

Geochronology Discuss., referee comment RC1
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Comment on gchron-2021-12

Anonymous Referee #1

Referee comment on "A 62 kyr geomagnetic palaeointensity record from the Taymyr Peninsula, Russian Arctic" by Stephanie Scheidt et al., Geochronology Discuss., <https://doi.org/10.5194/gchron-2021-12-RC1>, 2021

The manuscript by Scheidt et al. presents a paleomagnetic record from lake Levinson-Lessing over the last ~60 ka. The authors find variability in the normalized NRM intensity record to correlate with existing paleomagnetic records from the Northern Hemisphere and use this variability to establish an age model, evaluate sediment lock-in, and develop a relatively rare, long, high-latitude paleomagnetic record. In general, the manuscript is well written, and the discussion and conclusions are well supported by the data. I have some comments and raise some questions below.

Line 28: Awkward phrasing of "but although includes", maybe consider removing "although".

Line 50: Maybe provide an age associated with the Last Glacial Maximum.

Line 95: No need to abbreviate approximately as "approx."

Line 99: No need to start the sentence with "Whilst"

Line 101: How were the gaps between cores determined? This was also unclear in Scheidt et al., 2021a. The gray lines in figure 2 are not exactly every 2m, so I assume that coring proceeded using the length of recovered material? In which case how are the gaps between cores determined? If coring advanced 2m with every drive (regardless of recovery), and the recovered amount was used to estimate gaps, then the gray lines on figure 2 should be changed to be regularly spaced and reflect this. Either way, a sentence in section 3.1 would be useful to clarify this.

Also, were the 2m cores sectioned into the shorter segments (line 111) following recovery? It is unclear on line 111 whether the whole core analyses were run on the 2m cores or the shorter sections (segments)? It should be clarified when the cores were cut into the 1m sections prior to, or following the whole core analysis.

Line 140: Sentence should read, "...magnetic susceptibility, ARM, and IRM as normalizers."

Line 142: Unlike PSV, which has strong regional imprints, RPI (at least at the millennial-scale and for the features you are matching) is thought to be more of a global signal. A case is made for the coherence between the North Atlantic records and Co1401 and the age model depends on this long-distance correlation. With this in mind, I am confused as to why such a strong case is made that the RPI records should come from within ~1500km.

Line 151: These six sites should be referenced individually using their original datasets, in addition to their synthesis within the GLOPIS-75 stack. These records are also presented unreferenced in the figures, they should be properly attributed.

Lines 151-167: This information is presented as bullet points. I am unfamiliar with the journal style requirements, but generally, bullet points consisting of several sentences are refrained from in the main body of a manuscript. I will leave this decision up to the authors and editor though.

Line 211: I am slightly confused as to why RPI values were retained if PSV values were discarded. As intensity and directions are both part of the same vector, if the directional data is affected by disturbance/compression, then one would expect the intensity values to also be affected. The result might be lower NRM/normalizer ratios than expected with all else being equal.

Line 263: Suggest revising the sentence, particularly the use of "reduce", it is not entirely clear what this sentence is pointing to. I assume it is that if the low inclination values between 16-20m and those around 25m are removed from the analysis the mean and median ChRM values increase by a couple of degrees and better approach GAD?

Line 276: I hesitate to use the term RPI for the first description of the normalized intensity ratio presented in figure 2. By using the term RPI you are stating that the downcore ratio is a faithful recorder of the Earth's magnetic field, however, no exploration of this ratio has been attempted as to whether it preserves a history of the geomagnetic field or not (see Tauxe, 1993 for a useful discussion). It might seem like semantics, but the ratio of NRM to X/ARM/IRM is just that until you can demonstrate it is likely a proxy for RPI. I would change the terminology to normalized NRM intensity, or normalized intensity ratio, or

something similar throughout this section until the evidence that the record can be used as a proxy for RPI is established.

Line 278: All three normalizers do show similar patterns, but it is interesting that the different normalizers change relative to each other in intensity throughout. For example, while ARM consistently gives the highest ratio, X gives the lowest ratio in the lower part of the record but gives the highest ratio values in the upper part of the record. These variations result from rock magnetic variability and the relative acquisition of X relative to ARM relative to IRM. Could these variations be brought out more to discuss the variations in mineralogy and or grain size?

