

Dear editor,

We thank reviewer#1 for reviewing the manuscript again. Please find our replies to reviewer#1's suggestions and questions below:

Line 31: The authors say in the abstract that there are high lock in depths. Later in the manuscript this is discussed as being at least 18 cm, but, due to high sedimentation rates, this only represents a few hundred years. What do the authors mean by high lock in depths? 18cm is within the 'usual' range of 10-30 cm that is often quoted. A few hundred year delay is not a lot (marine sediment cores can have much larger offsets due to low sedimentation rates). Maybe the authors should think about their wording in this context, especially as it is in the abstract.

Reply: Thank you for highlighting this issue. The wording is incorrect indeed. We changed the sentence into: "The low variability of the magnetic record compared to data sets of reference records with lower sedimentation rates may be due to a smoothing effect associated with the lock-in depths" to mention both, the low variability and the lock-in in the abstract.

Line 142: For transparency, I would mention how many samples of those used were anchored to the origin. This can have a large (positive) effect on MAD values, artificially lowering them.

Reply: This is of course absolutely true. We indicate now that only for 14 samples anchoring was used. In addition, we include the citation of the data depository, in which it is possible to look up which samples this concern.

Line 264: Unless it is shown elsewhere, I would remove the suggestion that the magnetic record results from soil forming processes. Soil forming processes generate SP magnetite during pedogenesis. This would be reflected in IRM/X ratios or the ARM record, which are mentioned to be stable. I would simply state that the downcore records in X, ARM, and IRM reflect detrital input.

Reply: Thank you for pointing this out. We changed the wording as suggested.

Line 348: I don't understand this sentence "the core parts were probably twisted against each other during transport along the layering". Also, if the original fabric is altered, how can only the declination be affected, but not the RPI?

Reply: We are sorry, but we may have expressed ourselves incorrectly. We mean that the core parts are rotated against each other along the Z-axis. If they were twisted, deformation could occur. This is not the case. We have changed the wording to express ourselves correctly.

Line 398: Wouldn't sediment compaction and compression also result in inclination shallowing especially at high latitudes?

Reply: Of course, sediment compaction may result in inclination shallowing. This is well known and we claim that it may be the cause for the shallower inclination at the site compared to GAD (lines 341-344). In turn, inclination shallowing may have an impact on RPI. We describe this issue in line 398-399, when we claim that a misalignment of magnetic minerals caused by compaction may be responsible for lower RPI in the lower part of the core.

Line 403: Awkward use of the term "final proof". I'd suggest another term like "additional evidence" or "further demonstrating"

Reply: Thank you for pointing this out. We changed the wording into "contribute further evidence"

Line 532: I am curious as to why the authors suggest the change in TOC in Lake Levinson-Lessing is associated with the Bølling-Allerød interstadial in the NGRIP ice core rather than the transition out of the Younger Dryas into the Holocene? It does agree better with the C14 ages in table 1, but the authors have already stated that these could be too old. As the determination of the 18cm lock in

and the lock in discussion depends on this correlation, I'd like the authors to explain why the B/A and not the termination (which would presumably have a larger and lasting effect on TOC than the transient interstadial) was used as the tie point.

Reply: The provided correlation scheme is reinforced by palynological evidence published in Lenz et al. 2022. Since the study is accepted and online available now (<https://doi.org/10.1002/jqs.3384>), we included the reference in the manuscript, and refer to this study for detailed explanations on this question.

We hope that we have now fully answered all open questions. We would very much appreciate it if this article could still be given a publication date in 2021 so that it is cited in existing works with the correct year.

Kind regards,
Stephanie Scheidt and Co-authors