From Disorder to Order: Inheritance of Magnetic Remanence in Tetrataenite Bearing Meteorites from Multi-Phase Micromagnetic Modeling

Response to Reviewers 2

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We are resubmitting the revised manuscript entitled “From Disorder to Order: Inheritance of Magnetic Remanence in Tetrataenite Bearing Meteorites from Multi-Phase Micromagnetic Modeling”. We would like to thank both referees for revising our manuscript and for their valuable comments. We have addressed our responses to their comments below and we believe that their criticisms were constructive and led to better readability and overall quality of the manuscript.

**Reviewer 1**

I have reviewed the revised manuscript and am happy to accept the revisions. Thanks to the authors for engaging in the discussion and constructive response to the points raised in the initial review. The changes have made a significant improvement. Apologies that it has taken me some time to get round to looking at it in detail.

A: Thank you for taking the time to review the revised manuscript and for your positive feedback. We greatly appreciate your careful consideration of our work and your constructive comments. Your insights have certainly contributed to improving the overall quality of the paper.

**Reviewer 2**

Thank you for making so many changes to the manuscript to account for my comments - it is really appreciated to so my thoughts taken onboard so thoroughly and widespread throughout the paper. I still have two remaining comments that I think would strengthen the manuscript if they are addressed:

1. The first comment relates to comment 7 from reviewer 2 in the rebuttal. The authors found a nice example of where short timescales of ~1-3 Myr are relevant to asteroid evolution (Zhang and Bercovici, 2023), which was great to see. I just have a few outstanding concerns with some of the wording. For instance, on line 79 of the revised version of the text, the authors say 'lapses of ~10^5 years might encompass \*completely distinct\* stages of their magnetic history'. Can the authors provide an example of this? I would argue that excursions and reversals are not completely distinct. Moreover, on lines 264-265, the authors state this 10^5 year period could correspond to ~15 – 50 % of the dynamo period - this percentages require justification.

A: As discussed in the 1st rebuttal (reply to comment #7), in Bryson et al. (2017) the authors reported a 200 kyr magnetic history recovered from the Steinbach and São João Nepomuceno IVAs; within this time-resolved record, the authors identified a period of relatively intense magnetic field (~100 μT), followed by a possible reversal over a period of ~30 kyr. These two distinct events observed by the authors occurs within the time difference (~105 years) created by magnetic inheritance; notably, 105 years corresponds to ~50 % of the total dynamo history reported by Bryson et al. (2019). If we consider now the longest dynamo lifetime proposed by Zhang & Bercovici (2023) (~3 Myr), then 105 years would correspond to ≲4 % of the total IVA dynamo history. Even though such “long”-lived dynamo history is considered somehow unlike to occur according to Zhang & Bercovici (2023), we decided to adjust the lower limit of our percentage range to 4%. We should say, however, that the main purpose of our statement is to suggest that a robust interpretation of the short-lived magnetic history of fast-cooled bodies should incorporate our observation of magnetic inheritance through tetrataenite ordering, and that the impact of such inherited magnetic signature is to push back the timing of paleomagnetic record; most importantly, based on our current understanding of dynamo history of the IVA’s parent body, such temporal discrepancy created by inheritance can corresponds to up to 50 % of the total dynamo lifetime (depending on the cooling rate of meteoritic metal, see L. 257–261).

1. Finally, if a recovered age was adjusted by 10^5 years, what would this mean for the properties of the parent body. For instance, if we found the IVA parent body was generating a field at 2.9 Myr after CAI formation rather than 3 Myr, how different would the size or accretion age of the parent body have to be? I think this would really help to contextualize the findings of this study.

A: Thank you for bringing up this point. Our study primarily aimed to establish a robust physical framework for understanding the recording of stable magnetization states in CZs. This framework is important for developing a quantitative theory that can effectively link observed magnetizations in CZs with ancient dynamo fields and planetary processes. Providing such theory, which is fundamental for appropriately interpreting records in CZs and their planetary implications, is beyond the scope of our work. Therefore, while our research provides a deeper understanding of how CZs record magnetization, we acknowledge that our findings cannot directly describe planetary processes. Our work represents an important step towards achieving this goal in the future.

1. The second comment relates to comment 9 from reviewer 2 in the rebuttal. I believe the authors adopt to idea that course islands rerecord on tetrataenite ordering, and there is then a narrow band in the CZ where the tetrataenite inherits a remanence, and then (presumably) there is the very fine CZ that does form as tetrataenite. The authors do not present a holistic view of the CZ like I just outlined, which makes their conclusions are little more opaque. I would recommend including a discussion of the very fine CZ (even if this is to say that it forms as tetrataenite, so can't record a remanence in the manner explored in this study), and include a schematic of the CZ showing how different regions record - I think this would help to convey their ideas and the understanding on non-experts. Finally, in Fig 7 in the revised version of the manuscript, the authors consider Ni compositions from 41 - 34 %. Why were these Ni compositions chosen? According to the low temperature Ni-Fe phase diagram, I believe islands would form as tetrataenite at a bulk composition of around ~39% Ni (Uehara et al 2011), so would the islands at Ni compositions covered by the orange lines in Fig 7 have formed as tetrataenite? On the other hand, we understand the exact position of these lines very poorly at these low temperatures - for instance, Maurel et al (2019) argue islands would form as tetrataenite closer to 35% Ni, so you may be able to use our uncertainty in the position of the spinodal region to justify considering Ni compositions as low as 34%. In the interest of completeness, I would recommend discussing the possibility that islands at a bulk composition of ~39% Ni did form as tetrataenite as well.

A: Thanks for pointing this out. We focused on Ni compositions ranging from 34 – 41 wt. Ni% because, according to the updated Fe-Ni phase diagram proposed by Maurel et al. (2019), this corresponds to the compositional range in which CZ formation is initiated at temperatures > 320oC. Moreover, we did not include the paleomagnetic importance of very fine regions in CZs for three reasons. First, if we extrapolate the curves obtained with the model of Maurel et al. (2019) shown in Fig. 6 to regions of lower Ni content – i.e., up to ~25 wt. Ni%, which corresponds to the compositional range in which CZs form (Maurel et al., 2019; Yang et al., 1997 – we observe that for all cooling rates we investigated, the islands forming within 34 – 31 wt. Ni% will be already blocked when they form (i.e., they readily form at sizes larger than tetrataenite’s blocking volume ~7 nm), so there’s no paleomagnetic record taking place within this compositional range. Second, for lower Ni regions below 30 wt. Ni%, the islands form as superparamagnetic tetrataenite and never reach their blocking volume, so again, no paleomagnetic record can be created within this size/compositional range. Lastly, assessing any paleomagnetic record from very fine regions poses significant experimental challenges; XPEEM measurements have a spatial resolution of ~10 – 20 nm, so obtaining meaningful magnetization maps from these regions is virtually impracticable with our current technology. Therefore, our goal is to indicate “measurable” regions of the CZ in which inheritance can occur and their paleomagnetic implications. For this reason, we decided not to include discussions about very fine regions as we believe they cannot provide reliable/measurable paleomagnetic records.