing new. Moreover, most of the questions discussed are addressed in a rather superficial manner and do not advance our knowledge in any of them.
5. Some minor comments: the authors seem not to know some classical original literature. For example, when describing the low-temperature Verwey transition in magnetite, the authors cite everything but the original paper by Verwey (1939). While I am confident that the second author is familiar with this foundational study, referencing the original study would be more appropriate and appreciated.
Data on geomagnetic field intensity in the geologic past are crucial for our understanding of geodynamic evolution. Furthermore, these data are the only measurable quantity that reflects the efficiency of the geodynamo and, therefore, may shed light on long-term interactions between surface processes, such as plate tectonics, and those that occurred in the deep-Earth interior. As a paleomagnetist, I am keen on seeing more robust paleointensity determinations. However, the present study raises serious concerns. The uncertainty surrounding the age of magnetization renders the reported paleointensity values questionable at best. Incorporating such ambiguous data into the global paleointensity database risks undermining its overall reliability.
Overall, given the lack of paleomagnetic directions from the studied rocks and, more importantly, the largely unknown magnetization age, I do not recommend this manuscript for publication in Nature Communications.
If the authors decide to (re)submit the manuscript after necessary corrections/revisions/additions, I recommend a journal with an extended publication format.

Reviewer #2

(Remarks to the Author)
This paper "Late Permian core, surface and magnetosphere conditions revealed from the Emeishan Traps" by Huang et al. report paleointensity values around 259 Ma, obtained using the single crystal paleointensity (SCP) method on plagioclase crystals from Emeishan Traps gabbro. It has been increasingly recognized in recent studies that paleointensity records from slowly cooling rocks are essential for the precise evaluation of long-term evolution of geomagnetic field. However, these rocks often contain coarse-grained ferromagnetic minerals, which are unsuitable as paleomagnetic recorders and can hinder paleointensity determinations. Paleointensity values obtained using the SCP method on plagioclase crystals separated from slowly cooling gabbro provide a significant contribution to our understanding of long-term variations in the geomagnetic field and their relationship to geodynamics and surface environments.

The descriptions of the rock magnetic, paleomagnetic, and paleointensity experiments are clear. In the case of the SCP method, sample magnetization tends to be weak due to the small sample volume, resulting in a low signal-to-noise ratio. The plagioclase crystals used in their study have relatively large volumes (greater than 1 mm), leading to strong remanence and high-quality data. I find nothing in the text that would warrant rejection, but the paper could be further improved by addressing the following points:

[State of exsolved magnetite/titanomagnetite]
Empirical evidence shows that exsolved magnetite has a nearly pure composition, both in cases where it exsolved directly and where it experienced oxidation after exsolution.

- The majority of plagioclase samples exhibited blocking temperatures below 450 °C. Please clarify the underlying reason for this observation.
- Lines 124–128: Please explain how to determine mineral phases and how to distinguish titanomagnetite/ilmenite based on EDS analysis.
- Lines 124–128: Providing data on the ulvospinel content in titanomagnetite and the magnetite-to-titanomagnetite abundance ratio would be valuable for the interpretation of paleointensity data.
- Providing quantitative data for magnetic anisotropy of plagioclase crystals would be valuable for the interpretation of paleointensity data.

[Paleomagnetic data of plagioclase crystals]
- To ensure reliable paleomagnetic recording, the directional relationship between the bulk rock remanence and the remanence of plagioclase crystals should be reported whenever possible.

[Compilation of paleointensity data]
- For the reasons described above, a precise understanding of long-term variations in the geomagnetic field requires comparison of paleointensity data obtained using the SCP method on slowly cooled rocks. However, the present manuscript

relies on a subset of SCP results without providing justification. The authors should either include all available paleointensity data obtained from slowly cooled rocks using the SCP method or clearly explain the criteria for data selection."

[Other point]
- Lines 161–163: It is difficult to understand the meaning of this sentence.
- Figure 3: Field Strength -> Dipole Moment

Reviewer #3

(Remarks to the Author)
Comments on the manuscript (MS) titled “Late Permian core, surface and magnetosphere conditions revealed from the Emeishan Traps”, authored by Wentao Huang et alii.

I believe that the Authors present relevant data, acquired with a methodology that more and more seems to be the “way to go” for reliable paleomagnetic field intensity estimates. The MS addresses a geological problem (the late Permian extinction events) of wide interest.
Nonetheless the MS requires some work before acceptance. Specifically, there is an analytical problem that requires clarification. Other than that, there are some minor things to fix throughout the text.

ANALTICAL PROBLEM. I believe that the possible presence of anisotropy should be addressed better. Even if (Tarduno et al., 2006) state about anisotropy that “The higher value could result in only a few degrees of pTRM deflection during a typical Thellier experiment”, it could contribute to the presented data. Did you try to systematically explore the possible presence of anisotropy by measuring hysteresis parameters under different angles? I state this, because when looking at the published natural remanent magnetization (NRM) directions shown in the supplementary material, there are clear indications of remanence anisotropy. This is indicated by a marked elongated shape of the virtual geomagnetic poles (VGPs) in the equal area plot (see comments on supplementary figure 3 below).

OTHER COMMENTS. I feel like there is some misuse of plural form and prepositions throughout the MS, but I leave that to the native speaker co-authors.

Line 26. Change “have been” with “remains”.

Line 27. Just “We present”.

Line 28. Add some minimal details on the Emeishan Traps (location and age).

Lines 48-51. Please review this sentence, as I believe “ozone depletion” is repeated twice (beginning and end).

Line 82. With this citation stile style (superscript numbers) this statement “following42,43,46” is not clear. It is not a mistake, but I would rephrase to make it more readable.

Line 93. Also (Kato et al., 2024) recently published a review describing (and promoting) the single crystal method that might worth citing.

Lines 101-103. There is something off with the verbs. A physical property is determined and characterizes a sample. It is not characterized to determine something. I hope this makes sense.

Line 107. That is even less than small, it is barley noticeable. I would write “indicating the possible minor presence” so that it is also clear that is not observed systematically. In supplementary figure 4a the little hump is present also in the cooling curve, so if it does represent contribution of sulphides, it is still there…

Line 114. You should also cite the original (Day et al., 1977)

Line 142. The Arai plot original paper (Nagata & Arai, 1963) should be added.

Line 222. Change “could be” with “could derive”.

Lines 266-269. The second part of this sentence, about how the proxy has been improved, is not clear. Maybe “volcanic activity for a global” should be “volcanic activity as a global”? Not sure.

Line 271-272. What does exactly the statement “to induce enough CMB thermal condition changes” mean? This is a key point to understand Figure 3 and the rest of the main text. I believe that this planetary-scale process (in what way a lower subduction flux affects the reversals rate) should be described more clearly.

Line 298. Add country after Rochester.

Figure 1. It is somehow awkward to call panels not in the right (a, b, c, …) order. Just change the letters in the panels?

Furthermore, acronyms (SEM, EDS) should be spelled out. The same applies for all the figures.

Figure 2. The insets are clear enough, I do not think it is necessary to specify the position, so you can shorten a bit the caption. I would use “red” instead of “color”, also because black and blue of the horizontal projections are virtually undistinguishable (maybe use green?).

Figure 3. Very long intervals the blue belt seems very speculative, for examples between 160 and 260 Ma, and beyond 300 Ma, where similar data are missing. Maybe skip that final part of the graph, or use a lighter color? Put a semi comma in line 362, between “areas” and “reversals”. Last thing: it might be my limitation, but I do not understand what the red line in panel c represents.

Supplementary text 2. I understand it is not the subject of this study, but some concept can be clarified. The 45° cutoff refers to the virtual geomagnetic poles? The most recent works on paleosecular variations (Tauxe et al., 2024) strongly suggest checking the consistency of paleomagnetic directions with a paleosecular variations model avoiding the use of any cut-off, before using the directions themselves for paleogeography purposes. I am sure that this would not affect the presented result, but it might be worth mentioning such works, or maybe even try to test the directions dataset, even if already published.

Supplementary figure 3. The caption should explain the diagram better. I suppose panel c is the projection of the virtual geomagnetic poles (VGPs) onto an equal area diagram. I assume that the VGPs are rotated to be centered onto the vertical of the diagram (this should be specified). Why is the distribution so elongated? Even without test, I am sure this would not be consistent with any of the published paleosecular variation models (e.g., CP88, TK03.GAD, THG24), making the paleolatitude calculation suspicious (although both positive and negative paleomagnetic directions inclinations should anyway indicate a peri-equatorial acquisition of the remanence). Why is that? The first reason that comes into my mind is a strong anisotropy of magnetic remanence. This could have implications in your absolute paleointensity calculation, and it should be addressed (see main comment to the MS).

References cited in the review

Day, R., Fuller, M., & Schmidt, V. A. (1977). Hysteresis properties of titanomagnetite: grain-size and compositional dependence. Physics of the Earth and Planetary Interiors, 13(4), 260–267. https://doi.org/10.1016/0031-9201(77)90108-X

Kato, C., Usui, Y., & Sato, M. (2024). A brief review of single silicate crystal paleointensity: rock-magnetic characteristics, mineralogical backgrounds, methods and applications. Earth, Planets and Space, 76(1). https://doi.org/10.1186/s40623-024-01994-w

Nagata, T., & Arai, K. M. (1963). Secular Variation of the Geomagnetic Total Force during the Last 5000 Years. Journal of Geophysical Research, 68(18), 5277–5281.

Tarduno, J. A., Cottrell, R. D., & Smirnov, A. V. (2006). The paleomagnetism of single silicate crystals: Recording geomagnetic field strength during mixed polarity intervals, superchrons, and inner core growth. Reviews of Geophysics, 44(1), 1–31. https://doi.org/10.1029/2005RG000189

Tauxe, L., Heslop, D., & Gilder, S. A. (2024). Assessing Paleosecular Variation Averaging and Correcting Paleomagnetic Inclination Shallowing. Journal of Geophysical Research: Solid Earth, 129(8). https://doi.org/10.1029/2024JB029502

Version 1:

Reviewer comments:

Reviewer #3

(Remarks to the Author)
Comment on the manuscript titled “Late Permian core-surface-magnetosphere conditions revealed by single crystal paleointensities from the Emeishan large igneous province”, authored by Wentao Huang and coauthors.

firs comment: the manuscript could benefit from a further round of deep reading, as I found some typos or wrong verbs here and there (below).
I believe that this Manuscript represent a valuable contribution to the knowledge of a complex time of the Earth’s history, at least excluding a possible cause (or at least contributing cause) to the Late Permian mass extinction.
Having said that, I would like to suggest some improvement, especially for the paleomagnetic analyses. They have been performed (in terms of directions isolation and cluster analysis) ignoring some of the cutting-edge quality check and statistical approaches.

