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Reply on RC1

Stephanie Scheidt et al.

Author 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-AC1, 2021

Many thanks to Reviewer#1 for your very constructive comments on our work. It is an excellent review in which inconsistencies are identified that we had not noticed. Reviewer#1's comments were also very helpful in identifying text passages where we did not express ourselves clearly enough. We very much appreciate the time Reviewer#1 has invested.

Below we provide responses to the comments.

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

Reply: We are sorry to mix-up "also" and "although". This will be changed

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

Reply: Indeed, a good suggestion. We will present an age range.

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

Reply: True. We will write out the word everywhere in the manuscript.

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

Reply: True. The word is deleted

- 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."

Reply: Thank to Reviewer#1 for bringing the issue with the gray lines to our attention. Some of them must have been displaced slightly without being recognised. Fig. 2 will be corrected in the revised version.

Regarding Reviewer#1's question to the coring procedure, we will clarify this in section 3.1. The additional information will show that the core depth is derived from the drilling depth at which the barrel was opened to collect the sediment. The fact that the cores were directly cut in two individual sections during field work will also be included in section 3.1.

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

Reply: True. The sentence is changed.

- Line 142: Unlike PSV, which has strong regional imprints, RPI (at least at the millennialscale 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.

Reply: Thanks to Reviewer#1 for pointing this out. We obviously mixed up PSV and RPI issues. We will consider this in the revised version. However, the complete section is reorganized following the suggestions of Reviewer#2.

- 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.

Reply: We agree and will include the references of the individual records.

- Lines 151-167: This information is presented as bullet points. I am unfamiliar with thejournal 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 theauthors and editor though.

Reply: There are no such rules explained in the guidelines. However, we will change the list of bullet points into subsections.

- 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 valuesto also be affected. The result might be lower NRM/normalizer ratios than expected with all else being equal.

Reply: We are sorry for the confusion. It is true, that there might be a vector subtraction if a single magnetization vector is deformed in a certain way. We actually intended to describe a rotation of the material, which has no effect on the intensity but on the direction of the magnetization. However, since anonymous reviewer#2 was also confused and only two samples are affected we decided to discard one of these samples completely and keep the complete data of the other sample, though showing somewhat misaligned directions. Accordingly, the text section and the figures will get corrected.

- 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?

Reply: Your assumption is correct. We will revise the sentence.

- 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.

Reply: As reviewer#2 made a similar suggestion, we will repeat our arguments in greater detail and use different wording until we show that the RPI proxy represents fluctuations of the EMF.

- 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?

Reply: Thanks to Reviewe#1 for pointing this out. We will make aware of the change in normalizer intensity and discuss the implications of this issue.

- 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.

Reply: This is correct. We did not express the differences properly. In the revised version, we will make substantial changes in the section related to line 211.

- 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 otherintervals 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.

Reply: We understand the point and we will explain the reasons for the assignment of the Mono Lake excursion to the RPI low at 21.3 mcd in greater detail in the revised version. However, the Fe/Ti ratios will not be discussed in this context. This is due to the fact that the Fe and Ti values were measured at core material in 2 mm resolution. The slightly increased values around 21.3 mcd result from isolated measuring points with elevated Fe values. However, the range of each sample includes about 11 measuring points. It is thus likely, that the sensor was placed directly on a Fe rich spot resulting in higher Fe values. This does not mean that the complete sample is characterized by higher Fe values. If the Fe/Ti ratio of a complete sample was elevated, the sample was discarded.

- 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).

Reply: We agree and change the order accordantly.

- 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.

Reply: We understand now that the reason for the chosen strategy is not sufficiently explained by us. In the previous study of Andreev et al. 2003 of core PG1228, ^{14}C ages of macro remains were found to be ~2500 years too old in the Holocene part and approximately consistent >18 ka. We will include this information into section, which will be 4.1 after restructuring related to reviewer#2's suggestions. By doing so, the reader may understand why we did not take the ^{14}C ages for granted. In the Holocene part, the

correlation using the ^{14}C ages as tie points did not yield meaningful correlation schemes for RPI correlation. This will also be stated in the revised version. Overall, ^{14}C ages were used for orientation if different correlation schemes were possible from the RPI point of view. This was shown by example in the discussion, but will be more emphasized in section 4.5.

Regarding age-depth modelling, we have decided against this approach, because age-depth models are just as good as the methods /software used, the model parameters chosen and the input made. In our study modelling assumptions for the possible offset of the ^{14}C ages in the individual depth intervals (at least in the part <18 ka) and the log-in depth of the remanence acquisition would be necessary, just as a weighting for the individual data inputs. As described in the text ^{14}C ages influenced the RPI correlation scheme. Thus, ^{14}C and RPI data points are not independent from each other. In absence of many independent ages, a Bayesian model, such as Bacon, likely creates a best-fit model that resembles linear interpolation (which is applied in our approach) but reduces uncertainties. Hence, there is a risk that the age-depth model created conveys greater certainty than actually exists. Overall, we do not see a benefit from age-depth modelling in this study and consider the age-depth model presented to be the purest form of representation of the data. For any future studies, we provide all data open access to enable this approach. Besides, the data of this study that has already been accepted by the Journal of Quaternary Science and will thus be available soon.

- 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 labelled as Laschamps and Mono Lake.

Reply: Thank to Reviewer#1 for making us aware of the mix-up of temporal and spatial expressions. We will check the text thoroughly. In this course, we will also make sure that the reader will understand to which RPI low we refer to between TP6 and TP7.

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

Reply: We have to disagree with this statement, since 6a occurs for the first time in line 379, while Fig. 6b is mentioned for the first time in line 432.

- 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).

Reply: We agree and will change the wording accordingly.