Line 282: Previously it was stated that compaction would not affect intensity values (see comment on line 211). Here it is stated that intensity could affect RPI values.

Line 288: The low in normalized intensity values at ~21.3 m is interesting for a few reasons. Directional variability is low, in fact, after the Laschamps excursion, two other intervals have greater deviations in inclination than this event. It also has the consistently highest Fe/Ti ratio values of the section >6 m, has low NRM intensity values, but has relatively unchanging rock magnetic properties. The shallower interval at 19.5 m seems to be a better candidate for an excursion than the interval at 21.3 m (greater inclination deviation, similarly low normalized intensity ratios, and "normal" Fe/Ti ratios). It is then said that the low in normalized intensity is the Mono Lake excursion, and the lack of directional variability is ok in this case because this lack of directional variability has been observed before.

I should be clear in that my comment is not that this interval is not linked to geomagnetic variability that has been previously observed at Mono Lake, it is that there is a very quick attribution of every low in normalized intensity record to a geomagnetic event before there has been an evaluation of the nature of the normalized intensity record. I understand that a previous paper set the stage for this interpretation, but the commentary here rapidly assigns excursions to a record with little to no directional variability, solely on the basis of lows in NRM intensity which, in the case of the event at 21.3m, are associated with spikes in geochemical proxy data. The event at 25 m is more than likely the Laschamps event, whether the other events are true excursions is a little less certain in my opinion.

Line 297: I think this whole section (4.4) that discusses remanence acquisition of the NRM intensity record would benefit from coming before the discussion of RPI (currently section 4.3).

Line 336: Section 4.4 should be section 4.5. This has knock-on effects for Section 4.5.

Section 4.4: I found the approach to creating the age model somewhat puzzling. It seems that there is a fairly linear and largely uncomplicated age-depth model relationship available from the C14 dates (aside from date "D"). There is no discussion as to why the remainder of these C14 dates are untrustworthy (aside from an oft observed offset between C14 and OSL), yet these radiometric ages are largely ignored as a primary chronological tool and are only used to support the RPI tie points (which could themselves have up to half a millennia lock-in delay offset).

As a result, I wondered why the C14 dates were not used as the primary age control, with the Pmag tie points being used between the anchored C14 dates to refine the age model? If problematic dates are revealed through RPI correlation (e.g. date "D") then these could be discussed in specificity as to why Pmag wiggle matching might be a better approach. Thinking of the long-term sustainability (and citability) of this record, a geomagnetic record that is based on an independent C14 chronology and then improved through regional-global RPI correlation is probably better positioned than a RPI record that is matched to north Atlantic RPI variability and then qualitatively supported by overlapping C14 dates.

The previous paper by similar authors and the first part of this paper makes the case that the upper 7m has a different magnetic composition than the lower 7m and that the paleomagnetic record is less reliable. There is also a discussion (on line 343) that the Holocene records are complex and beyond the scope of the paper. Then, on line 376, two preliminary tie points are made between the RPI record that are not that entirely convincing in my opinion. I wonder why the authors don't just use the C14 age points through this interval? The authors do state that the interval is complex, and that offset is observed with the C14 dates (line 378), but then promote the pmag tie points over the C14 dates in Table 3 and the resulting age model.

Finally, regarding the age model, have the authors considered putting their C14 ages, OSL ages, and RPI tie points into a Bayesian age-depth modelling program to evaluate uncertainty? A few good ones are available (e.g. Bacon, Undatable). Undatable (Lougheed and Obrochta, 2019) is a particularly useful (and user-friendly program) as you can input uncertainty in age-depth points in terms of depth and age.

Line 357 (and in the following sections): Relative changes in RPI are described (e.g. drops, upward increasing trend, increase towards the top) but the nature of these relative changes depends on whether we are considering downcore variability or progressing forward in time. Just make sure that these are always considered in the same reference frame, as I was unclear which drops were being referred to between the intervals labeled as Laschamps and Mono Lake.

Line 419: Figure panel 6b is referenced in the text before panel 6a.

Line 469: Most of the analysis in the manuscript concerns intensity variations. In these

final paragraphs the comparison to Lake Baikal also only considers intensity variations. However, these variations are described here as PSV (invoking directions and intensity), I would consider revising to make it clear that you are only considering intensity variations (or add a directional comparison).