Please find my main comments below:
Figure 2 & paleomagnetic part
I am aware that this is not meant to be a pure paleomagnetic work, but I have a few concerns that should be addressed. Are the ChRM direction anchored to the origin of the demagnetization axis or not? If yes, did you apply the approach of Heslop

and Roberts (2016)? Anchored directions tend to have a artificially low MAD, and this method provides a metric for assessing if directions can be anchored or not.

Why did you use an inclination only fold-test? Your data have both declination and inclination, so the classic fold test (e.g., Enkin,2003; Tauxe & Watsons, 1994).

The ‘equal area’ diagrams have a strangely up-down elongated confidence ellipse. Is that a Fisher (1953) confidence? Or maybe diagrams are not ‘equal area’? Low inclination Fisher circles are much more horizontally elongated. See for example the attached equal area plot of your Figure 2e using another plotting software

Does the equatorial paleolatitude fit with paleogeographic reconstructions (i.e., coeval paleomagnetic data from elsewhere) for the Late Permian?

As a good practice (FAIR data), it would be nice to provide a repository where original raw demagnetization data can be extracted and plotted again. Personally, I struggle with the MagIC database but it seems to me the only the paleointensity data have been upladed. In any case, the choice to share the raw demag data is up to the Authors, as there is no obligation in that sense.
In the rebuttal letter it is stated that test for consistency with GGP models like the THG24 (Tauxe et al., 2025) were arbitrarily avoided. Since the scope of the research is not paleomagnetic in nature, and anyway quasi-horizontal directions are hardly affected by distortion that would change significantly the average significantly, I leave this choice to the Authors. But someone might want to use your paleomagnetic data for different (e.g. paleogeographic) analysis in the future, so it would be useful to have them accessible.
Final figure

The 85 Myr delay shown in figure 5. Is it assumed just “visually” in this work or is there any geophysical (e.g. subduction simulation) evidence? That would be important to support the connection between reversal frequency and slab subduction.

Reference n. 30 of the SI seems incomplete, there is no year, no DOI, and I could not find it online.
Some mistake through the main text
Line 25: wrong verb, should be ‘is’.
Line 26: add ‘the’ before ‘ca.’
Line 31: should be ‘gabbors’?
Line 46: change ‘kill’ with ‘trigger’
Line 50: check ‘deletion’
Line 65: change ‘the’ with ‘a’
Line 137-138: that is debatable. The cooling curve of both panels a and b (more evident in a) shows a similar hump at the same temperature.
Line 377: ‘by using’, or just ‘using’ instead of ‘by’
Line 408: I believe the company is called only AGICO, and Geofyzika is the former name.

Reference cited in the revision
Enkin, R. J. (2003). The direction - correction tilt test: An all-purpose tilt/fold test for paleomagnetic studies. Earth and Planetary Science Letters, 212(1–2), 151–166. https://doi.org/10.1016/S0012-821X(03)00238-3
Fisher, R. (1953). Dispersion on a sphere. Proceedings of the Royal Society of London, A217, 295–305. Retrieved from https://doi.org/10.1098/rspa.1953.0064
Heslop, D., & Roberts, A. P. (2016). Analyzing paleomagnetic data: To anchor or not to anchor? Journal of Geophysical Research: Solid Earth, 121(11), 7742–7753. https://doi.org/10.1002/2016JB013387
Tauxe, L., & Watson, G. S. (1994). The fold test: an eigen analysis approach. Earth and Planetary Science Letters, 122(3–4), 331–341. https://doi.org/10.1016/0012-821X(94)90006-X
Tauxe, L., Heslop, D., & Gilder, S. A. (2024). Assessing Paleosecular Variation Averaging and Correcting Paleomagnetic Inclination Shallowing. Journal of Geophysical Research: Solid Earth, 129(8). https://doi.org/10.1029/2024JB029502
Vandamme, D. (1994). A new method to determine paleosecular variation. Physics of the Earth and Planetary Interiors, 85(1–2), 131–142. https://doi.org/10.1016/0031-9201(94)90012-4

Reviewer #4

(Remarks to the Author)
This paper addresses a topic of great general interest: What caused the massive extinctions at the end of the Permian? One idea that was floated recently by Zhang et al., 2022 was that an extremely weak geomagnetic field was the culprit. This paper calls into question the reliability of those data and having looked at them myself, I agree that they are of highly questionable reliability. Then this paper introduces new data of exceptional quality from single crystals carefully extracted from two units dated at the End-Guadalupian and End Permian extinction events, just after the Permo-Carboniferous Reversed Polarity Superchron.
The two new results are equivalent to about half the present field, or the same as the Cenozoic long term average (see Tauxe, 2006). While these authors seem to discount results from that study by not even citing it I do think it is worth mentioning. There are, after all, a number of assumptions involved in the single crystal approach. 1) What was the cooling rate and what is an appropriate cooling rate correction - 10\% is pulled out of thin air to justify this. 2) The authors claim that

each sample represents an average field. This makes more sense and there is additional justification for that. Also, 3) The authors claim that no anisotropy correction is necessary based on the IRM results. IRM will show the least amount of anisotropy and is in no way equivalent to a thermal remanence. This has been the argument since the first paper on single crystals and I am still not buying it. Also, if magnetite is the dominant carrier, why are the blocking temperatures in the mid-400s? I find that a bit strange. Also, the argument using the Pullaiah Plot should be reconsidered in light of new results of Cych et al. (2025, 2026) using micromagnetic modelling. That shows that the traditional Neel theory based nomogram needs to be re-examined.
So, while I strongly agree that the results address a topic of great interest and that the technical quality of the results is excellent, I question whether Nature Communication is the appropriate venue.

minor comments:
line 25: The Late Permian are => The Late Permian is
line 188: "continuous range of Ti/Fe atomic from" - is there something missing here?

Version 2:

Reviewer comments:

Reviewer #3

(Remarks to the Author)
The authors have addressed my comments throughout the manuscript. I have only two minor requests, after which I recommend accepting the manuscript for publication without further revision.

PPCA Analysis (Supplementary table 5)
In Heslop & Roberts (2016), two probabilistic parameters are used to select between the Unanchored, Anchored, and Constrained interpolation approaches. The Constrained interpolation method was introduced in that same paper, and the choice between models is based on the parameter p(Hc|D) rather than p(Ha|D). In Supplementary Table 5, the listed interpolation methods are either Unanchored or Constrained. My concern is that the term Constrained may have been used in place of Anchored. If this is the case, please revise the “PCA model” column accordingly. Alternatively, if the interpolation method is indeed the Constrained approach of Heslop & Roberts (2016), please also report the corresponding p(Hc|D) values to justify the model selection.

Mean paleomagnetic directions and confidence.
The choice to use confidence ellipses following the approach of Deenen et al. (2011) is legit. However, the description provided in both the text and figures is currently insufficient. Stating that the ellipses are “based on A95 pole Fisher statistics” does not clearly describe the methodology. It should be explicitly stated that the confidence ellipses were calculated following the procedure of Deenen et al. (2011), and the paper should be cited.
Furthermore, this method yields two independent confidence parameters, namely the uncertainty in declination (ΔDec) and the uncertainty in inclination (ΔInc). Both values should be reported, rather than only the A95 confidence angle around the virtual geomagnetic pole.
I am not familiar enough with Paleomagnetism.org to know whether these parameters can be exported directly. If not, other freely available alternatives perform the same calculations and provide exportable results. One is Magnetic-A (Dallanave, 2026; https://doi.org/10.1029/2025EA004863), which also includes implementation of the Heslop & Roberts (2016) PCA model analysis with exportable text outputs.

Reviewer #4

(Remarks to the Author)
The authors have carefully considered the comments by reviewers #3 and #4. I think the manuscript in its revised state merits publication in Nature Communications and hope future work will expand the number and quality of paleointensity results from this very interesting time period. This is a good start.
Lisa Tauxe

Response to Comments

Reviewer #1 :

Comment: "The presented study examines absolute geomagnetic paleointensity measured from single plagioclase crystals extracted from mafic (gabbroic) intrusions associated with the Emeishan Traps, a large igneous province. The authors report an average virtual dipole moment VDM = 4.1 ± 0.7 x 10^{22} Am/m^2 measured from ~ twenty individual crystals and argue for the robustness of these paleointensity estimates. The authors utilize the fact that the studied rocks cooled slowly to support further their argument that the obtained paleointensity represents the time-averaged geomagnetic field and is of high quality. Using the obtained paleofield strength value, the authors speculate about the magnetopause standoff, surface conditions – specifically whether emplacement of large igneous provinces and associated degassing could have triggered mass extinctions, the relationship between the paleofield strength and geomagnetic reversal frequency, and, finally, about changes in subduction flux that may affect the heat flux across the core-mantle boundary. The presented paper was not the easiest to review. But I will try to keep my review short."

Response: We thank the reviewer for this thorough and thoughtful assessment of our manuscript. We appreciate the effort taken to summarize the scope of the study and its broader implications. In response to the reviewer's comments, we have substantially revised the manuscript to improve clarity, strengthen the presentation of the paleointensity dataset, and better structure the discussion of its geodynamic and environmental implications. In particular, we have clarified the basis for interpreting the results as time-averaged field estimates, strengthened constraints on magnetization age and stability, and refined the discussion to focus on quantitatively supported interpretations.

Comment: "First things first. Although the reported paleointensity results are likely to be of exceptionally high technical quality, the scientific value and meaning of these data remain questionable. My primary concerns about the presented manuscript are:"

Response: We thank the reviewer for recognizing the high technical quality of our paleointensity data. We agree that establishing the scientific value and interpretation of these results requires robust constraints on magnetization age and stability. In the revised manuscript, we have directly addressed these concerns by incorporating new ***bulk-rock paleomagnetic data, zircon U–Pb and***

fission track geochronology. *These independent datasets demonstrate that (i) the studied gabbros carry stable and regionally consistent primary remanent magnetization, and (ii) magnetization was acquired shortly after emplacement during relatively rapid cooling*[1] *through the relevant blocking temperature range. These additions remove the ambiguity surrounding magnetization age and provide a firm temporal and physical basis for interpreting the single-crystal paleointensity results. As a result, the scientific significance of the dataset is now clearly established.*

Comment: "1. The authors argue for the primary nature of natural remanent magnetization preserved in the samples studied. However, there is no evidence for this. No bulk-rock paleomagnetic study has been conducted on these rocks. It would be nice to see that the paleomagnetic signal recorded by these gabbros is consistent with published paleomagnetic results for the Emeishan large igneous province. The argument that similar rocks sampled some 40-50 km away from the studied location yielded typical (not so) Emeishan paleomagnetic directions does not stand criticism. Every paleomagnetist knows that the paleomagnetic signal may vary from "ideal" to unstable and non-interpretable even within a single lava flow or shallow intrusion."

Response: We thank the reviewer for this critical comment, which prompted us to conduct new fieldwork and bulk-rock paleomagnetic analyses on the studied gabbros. To directly evaluate the remanence recorded by these rocks, we collected samples from ***eight oriented sites*** *(eight samples per site) in the same open-pit mine from which the plagioclase crystals used for paleointensity measurements were obtained. Stepwise thermal and alternating-field demagnetization successfully isolated stable characteristic remanent magnetization (ChRM) components from the majority of samples (Fig. 2). The site-mean ChRM directions yield a tilt-corrected mean of* ***D/I = 350°/5.1°****. The inclination is consistent with published paleomagnetic results from the ELIP lava flows in the Bingchuan area and gabbro from the Panzhihua intrusion (Fig. 2; Supplementary Fig. 3), supporting a primary geomagnetic field origin. The difference in declination is interpreted as the result of local vertical-axis rotations, which are well documented in this region. In addition, an inclination-only fold test shows that the Arason-Levi's k parameter reaches a maximum at ~70–110% unfolding (Supplementary Fig. 3g), indicating a* ***pre-tilting origin*** *of the ChRM and further*

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[1] *Here we note that the cooling was sufficiently rapid such that resetting due to prolonged time at elevated temperature is not an issue, but sufficiently long that significant time-averaging occurs.*

supporting its primary nature. These new data directly address the reviewer's concern by demonstrating that the studied gabbros record a stable and regionally consistent paleomagnetic signal. Together with the independent geochronological constraints and the thermal stability of the single-crystal remanence, they provide robust evidence that the magnetization used for paleointensity determination is primary.

Comment: "2. Providing evidence for the slow cooling of the studied rocks, necessary to obtain the time-averaged paleomagnetic signal and to demonstrate that natural magnetic remanence has not been reset, the authors estimated that the gabbro intrusion cooled down to above 120 ºC by 35 Ma. Suggesting that it took 224 Myrs to reach the estimated temperature. Unfortunately, this is fatal to all authors' interpretations. Such prolonged cooling begs a question: what is the age of magnetization in the studied rocks? The timing of magnetization blocking is crucial in studying slowly cooled rocks. Blocking (and unblocking) temperatures depend on a variety of factors, including the chemistry of magnetic carriers and domain states. For example, magnetic remanence in magnetite, blocked at approximately 400 degrees, will completely reset if subjected to ambient heat of ~250 ºC for a sufficiently long time. In contrast, magnetization blocked at the Curie point is practically time-insensitive and would withstand even higher temperatures for more extended periods (Pulliah et al., 1975). Brown and McEnroe (2012) have shown that approximately 570 °C would be a reasonable estimate for the blocking temperature of single-domain oxyexsolved magnetite. How long did it take to cool the studied rocks from closure temperatures of the U-Pb system in zircon (950 degrees??) at 259 Ma (crystallization age, as indicated by U-Pb geochronology) to, say, 570-560 ºC, given that it took over 220 Myr to reach temperatures above 120 ºC? Using a crude estimate and assumption of conductive heat transfer, I arrived at a somewhat expected number - the magnetic remanence would be blocked some 50-55 Myrs after gabbro's emplacement. I realize that such an estimate ultimately depends on the selected boundary conditions. However, blocking of magnetic remanence in slowly cooled massive intrusions MUST post-date crystallization and U-Pb closure in zircons. In addition, Triassic and Jurassic Ar-Ar ages have also been reported, which may suggest a more complex thermal history of the area. Unfortunately, blocking temperatures and the age of magnetization were not discussed whatsoever, which I am particularly and unpleasantly surprised by."

Response: We thank the reviewer for this important and insightful comment regarding the timing of magnetization acquisition in slowly cooled intrusions. We agree that the relationship between cooling history and blocking temperatures is critical for interpreting paleomagnetic data. In the revised manuscript, we have added new geochronological constraints to directly address this issue. Zircon U-Pb dating confirms emplacement of the Baima intrusions at ~260 Ma, while zircon fission track ages from the same sample yield ~255 Ma. When combined with published apatite fission-track and (U-Th-Sm)/He data from this region, inverse thermal modeling (Fig. 1d) indicates that the intrusions cooled rapidly to ~220 °C within ~2 Myr and to ~160 °C within ~6 Myr after emplacement, followed by long-term slow cooling to ~120 °C by ~37 Ma. This thermal history demonstrates that the rocks passed through the ***relevant blocking temperature window (~560–400 °C)*** *very shortly after emplacement, likely within* ***the first few million years****. Therefore, the primary thermoremanent magnetization was acquired during early post-emplacement cooling, rather than tens of millions of years later. Importantly, subsequent temperatures remained well below those required for thermally resetting single domain magnetite or low-Ti titanomagnetite over geological timescales (e.g., Pullaiah et al., 1975), excluding the possibility of delayed remanence acquisition or later thermal overprinting. These conclusions are further supported by (i) the consistency of bulk-rock paleomagnetic directions with regional ELIP data, and (ii) the thermal stability of remanence in single plagioclase crystals, which plot within the stable field on Néel's single-domain blocking/unblocking nomograms (Supplementary Fig. 11).*

Comment: "3. All parts of the discussion ultimately depend on the age assigned to the reported paleointensity value. The unknown magnetization age renders the study questionable at best."

Response: We agree with the reviewer that the age of magnetization is critical for interpreting paleointensity data. In the revised manuscript, we have directly addressed this issue by integrating new paleomagnetic and geochronological constraints. Bulk-rock paleomagnetic analyses from eight oriented sites demonstrate stable ChRM directions that are consistent in inclination with published Emeishan large igneous province data, supporting a primary geomagnetic field origin. In addition, an inclination-only fold test indicates a pre-tilting acquisition of the remanence. New zircon U-Pb and fission track data, together with inverse thermal modeling, show that the intrusions cooled rapidly through the relevant blocking temperature range (~560-400 °C) within the first few million years after emplacement at ~260 Ma. Subsequent thermal histories remained *well below temperatures required for remagnetization. These independent lines of evidence demonstrate that the magnetization was acquired shortly after emplacement and is coeval with Emeishan magmatism. Therefore, the age of the remanence is well constrained and does not introduce uncertainty into the interpretation of the paleointensity results.*

Comment: "4. I would like to point out the lack of novelty. All parts of the discussion, solely based on the assumption of primary and stable magnetization in the studied rocks, provide nothing new. Moreover, most of the questions discussed are addressed in a rather superficial manner and do not advance our knowledge in any of them."

Response: We thank the reviewer for this critical comment. In the revised manuscript, we have substantially strengthened the evidence for primary magnetization, clarified the novelty of our study, and significantly deepened the discussion.

Primary and stable magnetization. *We emphasize that the primary nature of the remanence is not assumed, but is now supported by multiple independent lines of evidence: (1) Stable ChRM is isolated from bulk Baima gabbro and shows regional consistency in inclination with Emeishan large igneous province data. (2) Combined zircon U-Pb and fission track dates demonstrate a simple cooling history, with rapid cooling through the relevant blocking temperature range shortly after emplacement, indicating early acquisition of thermoremanent magnetization. (3) The unblocking temperature spectra of plagioclase-hosted inclusions, when evaluated using Néel's single domain theory, exclude the possibility of later thermal resetting.*

Novelty of the study. *Our high-quality single-crystal paleointensity dataset from Emeishan gabbro differs fundamentally from previously published bulk-rock lava flow results, which are affected by secular variation, remagnetization, and multidomain effects (Supplementary Text 2). Notably, published studies have reached highly divergent conclusions, ranging from strong geomagnetic field intensity (Thomas et al., 1998) to extremely weak fields linked to environmental crises (Zhang et al., 2025, <u>we remind the reviewer to note that this study was published after our initial submission</u>). The novelty of this study lies in: (i) the application of state-of-the-art single-crystal paleointensity techniques to the ELIP gabbro, allowing isolation of primary thermoremanent magnetization and effectively overcoming biases inherent in bulk-rock measurements; and (ii) the* *provision of the first robust constraint, based on single domain magnetic carriers, on the Late Permian geomagnetic field strength during emplacement of the Emeishan and Siberian large igneous provinces.*

Substantially expanded discussion. *We have restructured the Discussion to provide quantitative, hypothesis-driven interpretations: (1) We demonstrate that the Late Permian dipole moment (PDM = $4.1 \pm 0.7 \times 10^{22}$ A m^2) is robust and reconcile discrepancies among previous datasets by identifying their systematic biases. (2) We quantitatively test the hypothesis that reduced geomagnetic field strength contributed to enhanced surface UV-B radiation. Our results (~51% of the present-day field) rule out extreme field weakening as a primary driver of ozone collapse and biotic stress. (3) We explicitly compare geomagnetic forcing with alternative mechanisms, showing that volcanic halogen release provides a more plausible explanation for environmental change. (4) We integrate our result with global datasets to evaluate the relationship between geomagnetic field strength and reversal frequency, providing a reliable anchor point for geodynamo models.*

These revisions transform the manuscript from a descriptive study into a set of quantitative constraints that resolve discrepancies in existing datasets and advance understanding of Late Permian geomagnetic field behavior and its environmental implications. We believe these changes fully address the reviewer's concerns regarding novelty, robustness, and depth of interpretation, and we thank the reviewer for helping us improve the manuscript.

Comment: "5. Some minor comments: the authors seem not to know some classical original literature. For example, when describing the low-temperature Verwey transition in magnetite, the authors cite everything but the original paper by Verwey (1939). While I am confident that the second author is familiar with this foundational study, referencing the original study would be more appropriate and appreciated."

Response: We thank the reviewer for this helpful comment. We have now added the original study by Verwey (1939) to the relevant section discussing the low-temperature transition in magnetite, and updated the citations accordingly in the revised manuscript.

Comment: "Data on geomagnetic field intensity in the geologic past are crucial for our understanding of geodynamic evolution. Furthermore, these data are the only measurable quantity that reflects the efficiency of the geodynamo and, therefore, may shed light on long-term interactions between surface processes, such as plate tectonics, and those that occurred in the deep-Earth interior. As a paleomagnetist, I am keen on seeing more robust paleointensity determinations. However, the present study raises serious concerns. The uncertainty surrounding the age of magnetization renders the reported paleointensity values questionable at best. Incorporating such ambiguous data into the global paleointensity database risks undermining its overall reliability. Overall, given the lack of paleomagnetic directions from the studied rocks and, more importantly, the largely unknown magnetization age, I do not recommend this manuscript for publication in Nature Communications. If the authors decide to (re)submit the manuscript after necessary corrections/revisions/additions, I recommend a journal with an extended publication format."

Response: We thank the reviewer for this detailed and thoughtful assessment. We fully agree that robust constraints on the age and origin of magnetization are essential for reliable paleointensity determinations and for their inclusion in global databases. In the revised manuscript, we have directly addressed this concern by incorporating multiple independent datasets that tightly constrain both the timing and primary nature of the recorded remanence. We emphasize that ***1. magnetization age of the ELIP gabbro is now well constrained; 2. our single crystal approach ensures remanence and paleointensity fidelity; 3. we strengthens the reliability of global paleointensity compilations by providing a well-constrained benchmark for the Late Permian.***

Reviewer #2

This paper "Late Permian core, surface and magnetosphere conditions revealed from the Emeishan Traps" by Huang et al. report paleointensity values around 259 Ma, obtained using the single crystal paleointensity (SCP) method on plagioclase crystals from Emeishan Traps gabbro. It has been increasingly recognized in recent studies that paleointensity records from slowly cooling rocks are essential for the precise evaluation of long-term evolution of geomagnetic field. However, these rocks often contain coarse-grained ferromagnetic minerals, which are unsuitable as paleomagnetic recorders and can hinder paleointensity determinations. Paleointensity values obtained using the SCP method on plagioclase crystals separated from slowly cooling gabbro provide a significant contribution to our understanding of long-term variations in the geomagnetic field and their relationship to geodynamics and surface environments.

Response: We agree and thank the reviewer for this positive and insightful assessment.

The descriptions of the rock magnetic, paleomagnetic, and paleointensity experiments are clear. In the case of the SCP method, sample magnetization tends to be weak due to the small sample volume, resulting in a low signal-to-noise ratio. The plagioclase crystals used in their study have relatively large volumes (greater than 1 mm), leading to strong remanence and high-quality data. I find nothing in the text that would warrant rejection, but the paper could be further improved by addressing the following points:

Response: We thank the reviewer for this positive and constructive assessment. We appreciate the recognition that the rock magnetic, paleomagnetic, and paleointensity experiments are clearly described, and that the relatively large plagioclase crystals used in this study yield strong remanence and high-quality SCP data. We agree that signal-to-noise ratio is a critical factor in single-crystal paleointensity studies. In this work, the use of millimeter-scale plagioclase crystals, combined with careful sample selection and strict experimental criteria, allows us to obtain stable and reproducible results. We thank the reviewer for the helpful suggestions below, which have improved the clarity and robustness of the manuscript.

[State of exsolved magnetite/titanomagnetite]

Empirical evidence shows that exsolved magnetite has a nearly pure composition, both in cases where it exsolved directly and where it experienced oxidation after exsolution. The majority of plagioclase samples exhibited blocking temperatures below 450 °C. Please clarify the underlying reason for this observation.

We thank the reviewer for this helpful comment. In our study, the magnetic inclusions hosted within plagioclase are interpreted to have formed at high temperature and are dominated by magnetite and titanomagnetite. We conducted SEM observations on nine representative plagioclase grains and performed 257 EDS spot alalyses on (titano)magnetite neddle-like inclusions. The Ti/Fe atomic ratios range from 0 to 2.8, corresponding to x = 0-2.2 in $Fe_{3-x}Ti_xO_4$. Among these, 64 analyses yield x > 0.8, which corresponds to very low unblocking temperature (20 °C) (Lattard et al., 2006) and therefore do not contribute to the NRM, 87 analyses have x of 0, indicating pure magnetite, and 56 analyses fall within x = 0-0.2, indicating low-Ti titanomagnetite.

These inclusions are typically compositionally homogeneous at the scale of analysis, consistent with empirical observations that exsolved magnetite is commonly near-stoichiometric (Fig. 3e). In addition, SEM observations reveal needle-like inclusions exhibiting intergrowth of magnetite with titanomagnetite, with magnetite domains locally associated with minor Ti-rich titanomagnetite segments (Fig. 3f; Supplementary Fig. 8d). These Ti-rich regions are characterized by lower blocking temperatures and are unlikely to contribute significantly to the NRM (Supplementary Table 7). Their presence further supports a high-temperature origin of the magnetic inclusions.

The 193 analyses ($x \leq 0.8$) have a mean x value of 0.14, corresponding to a T_c of ~490 °C. This value closely matches the maximum temperature used for reliable paleointensity fit (up to 488 °C, Supplementary 10d, Supplementary Table 7), indicating that the observed unblocking temperature spectrum is consistent with the intrinsic titanomagnetite composition of the magnetic inclusions. Although the analyzed titanomagnetite inclusions represent a subset of these present within each crystal, the results demonstrates that the observed blocking temperatures below ~450 °C for most grains are a natural consequence of titanomagnetite composition.

- Lines 124–128: Please explain how to determine mineral phases and how to distinguish titanomagnetite/ilmenite based on EDS analysis.

Response: We thank the reviewer for this important question. Mineral phases were identified using a combination of SEM imaging and EDS analyses, based on elemental composition and stoichiometry. Titanomagnetite and ilmenite were distinguished primarily using Fe-Ti atomic ratios derived from EDS spectra. Titanomagnetite follows the solid solution $Fe_{3-x}Ti_xO_4$ (spinel structure), characterized by Fe-rich compositions with variable Ti substitution (expressed as x), whereas ilmenite corresponds to $FeTiO_3$, with Fe:Ti ≈ 1:1. In our 257 spot analyses of the needle Fe-Ti oxide inclusions, Ti/Fe ratios range from 0 to 2.8. Of these, 241 analyses fall between 0 and 0.9, and the rest between 0.9 and 2.8 (Supplementary Table 7). Notably, none of the analyses yield Ti/Fe ratios close to 1, thereby excluding the presence of ilmenite.

- Lines 124–128: Providing data on the ulvospinel content in titanomagnetite and the magnetite-to-titanomagnetite abundance ratio would be valuable for the interpretation of paleointensity data.

Response: We thank the reviewer for this helpful suggestion. In the revised manuscript, we have added the compositional characterization of the plagioclase background (30 spot analyses) and magnetic inclusions (257 spot analyses) based on our EDS dataset (Supplementary Table 7). Specifically, we now report the distribution of ulvöspinel content (x in $Fe_{3-x}Ti_xO_4$) derived from Ti/Fe atomic ratios. The results show that x values span a wide range (0-2.2), with the majority of analyses (193 out of 257) falling within $x \le 0.8$, corresponding to magnetic carriers capable of preserving a stable NRM.

- Providing quantitative data for magnetic anisotropy of plagioclase crystals would be valuable for the interpretation of paleointensity data.

Response: We thank the reviewer for this valuable suggestion. Magnetic anisotropy can influence paleointensity estimates, particularly in bulk-rock measurements. In our study, the use of single plagioclase crystals hosting nanoscale magnetic inclusions inherently minimizes this effect, as the remanence is carried by a large population of inclusions with effectively random orientations within each crystal. To further evaluate this, we have conducted hysteresis measurements of individual crystals along eight different orientations, and included these new data in the revised manuscript (Supplementary Fig. 7; Supplementary Table 4). The results show only minor variations in hysteresis parameters across different directions, indicating that magnetic anisotropy is weak and does not significantly affect the magnetic response of the crystals. These *observations support the interpretation that anisotropy effects on the derived paleointensity values are negligible.*

[Paleomagnetic data of plagioclase crystals]

- To ensure reliable paleomagnetic recording, the directional relationship between the bulk rock remanence and the remanence of plagioclase crystals should be reported whenever possible.

Response: We thank the reviewer for this valuable suggestion. In the revised manuscript, we have added new bulk-rock paleomagnetic data to evaluate the relationship between bulk remanence and that carried by plagioclase crystals. Specifically, we conducted paleomagnetic analyses on eight sites (eight samples per site) of gabbro from the Baima open-pit mine. Stable characteristic remanent magnetization (ChRM) directions were isolated from most samples. The resulting inclinations are consistent with those reported for the ELIP lava flows and layered intrusions (Fig. 2; Supplementary Tables 5, 6), while variations in declination are attributed to local vertical-axis rotations. These results, together with new zircon U-Pb and fission track dates that constrain a simple post-emplacement cooling history, support the interpretation that the Baima gabbro preserves primary thermoremanent magnetization and excludes significant remagnetization or delayed remanence acquisition. Paleointensity experiments were conducted on unoriented plagioclase crystals. The thermal demagnetization behavior of these grains is stable, with the remanence used for paleointensity determinations consistently isolated over temperature intervals of ~296-400 °C and 400-488 °C. According to Néel's single-domain theory, remanence blocked within this temperature range is resistant to resetting under the restored subsequent thermal conditions (Supplementary Fig. 11). While direct directional comparison between bulk-rock and individual plagioclase crystals would ideally require oriented crystal extraction, the consistency between bulk-rock ChRM directions, thermal stability of single-crystal remanence, and independent geochronological constraints collectively provide strong evidence that the plagioclase-hosted remanence is primary and faithfully records the geomagnetic field.

[Compilation of paleointensity data]

- For the reasons described above, a precise understanding of long-term variations in the geomagnetic field requires comparison of paleointensity data obtained using the SCP method on slowly cooled rocks. However, the present manuscript relies on a subset of SCP results without providing justification. The authors should either include all available paleointensity data obtained from slowly cooled rocks using the SCP method or clearly explain the criteria for data selection."

Response: We thank the reviewer for this important point. We fully agree that robust evaluation of long-term geomagnetic field variations requires careful consideration of paleointensity data. In the revised manuscript, we clarify that ***all currently available SCP paleointensity results since ~400 Ma (n = 6)*** *are included in Fig. 5a; no SCP data have been excluded. Specifically, we distinguish between:* ***(1) Time-averaged paleointensity estimates*** *(large blue squares in Fig. 5a), which are more suitable for constraining long-term geomagnetic field behavior. These include SCP results from Cretaceous (117.6 Ma, 95.3 Ma) and Paleogene (55.6 Ma) lavas (Tarduno et al., 2001, 2002; Tarduno and Cottrell, 2005), as well as the Cretaceous (~115 Ma) granite (Kato et al., 2018).* ***(2) Instantaneous paleointensity estimates****, derived from small groups of individual lava flows (160.1 Ma and 265.4 Ma), which may not adequately average geomagnetic secular variation and are therefore shown separately (small blue squares in Fig. 5a). We have revised the manuscript to clearly explain this distinction and its implications for interpreting long-term field behavior. We further emphasize that* ***only one previous SCP study (~115 Ma granite)*** *provides a paleointensity estimate from a slowly cooled intrusive rock*[2]*. Magnetic carriers in these plagioclase crystals are PSD magnetite, thus no cooling rate correction should be applied to the data. In contrast, our new results from Emeishan gabbro represent the* ***first robust, time-averaged paleointensity constraint since ~400 Ma derived from SD (titano)magnetite hosted in slowly cooled rocks****. This dataset therefore fills a critical gap in the currently sparse high-quality SCP record and provides a key benchmark for evaluating long-term geomagnetic field evolution.*

[Other point]

- Lines 161–163: It is difficult to understand the meaning of this sentence.

Response: We thank the reviewer for this comment. We have revised this sentence in the text.

- Figure 3: Field Strength -> Dipole Moment

Response: Thank you, we have revised this figure accordingly.

[2] *There are many other SCP studies of slowly cooled rocks available for the Precambrian.*

Reviewer #3

I believe that the Authors present relevant data, acquired with a methodology that more and more seems to be the "way to go" for reliable paleomagnetic field intensity estimates. The MS addresses a geological problem (the late Permian extinction events) of wide interest. Nonetheless the MS requires some work before acceptance. Specifically, there is an analytical problem that requires clarification. Other than that, there are some minor things to fix throughout the text.

Response: We thank the reviewer for the positive assessment of our study and for recognizing the potential of the single-crystal paleointensity approach for obtaining reliable geomagnetic field intensity estimates. We also appreciate the acknowledgment of the broader geological significance of our work. In the revised manuscript, we have carefully addressed the analytical issue raised by the reviewer and provide detailed clarification in the relevant sections (see response to specific comment below). In addition, we have thoroughly revised the manuscript to address all minor comments and improve clarity, consistency, and presentation throughout the text. We believe these revisions have significantly strengthened the manuscript and improved its overall quality.

ANALTICAL PROBLEM. I believe that the possible presence of anisotropy should be addressed better. Even if (Tarduno et al., 2006) state about anisotropy that "The higher value could result in only a few degrees of pTRM deflection during a typical Thellier experiment", it could contribute to the presented data. Did you try to systematically explore the possible presence of anisotropy by measuring hysteresis parameters under different angles? I state this, because when looking at the published natural remanent magnetization (NRM) directions shown in the supplementary material, there are clear indications of remanence anisotropy. This is indicated by a marked elongated shape of the virtual geomagnetic poles (VGPs) in the equal area plot (see comments on supplementary figure 3 below).

Response: We thank the reviewer for this important comment regarding potential magnetic anisotropy. We agree that anisotropy can, in principle, influence paleointensity estimates and therefore requires careful evaluation. To address this, we have now systematically assessed anisotropy at the single-crystal scale by conducting ***hysteresis measurements along eight different orientations*** *for representative plagioclase crystals. The results show only minor variations in hysteresis parameters with orientation, indicating that magnetic anisotropy is weak* *and does not significantly affect the magnetic behavior of the inclusions. These new data are presented in Supplementary Fig. 7 and Table 4, and discussed in the revised manuscript.*

Regarding the reviewer's observation of elongated virtual geomagnetic pole (VGP) distributions, we note that these correspond to previously published paleomagnetic results of bulk rock from the ELIP basaltic lava flows in the Binchuan area (Xu et al., 2022), rather than data from the studied gabbro. In that study, samples were collected from multiple fault-separated sections, and the observed VGP elongation is likely influenced by variable local vertical-axis rotations associated with fault activity. In contrast, our new bulk-rock paleomagnetic results from eight gabbro sites at the studied locality yield stable ChRM directions without pronounced elongation, with a mean direction of D/I = 350°/5.1° (Fig. 2). Moreover, there is no alignment of feldspar crystals in the gabbro studied.

Furthermore, the use of single plagioclase crystals containing a large population of nanoscale magnetic inclusions with effectively random orientations inherently minimizes anisotropy effects. This interpretation is consistent with previous experimental and modeling studies (e.g., Tarduno et al., 2006; Nikolaisen et al., 2022) as well as prior TRM anisotropy measurements on single plagioclase crystals (Bono et al., 2019; Zhou et al., 2022; Huang et al., 2024).

Taken together, the weak anisotropy indicated by our new measurements, the absence of VGP elongation in the studied gabbros, and the intrinsic properties of the single-crystal approach all demonstrate that anisotropy effects on the derived paleointensity values are negligible. We have added these new analyses and expanded the discussion of anisotropy in the revised manuscript.

OTHER COMMENTS. I feel like there is some misuse of plural form and prepositions throughout the MS, but I leave that to the native speaker co-authors.

Response: We thank the reviewer for this helpful comment. We have carefully revised the manuscript to correct the use of plural forms and prepositions, and to improve the overall clarity and readability of the text. In addition, the manuscript has been thoroughly edited by our native English-speaking co-authors.

Line 26. Change "have been" with "remains".

Response: We thank the reviewer for this suggestion. The text has been revised accordingly.

Line 27. Just "We present".

Response: We thank the reviewer for this suggestion. The text has been revised accordingly.

Line 28. Add some minimal details on the Emeishan Traps (location and age).

Response: We thank the reviewer for this suggestion. The age of the Emeishan Traps is already provided in the preceding sentence; however, we have revised the text to include additional information on the geographic location of the studied gabbro for clarity.

Lines 48-51. Please review this sentence, as I believe "ozone depletion" is repeated twice (beginning and end).

Response: We thank the reviewer for this helpful comment. We have revised the sentence to remove the repeated phrase "ozone depletion" at the end.

Line 82. With this citation stile style (superscript numbers) this statement "following42,43,46" is not clear. It is not a mistake, but I would rephrase to make it more readable.

Response: We thank the reviewer for this helpful comment. We have revised the sentence to improve clarity by removing these citations, as our added geochronological, thermochronological and bulk-rock paleomagnetic studies were not presented in these references.

Line 93. Also (Kato et al., 2024) recently published a review describing (and promoting) the single crystal method that might worth citing.

Response: We thank the reviewer for this helpful suggestion. We note that we have already cited the SCP study of 115 Ma granite by Kato et al. (2018) (Fig. 5a). Regarding the review by Kato et al. (2024), we agree that it provides a useful overview of the single-crystal paleointensity approach; however, in this section we specifically refer to case studies of mafic intrusions. We have therefore considered the suggestion carefully and now cite the review paper in the revised Discussion part to better acknowledge recent developments in the field.

Lines 101-103. There is something off with the verbs. A physical property is determined and characterizes a sample. It is not characterized to determine something. I hope this makes sense.

Response: We thank the reviewer for this helpful comment. We have revised the sentence by switching the positions of the two verbs to improve clarity and grammatical correctness.

Line 107. That is even less than small, it is barley noticeable. I would write "indicating the possible minor presence" so that it is also clear that is not observed systematically. In supplementary figure 4a the little hump is present also in the cooling curve, so if it does represent contribution of sulphides, it is still there…

Response: We thank the reviewer for this helpful comment. We have revised the sentence accordingly.

Line 114. You should also cite the original (Day et al., 1977)

Response: We thank the reviewer for this helpful comment. The original reference has been cited in the revised manuscript.

Line 142. The Arai plot original paper (Nagata & Arai, 1963) should be added.

Response: We thank the reviewer for this helpful comment. We have added this reference in the revised manuscript.

Line 222. Change "could be" with "could derive".

Response: We thank the reviewer for this suggestion. The text has been revised accordingly.

Lines 266-269. The second part of this sentence, about how the proxy has been improved, is not clear. Maybe "volcanic activity for a global" should be "volcanic activity as a global"? Not sure.

Response: We thank the reviewer for this suggestion. The subduction flux proxy established by Hounslow et al. (2018) is based on the age distribution of detrital zircons from arc environments. In contrast, the global subduction flux proxy proposed by Besse and Gallet (2025) incorporate only subduction-related volcanic frequency. The late one is related to subduction directly and represent the global subduction better. We also replaced "for" by "as".

Line 271-272. What does exactly the statement "to induce enough CMB thermal condition changes" mean? This is a key point to understand Figure 3 and the rest of the main text. I believe that this planetary-scale process (in what way a lower subduction flux affects the reversals rate) should be described more clearly.

Response: We thank the reviewer for highlighting the need to clarify this important point. In the revised manuscript, we have expanded the description of the physical mechanism linking subduction flux, core-mantle boundary (CMB) heat flow, and geomagnetic field behavior. Specifically, the heat flow from the core to the mantle is controlled by the thickness and thermal structure of the lowermost mantle thermal boundary layer (TBL). When subducted slabs descend or displace material ahead to the base of the mantle, they introduce relatively cold material into this region, which locally thins the TBL and enhances the temperature gradient across the CMB. This process leads to an ***increase in heat flux from the core into the mantle****. Enhanced CMB heat flow promotes more vigorous convection in the outer core, which in turn influences geodynamo behavior. Numerical dynamo simulations suggest that higher heat flux can lead to* ***increased reversal frequency and reduced dipole moment****, whereas lower heat flux may favor more stable field configurations, including superchrons. Therefore, temporal variations in global subduction flux can modulate the thermal structure at the base of the mantle, indirectly affecting geomagnetic field intensity and reversal frequency through changes in core convection. We have revised the manuscript to clarify this mechanism and its relevance to Fig. 5.*

Line 298. Add country after Rochester.

Response: We thank the reviewer for this suggestion. City and country have been added.

Figure 1. It is somehow awkward to call panels not in the right (a, b, c, …) order. Just change the letters in the panels? Furthermore, acronyms (SEM, EDS) should be spelled out. The same applies for all the figures.

Response: We thank the reviewer for this helpful suggestion. In the revised manuscript, we have reorganized the panel labels to follow a consistent (a, b, c, ...) sequence for improved clarity. We have also revised the figure captions to spell out all acronyms at first occurrence. The same changes have been applied consistently across all figures in the manuscript.

Figure 2. The insets are clear enough, I do not think it is necessary to specify the position, so you can shorten a bit the caption. I would use “red” instead of “color”, also because black and blue of the horizontal projections are virtually undistinguishable (maybe use green?). *Response: We thank the reviewer for this helpful suggestion. We have shorten the caption of this figure by removing unnecessary descriptions of inset positions. We have also adjusted the color scheme of the horizontal projections to improve visual distinction (e.g., replacing blue with green for the temperature ranges used in the paleointensity fit), ensuring that all elements are clearly distinguishable. Similar modifications have also been applied to Arai plots in Supplementary Fig. 9 and Fig 10.*

Figure 3. Very long intervals the blue belt seems very speculative, for examples between 160 and 260 Ma, and beyond 300 Ma, where similar data are missing. Maybe skip that final part of the graph, or use a lighter color? Put a semi comma in line 362, between “areas” and “reversals”. Last thing: it might be my limitation, but I do not understand what the red line in panel c represents. *Response: We thank the reviewer for these helpful suggestions.* ***(1) Blue shaded belt:*** *We agree that the blue shaded belt becomes increasingly uncertain in time intervals with sparse high-quality SCP data (e.g., 160-260 Ma and >300 Ma). We clarify that this envelope reflects the median time-varying dipole moment derived from the Monte Carlo Axial Dipole Average Model (MCADAM.1b; Bono et al., 2022), which is largely based on bulk-rock paleointensity data in these intervals. This also highlights the need for additional high-quality SCP data to better constrain long-term geomagnetic field strength.* ***(2) Punctuation:*** *We have corrected the punctuation by inserting a semicolon between “areas” and “reversals” .* ***(3) Red line in panel (c):*** *We apologize for the lack of clarity. The red line represents the subduction-related volcanism frequency after removal of a long-term exponential trend and normalization relative to its minimum value (Besse and Gallet, 2025). Although the geological significance of the long-term exponential trend remains uncertain, it is not critical to analyze dataset of subduction-related volcanism frequency.*

Supplementary text 2. I understand it is not the subject of this study, but some concept can be clarified. The 45° cutoff refers to the virtual geomagnetic poles? The most recent works on paleosecular variations (Tauxe et al., 2024) strongly suggest checking the consistency of paleomagnetic directions with a paleosecular variations model avoiding the use of any cut-off, before using the directions themselves for paleogeography purposes. I am sure that this would not affect the presented result, but it might be worth mentioning such works, or maybe even try to test the directions dataset, even if already published.

Response: We thank the reviewer for this insightful comment. The 45° cutoff used in Supplementary Text 2 refers to the angular distance of virtual geomagnetic poles from the mean pole, following conventional paleomagnetic data selection criteria. We have revised the caption to clarify this point explicitly. We agree that recent studies advocate evaluating paleomagnetic datasets using paleosecular variation models rather than applying fixed cutoff criteria (Tauxe et al., 2024). In the revised manuscript, we have adopted the paleolatitude of 2.6°N (Fig. 2f) derived from our new paleomagnetic results from the gabbro for the calculation of PDM, rather than relying on previously published lava-flow data from the Binchuan area. Although the Binchuan dataset passes reversal and fold tests (Xu et al., 2022), it is derived from 11 fault-separated sections, and the declination may be affected by local vertical-axis rotations. That might be the reason for elongated distribution of the ChRMs and exclusion of several sites by 45° cutoff. We note that our conclusions do not depend sensitively on the details of directional filtering of the Binchuan dataset. Given the scope of this study, we have therefore not reprocessed these data using PSV-based methods. However, we agree that such approaches represent an important direction for future work. We thank the reviewer for helping us improve the clarity and context of this aspect of the study.

Supplementary figure 3. The caption should explain the diagram better. I suppose panel c is the projection of the virtual geomagnetic poles (VGPs) onto an equal area diagram. I assume that the VGPs are rotated to be centered onto the vertical of the diagram (this should be specified). Why is the distribution so elongated? Even without test, I am sure this would not be consistent with any of the published paleosecular variation models (e.g., CP88, TK03.GAD, THG24), making the paleolatitude calculation suspicious (although both positive and negative paleomagnetic directions inclinations should anyway indicate a peri-equatorial acquisition of the remanence). Why is that? The first reason that comes into my mind is a strong anisotropy of magnetic remanence. This could have implications in your absolute paleointensity calculation, and it should be addressed (see main comment to the MS).

We thank the reviewer for this important and insightful comment. ***(1) Clarification of Supplementary Fig. 3:*** *We have revised the caption to clearly describe each panel. In particular, panel (b) now explicitly shows data filtering using a 45° cutoff based on the angular distance of virtual geomagnetic poles (VGPs) from the mean pole.* ***(2) Elongated VGP distribution:*** *We acknowledge that the Binchuan dataset shows an elongated distribution that may not conform to paleosecular variation (PSV) models. These data are from previously published Emeishan basaltic lava flows (Xu et al., 2022), sampled from multiple fault-separated sections, and the elongation is likely due to variable local vertical-axis rotations. In contrast, our new gabbro data show stable ChRM directions without such elongation (Fig. 2f), supporting the reliability of the paleolatitude used here.* ***(3) Anisotropy and paleointensity:*** *We have evaluated magnetic anisotropy through new hysteresis measurements on single plagioclase crystals along multiple orientations. The results show only minor variation, indicating weak anisotropy (Supplementary Fig. 7; Table 4). Together with the random orientation of nanoscale inclusions, this suggests negligible impact on paleointensity estimates.* ***(4) PSV considerations:*** *Although the Binchuan VGP distribution may not satisfy PSV models, these data are not central to our paleointensity analysis, which is based on the gabbro samples. We have revised the figure caption and text accordingly.*

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Response to Comments

Reviewer #3

Comment on the manuscript titled "Late Permian core-surface-magnetosphere conditions revealed by single crystal paleointensities from the Emeishan large igneous province", authored by Wentao Huang and coauthors.

first comment: the manuscript could benefit from a further round of deep reading, as I found some typos or wrong verbs here and there (below). I believe that this Manuscript represent a valuable contribution to the knowledge of a complex time of the Earth's history, at least excluding a possible cause (or at least contributing cause) to the Late Permian mass extinction. Having said that, I would like to suggest some improvement, especially for the paleomagnetic analyses. They have been performed (in terms of directions isolation and cluster analysis) ignoring some of the cutting-edge quality check and statistical approaches.

Response: *We thank the reviewer for the positive assessment of our study and for recognizing its contribution to understanding the Late Permian Earth system. We also appreciate the suggestions regarding the application of more recent paleomagnetic quality-control and statistical approaches. Our paleomagnetic directional analyses were included primarily to evaluate the origin and timing of remanence acquisition in the sampled rocks and to provide independent support for the primary nature of the magnetic carriers used in the single-crystal paleointensity experiments. The principal conclusions of the manuscript are based on the paleointensity results rather than on paleomagnetic directional interpretations. Nevertheless, we agree that the directional data should be analyzed using the most appropriate statistical tools available. We have therefore carefully considered the reviewer's suggestions and provide detailed responses below. Where appropriate, we have incorporated additional analyses and clarifications into the revised manuscript. Importantly, these additional analyses do not alter the interpretation that the remanence is primary or any of the conclusions regarding Late Permian geomagnetic field strength.*

Please find my main comments below:

Figure 2 & paleomagnetic part

I am aware that this is not meant to be a pure paleomagnetic work, but I have a few concerns that should be addressed. Are the ChRM direction anchored to the origin of the demagnetization axis or not? If yes, did you apply the approach of Heslop and Roberts (2016)? Anchored directions tend to have an artificially low MAD, and this method provides a metric for assessing if directions can be anchored or not.

Response: We thank the reviewer for this helpful suggestion. In the original submission, ChRM directions were determined using anchored principal component analysis (PCA). Following the reviewer's recommendation, we have reanalyzed all stepwise demagnetization data using the approach of Heslop and Roberts (2016) to objectively assess whether anchored or unanchored PCA fits are appropriate for each specimen. Specifically, the posterior probability criterion proposed by Heslop and Roberts (2016) was used to determine whether anchoring should be applied. As a result, unanchored fits were adopted for the majority of specimens, whereas constrained anchored fits were used only where justified. Consistent with the findings of Heslop and Roberts (2016), the corresponding MAD values of the anchored fits increased relative to those obtained using conventional anchored PCA.

All individual ChRM directions were recalculated, together with the site-mean directions and the overall mean direction (Supplementary Tables 5, 6). The recalculated ChRM directions differ only slightly from the original values for the vast majority of specimens. Consequently, the overall site-mean direction changes only from Dec./Inc. = 350.0°/5.1° to 350.3°/6.9° in stratigraphic coordinates (Supplementary Table 6), corresponding to a change in paleolatitude from 2.6°N to 3.4°N (Fig. 2f). The resulting paleomagnetic dipole moment (PDM) is essentially unchanged, increasing only from 4.08×10^{22} *to* 4.09×10^{22} *A m². Thus, although the revised analysis provides a more rigorous statistical treatment of ChRM determination, it has no material effect on the paleomagnetic interpretation or any of the conclusions of the study. The manuscript has been revised accordingly. The Methods section now describes the procedure of Heslop and Roberts (2016), Supplementary Tables 5, 6 report the fitting approach used for each specimen and site, and all figures and statistics have been updated using the revised ChRM directions.*

Why did you use an inclination only fold-test? Your data have both declination and inclination, so the classic fold test (e.g., Enkin,2003; Tauxe & Watsons, 1994).

Response: *We applied the inclination-only fold test to the characteristic remanent magnetizations (ChRMs) of the Emeishan Large Igneous Province (ELIP) obtained from the Baima layered* *gabbro (eight sites; this study), the Panzhihua layered gabbro (one site; ~40 km from Baima)(Krivolutskaya et al., 2016), and the Binchuan basaltic lavas (99 sites; ~230 km away)(Xu et al., 2022). As shown in Fig. 2e, f and Supplementary Fig. 3a, b, the ChRMs from the Panzhihua layered gabbro and Binchuan basaltic lavas exhibit declinations that are distinctly different from those of the Baima layered gabbro. These differences were most likely caused by differential post-emplacement vertical-axis rotations experienced by the various exposures. Under such circumstances, a conventional fold test that combines declination and inclination would be strongly influenced by these secondary rotations and therefore would not provide a meaningful assessment of the timing of ChRM acquisition. In contrast, the inclination-only fold test is insensitive to vertical-axis rotations and can provide a more robust constraint on whether magnetization was acquired before or after tilting. We therefore consider the inclination-only fold test to be the most appropriate approach for evaluating the timing of remanence acquisition in these geographically separated ELIP exposures.*

The 'equal area' diagrams have a strangely up-down elongated confidence ellipse. Is that a Fisher (1953) confidence? Or maybe diagrams are not 'equal area'? Low inclination Fisher circles are much more horizontally elongated. See for example the attached equal area plot of your Figure 2e using another plotting software.

Response: *We thank the reviewer for raising this point. As described in the Methods section, the paleomagnetic data were analyzed using the online program Paleomagnetism.org (Koymans et al., 2016). In this software, the default confidence ellipses are reported as ΔD for declination and ΔI for inclination following Butler (1992), which are derived from Fisher statistics (Fisher, 1953) applied to virtual geomagnetic poles (VGPs) rather than to the directions themselves. Indeed, Fisher statistics (with the k precision parameter and α_{95} confidence ellipse) should be applied to VGPs, not to directions. That is because VGPs are distributed circularly symmetric, while directions are not. Furthermore, Deenen et al. (2011) demonstrated that α_{95} is not an appropriate measure of dispersion when directional scatter is dominated by paleosecular variation. Accordingly, the confidence ellipses shown in Fig. 2e, f and Supplementary Fig. 3a-f are based on A_{95} pole statistics rather than α_{95} directional statistics. This distinction explains why the confidence ellipses appear elongated in the vertical direction and differ from the more horizontally elongated confidence regions expected for low-inclination directional data plotted using conventional Fisher*

α_{95} statistics. To avoid confusion, we have clarified the nature of the confidence ellipses in the figure captions of Fig. 2 and Supplementary Fig. 3.

Does the equatorial paleolatitude fit with paleogeographic reconstructions (i.e., coeval paleomagnetic data from elsewhere) for the Late Permian?

Response: *We thank the reviewer for this question. Yes, the near-equatorial paleolatitude inferred from our paleomagnetic data is consistent with existing paleogeographic reconstructions and published paleomagnetic results for the Yangtze Block during the Late Permian. Previous studies have consistently placed the Emeishan Large Igneous Province and the Yangtze Block at low latitudes, close to the equator, throughout the Late Permian (Enkin et al., 1992; Tang et al., 2024; Xu et al., 2022). Our paleolatitude estimate therefore agrees well with these independent reconstructions and supports the interpretation that the characteristic remanent magnetization is primary. To clarify this point, we have added a statement and appropriate references in the revised manuscript noting that the inferred paleolatitude is consistent with coeval paleomagnetic constraints and paleogeographic reconstructions for the Yangtze Block.*

As a good practice (FAIR data), it would be nice to provide a repository where original raw demagnetization data can be extracted and plotted again. Personally, I struggle with the MagIC database but it seems to me the only the paleointensity data have been uploaded. In any case, the choice to share the raw demag data is up to the Authors, as there is no obligation in that sense.

Response: *We thank the reviewer for this suggestion and agree that data accessibility is an important aspect of reproducible research. The paleointensity data underlying the principal conclusions of this study have been deposited in the MagIC database and are publicly available. The paleomagnetic directional dataset presented here forms part of a broader, ongoing investigation into the paleogeographic evolution of the Emeishan Large Igneous Province, involving multiple stratigraphic sections and localities that are currently undergoing further analysis. Preliminary results indicate a consistent near-equatorial paleolatitude for the ELIP across these sections, while also revealing significant differences in vertical-axis rotation among individual localities. These observations form the basis of an independent paleogeographic study that is still in preparation. For this reason, we prefer not to release the complete raw demagnetization dataset at this stage. Nevertheless, all individual-specimen directions, site-mean* *directions, fold-test results, and associated statistical parameters necessary for independent evaluation and paleogeographic reconstruction are provided in the manuscript and Supplementary Information. We therefore consider the data presented here sufficient for readers to assess and utilize the paleomagnetic results. The complete raw demagnetization dataset will be made publicly available upon publication of our ongoing paleogeographic study.*

In the rebuttal letter it is stated that test for consistency with GGP models like the THG24 (Tauxe et al., 2025) were arbitrarily avoided. Since the scope of the research is not paleomagnetic in nature, and anyway quasi-horizontal directions are hardly affected by distortion that would change significantly the average significantly, I leave this choice to the Authors. But someone might want to use your paleomagnetic data for different (e.g. paleogeographic) analysis in the future, so it would be useful to have them accessible.

Response: *We thank the reviewer for this comment and for recognizing that the primary objective of this study is to establish robust Late Permian paleointensity constraints. As noted in our previous response, we did not apply consistency tests based on published GGP models (e.g., CP88, TK03.GAD, and THG24). These models assume that the observed directional or VGP scatter is primarily geomagnetic in origin. However, the elongated VGP distribution reported by Xu et al. (2022) from the Binchuan area is derived from multiple fault-separated volcanic sections that likely experienced differing amounts of post-emplacement vertical-axis rotation. The observed elongation therefore contains a significant tectonic component, rendering comparison with GGP models inappropriate. Our use of the Binchuan dataset is limited to the inclination-only fold test, which is insensitive to vertical-axis rotations and remains suitable for assessing the timing of remanence acquisition. We therefore consider GGP-based consistency tests unnecessary for the purposes of this study.*

The directional dataset forms part of an ongoing paleogeographic investigation of the Emeishan Large Igneous Province. Nevertheless, all processed individual-specimen directions, site-mean statistics, and fold-test results necessary to evaluate the conclusions of this study are provided in the manuscript and Supplementary Information, these are sufficient for readers to utilize for paleogeographic analysis. The raw directional dataset will be made publicly available upon publication of our ongoing paleogeographic study.

Final figure

The 85 Myr delay shown in figure 5. Is it assumed just "visually" in this work or is there any geophysical (e.g. subduction simulation) evidence? That would be important to support the connection between reversal frequency and slab subduction.

Response: *We thank the reviewer for this insightful comment. We agree that the ~85 Myr delay is not independently constrained by our study. The delay was adopted from the recent work of Besse and Gallet (2025), who examined the temporal relationship between global subduction flux and Phanerozoic geomagnetic superchrons. Based on the apparent correspondence between minima in global subduction flux and the Kiaman and Cretaceous Normal Superchrons, they proposed an approximate lag of ~85 Myr between changes in subduction flux and the geomagnetic response. However, as also acknowledged by Besse and Gallet (2025), this interpretation remains uncertain,. Although there is broad agreement that subduction influences the geodynamo through its effects on CMB heat flux, current numerical geodynamo models disagree on whether geomagnetic superchrons correspond to maxima or minima in CMB heat flux, or whether the spatial distribution of heat flux is the primary controlling factor.*

To avoid overstating this still-debated interpretation, we have removed the panel comparing subduction flux and geomagnetic behavior from Fig. 5 and toned down the corresponding discussion in the main text. The revised manuscript now presents a more balanced assessment of the current understanding, emphasizing that our new paleointensity result provides an important observational constraint for future geodynamic and geodynamo models investigating the links between mantle evolution, CMB heat flux, and geomagnetic behavior, rather than direct evidence for a specific subduction-controlled mechanism or a fixed ~85 Myr time lag.

Reference n. 30 of the SI seems incomplete, there is no year, no DOI, and I could not find it online.

Response: *This is published in 2016 in a Russian Conference (Proceedings of the 11th Intl School and Conference "Problems of Geocosmos") without a DOI. We have updated this reference as follows: Krivolutskaya, N. et al. Correlation of intrusive and volcanic rocks in Emeishan Large Igneous Province by geochemical and paleomagnetic data. Proceedings of the 11th Intl School and Conference "Problems of Geocosmos", 45-53 (2016).*

Some mistakes through the main text

Line 25: wrong verb, should be ‘is’.

Response: *Improved.*

Line 26: add ‘the’ before ‘ca.’

Response: *Improved.*

Line 31: should be ‘gabbors’?

Response: *Improved.*

Line 46: change ‘kill’ with ‘trigger’

Response: *Improved.*

Line 50: check ‘deletion’

Response: *Replaced by ‘depletion’.*

Line 65: change ‘the’ with ‘a’

Response: *Improved.*

Line 137-138: that is debatable. The cooling curve of both panels a and b (more evident in a) shows a similar hump at the same temperature.

Response: We thank the reviewer for this observation. We agree that a similar feature can be recognized in both heating and cooling curves. To avoid overstating the difference between the two, we have revised the text to read: *“Such a drop is less pronounced during subsequent cooling (Supplementary Fig. 4b), suggesting partial alteration of this phase during experimental heating.”* This wording more accurately reflects the observed behavior of the thermomagnetic curves.

Line 377: ‘by using’, or just ‘using’ instead of ‘by’

Response: *Improved.*

Line 408: I believe the company is called only AGICO, and Geofyzika is the former name.

Response: *Improved.*

Reviewer #4:

This paper addresses a topic of great general interest: What caused the massive extinctions at the end of the Permian? One idea that was floated recently by Zhang et al., 2022 was that an extremely weak geomagnetic field was the culprit. This paper calls into question the reliability of those data and having looked at them myself, I agree that they are of highly questionable reliability. Then this paper introduces new data of exceptional quality from single crystals carefully extracted from two units dated at the End-Guadalupian and End Permian extinction events, just after the Permo-Carboniferous Reversed Polarity Superchron.

Response: *We thank the reviewer for this positive assessment of our study and for agreeing that the previously published Zhang et al. (2025) paleointensity data are problematic. We are encouraged that the reviewer recognizes both the significance of the scientific question and the exceptional quality of our single-crystal paleointensity dataset. As noted by the reviewer, our study provides an independent evaluation of the recently proposed hypothesis that an exceptionally weak geomagnetic field contributed to the Late Permian biotic crisis and demonstrates that the available evidence does not support such a scenario. We would also like to clarify one point. The new paleointensity results reported in this study are derived exclusively from the End-Guadalupian Emeishan intrusions. We do not present new paleointensity data from an End-Permian unit.*

The two new results are equivalent to about half the present field, or the same as the Cenozoic long term average (see Tauxe, 2006). While these authors seem to discount results from that study by not even citing it, I do think it is worth mentioning.

Response: *We thank the reviewer for this suggestion. We agree that Tauxe (2006) provides an important compilation of Phanerozoic paleointensity data and offers useful context for interpreting our results. We have therefore added a citation to this study and briefly discuss it in the revised manuscript. At the same time, we note that the reliability of paleointensity estimates derived from submarine basaltic glass has been debated in the literature, particularly with respect to the origin and stability of the remanence carrier (Tarduno et al., 2006).*

There are, after all, a number of assumptions involved in the single crystal approach. 1) What was the cooling rate and what is an appropriate cooling rate correction - 10% is pulled out of thin air to justify this.

***Response**: We thank the reviewer for raising this point. We would like to clarify that we did not apply a 10% cooling rate correction. Instead, we estimated a cooling rate correction factor of 1.5 following the approach outlined in Tauxe (2010). This estimate is based on the inferred cooling history of the Baima layered gabbro. The plagioclase crystals analyzed in this study are comparable in size to those from the Bushveld pyroxenite and the Passo da Fabiana gabbro that we previously investigated (Huang et al., 2024), for which cooling times through the relevant unblocking temperature interval have been estimated. Based on these analogues, we infer a cooling time of approximately ~100 kyr through the unblocking temperature range. Using this cooling history, an average unblocking temperature of ~437 °C (Supplementary Table 8), and the formulation described by Tauxe (2010), a cooling rate correction factor of approximately 1.5 is obtained. The basis for this estimate is described in Supplementary Text 4 ("Paleointensity cooling rate correction"). To further clarify the procedure, we have now added a Supplementary Fig. 12 illustrating determination of the cooling rate correction factor.*

2) The authors claim that each sample represents an average field. This makes more sense and there is additional justification for that.

***Response:** We thank the reviewer for this positive comment and are pleased that the additional discussion has clarified the basis for interpreting the paleointensity estimates as time-averaged field values. As discussed in the revised manuscript, the slowly cooled intrusive setting and the broad range of unblocking temperatures recorded by the magnetic inclusions indicate that remanence acquisition occurred over an extended period, resulting in a time-averaged record of the geomagnetic field, rather than a snapshot of short-term field behavior, as is typically recorded by individual lava flows.*

Also, 3) The authors claim that no anisotropy correction is necessary based on the IRM results. IRM will show the least amount of anisotropy and is in no way equivalent to a thermal remanence. This has been the argument since the first paper on single crystals and I am still not buying it.

Response: *We thank the reviewer for this comment and fully agree that anisotropy of IRM is not equivalent to anisotropy of TRM. Indeed, IRM anisotropy can be influenced by factors such as the shape of the measured plagioclase grain and the application of magnetic fields (up to 1 T) that are much stronger than the ambient geomagnetic field. For these reasons, we do not use the IRM measurements as a direct substitute for a TRM anisotropy correction. The IRM anisotropy measurements were included (Supplementary Fig. 7) in response to a suggestion from a reviewer (#2) in the previous round and were intended only as a first-order assessment of potential magnetic fabric. We measured hysteresis parameters in nine positions and found negligible anisotropy. This observation is consistent with SEM images showing that the needle-shaped Fe-Ti oxide inclusions occur in multiple orientations within the plagioclase crystals rather than exhibiting a strong preferred alignment.*

More importantly, our decision not to apply an anisotropy correction is based primarily on previous studies of plagioclase-hosted single-crystal paleointensity recorders from layered mafic intrusions similar to those investigated here. These studies demonstrate that pronounced magnetic fabric is absent and that any anisotropy of natural TRM, if present, has a negligible effect on paleointensity estimates (Bono et al., 2019; Huang et al., 2024; Zhou et al., 2022). This conclusion is also supported by hysteresis measurements and micromagnetic simulations reported in the literature (Nikolaisen et al., 2022; Tarduno et al., 2006). We have clarified this point in the manuscript to avoid implying that the IRM measurements alone justify the absence of an anisotropy correction.

Also, if magnetite is the dominant carrier, why are the blocking temperatures in the mid-400s? I find that a bit strange.

Response: *We thank the reviewer for this helpful comment. This question was also raised during the previous round of review, prompting us to conduct a comprehensive analysis of the EDS data obtained from the Fe-Ti oxide inclusions hosted within plagioclase crystals.*

Our results indicate that the magnetic inclusions are not composed exclusively of pure magnetite, but rather of magnetite and titanomagnetite with variable Ti substitution (Fig. 3e, f; Supplementary Fig. 8d; Supplementary Table 7). We performed 257 EDS spot analyses on needle-

like Fe-Ti oxide inclusions from nine representative plagioclase crystals. The measured Ti/Fe atomic ratios correspond to x values of 0-2.2 in $Fe_{3-x}Ti_xO_4$. Of these analyses, 87 correspond to pure magnetite (x = 0), 56 to low-Ti titanomagnetite (x = 0-0.2), and 193 have x ≤ 0.8. This latter group, which is expected to dominate the remanence, has a mean composition of x = 0.14, corresponding to a Curie temperature of approximately 490 °C based on the calibration of Lattard et al. (2006). This estimated Curie temperature closely matches the upper limit of the reliable paleointensity fitting range (up to 488 °C; Supplementary Fig. 10d), demonstrating that the observed unblocking-temperature spectrum is entirely consistent with the measured titanomagnetite compositions. In contrast, the more Ti-rich inclusions (x > 0.8) are expected to have much lower Curie and unblocking temperatures and therefore are unlikely to contribute significantly to the natural remanent magnetization.

We have provided these EDS data in Supplementary Table 7 and the corresponding discussion in the main text. Together, the mineralogical, geochemical, and rock-magnetic observations demonstrate that the predominance of unblocking temperatures below ~500 °C is an expected consequence of the titanomagnetite compositions present in the inclusions.

Also, the argument using the Pullaiah Plot should be reconsidered in light of new results of Cych et al. (2025, 2026) using micromagnetic modelling. That shows that the traditional Neel theory based nomogram needs to be re-examined.

Response: *We thank the reviewer for drawing our attention to the recent work of Cych et al. (2025) and (Cych et al., 2026). We agree that these studies provide important new insights into the thermoviscous behavior of single-domain particles and suggest that some assumptions underlying the classical Néel-Pullaiah framework may warrant further investigation. However, in this study the Pullaiah nomogram is used only as a first-order independent test of whether the primary TRM recorded by the plagioclase-hosted magnetic inclusions could have survived the independently constrained thermal history of the Baima intrusion. Even the simulations of Cych et al. (2025) produce Pullaiah curves that are broadly similar in shape to the original curves of Pullaiah et al. (1975), although the predicted relaxation times differ somewhat owing to the inclusion of magnetocrystalline anisotropy. We therefore consider the classical framework appropriate for this purpose. Importantly, our conclusion that thermal resetting of the primary remanence is unlikely* *is not sensitive to moderate variations in the estimated relaxation times. We have added a brief note and citation to Cych et al. (2025, 2026) in the caption of Supplementary Fig. 11.*

So, while I strongly agree that the results address a topic of great interest and that the technical quality of the results is excellent, I question whether Nature Communication is the appropriate venue.

Response: *We thank the reviewer for recognizing both the broad scientific importance of the question addressed in this study and the high technical quality of our new paleointensity dataset. We respectfully submit that this work is of broad interest because it addresses fundamental questions concerning the evolution of Earth's geodynamo and its potential links to large-scale environmental and biological change.*

Reliable Paleozoic paleointensity estimates remain exceptionally scarce, particularly those derived from single-domain magnetic recorders using the single-crystal approach. Our study provides only the fifth robust single-crystal paleointensity determination spanning the past 400 million years and fills a critical gap in the paleointensity record immediately following the termination of the Permo-Carboniferous Reversed Superchron. This interval encompasses two of the largest magmatic events in Earth history - the emplacement of the Emeishan and Siberian Large Igneous Provinces - as well as the end-Guadalupian and end-Permian mass extinctions, yet robust paleointensity constraints for this period have remained extremely limited.

Our new ~260 Ma paleointensity estimate from the Emeishan Large Igneous Province therefore provides a much-needed benchmark for evaluating the state of the geodynamo during this pivotal interval in Earth history. The results demonstrate that geomagnetic field strength decreased substantially following the termination of the Permo-Carboniferous Reversed Superchron but did not collapse to an exceptionally weak state. Consequently, our study places important new constraints on the long-term evolution of the geodynamo and provides an independent test of recently proposed hypotheses linking a weakened geomagnetic field to Late Permian environmental deterioration and mass extinction. We therefore believe that the implications of this work extend well beyond the paleomagnetic community and are of broad interest to researchers *studying Earth's deep interior, surface environment, and biosphere, making it well suited for the broad readership of Nature Communications.*

minor comments:

line 25: The Late Permian are => The Late Permian is

Response: *Improved.*

line 188: "continuous range of Ti/Fe atomic from" - is there something missing here?

Response: *Improved, we add 'ratios' after 'atomic'.*

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Response to Comments

Reviewer #3

The authors have addressed my comments throughout the manuscript. I have only two minor requests, after which I recommend accepting the manuscript for publication without further revision.

Response: *We sincerely thank the reviewer for the careful evaluation of our revised manuscript and for the positive assessment. We greatly appreciate the reviewer's constructive comments throughout the whole review process. We have carefully addressed the two remaining minor requests, as detailed below.*

PPCA Analysis (Supplementary table 5)

In Heslop & Roberts (2016), two probabilistic parameters are used to select between the Unanchored, Anchored, and Constrained interpolation approaches. The Constrained interpolation method was introduced in that same paper, and the choice between models is based on the parameter p(Hc|D) rather than p(Ha|D). In Supplementary Table 5, the listed interpolation methods are either Unanchored or Constrained. My concern is that the term Constrained may have been used in place of Anchored. If this is the case, please revise the "PCA model" column accordingly. Alternatively, if the interpolation method is indeed the Constrained approach of Heslop & Roberts (2016), please also report the corresponding p(Hc|D) values to justify the model selection.

Response: *We have now added both $p(H_C|D)$ and $p(H_A|D)$ values to Supplementary Table 5 and updated the mean ChRM directions accordingly in Fig. 2e, f and Supplementary Table 6.*

Mean paleomagnetic directions and confidence.

The choice to use confidence ellipses following the approach of Deenen et al. (2011) is legit. However, the description provided in both the text and figures is currently insufficient. Stating that the ellipses are "based on A95 pole Fisher statistics" does not clearly describe the methodology. It should be explicitly stated that the confidence ellipses were calculated following the procedure of Deenen et al. (2011), and the paper should be cited. Furthermore, this method yields two independent confidence parameters, namely the uncertainty in declination (ΔDec) and the uncertainty in inclination (ΔInc). Both values should be reported, rather than only the A95 confidence angle around the virtual geomagnetic pole. I am not familiar enough with

Paleomagnetism.org to know whether these parameters can be exported directly. If not, other freely available alternatives perform the same calculations and provide exportable results. One is Magnetic-A (Dallanave, 2026; https://doi.org/10.1029/2025EA004863), which also includes implementation of the Heslop & Roberts (2016) PCA model analysis with exportable text outputs.

Response: *We have revised the text and figure captions to state explicitly that the confidence ellipses were calculated following Deenen et al. (2011) and have added the corresponding citation. We now report both the declination uncertainty (ΔDx) and inclination uncertainty (ΔIx) in Fig. 2e, f and Supplementary Table 6.*

Reviewer #4:

The authors have carefully considered the comments by reviewers #3 and #4. I think the manuscript in its revised state merits publication in Nature Communications and hope future work will expand the number and quality of paleointensity results from this very interesting time period. This is a good start.

Lisa Tauxe

Response: *We sincerely thank Prof. Tauxe for the positive assessment of our revised manuscript and for the thoughtful and constructive comments throughout the review process. We greatly appreciate the reviewer's recognition of the value of our study, and we agree that further high-quality paleointensity data from the Late Permian will be important for better constraining geomagnetic field behavior during this critical interval.*