Geochronology Discuss., author comment AC2
https://doi.org/10.5194/gchron-2021-12-AC2, 2021

Reply on RC2

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-AC2, 2021

We thank Reviewer#2 for recognizing the importance of the record and the detailed comments on the study. They gave us a new perspective on our work and provided some very helpful suggestions that will enhance the quality of our study. Thus, we appreciate the time invested.

Reviewer#2 describes in his general comments that such an RPI study usually follows one of two conceptual approaches, but also shows that other possibilities exist. As Reviewer#2 correctly pointed out, this is the way we go. The main focus of our work is to present an RPI and PSV dataset from the High North as it has not existed before and to introduce its possible implications. Because only a chronological framework enables to use the data for further analyses, the emphasis is on chronostratigraphy. Thus, we intended to do exactly what Reviewer#2 suggested ("I guess what I am saying here is that they should focus on the stratigraphic aspects first (…) then pivot to the geomagnetic aspects after"). To follow the advice of Reviewer#1 and Reviewer#2 we will reorganize the order of the sections in the Results and discussion chapter as follows:

- 1) Basic chronology (spanning a rough time frame)
- 2) Magnetic mineralogy (shows the suitability of the sediment for RPI)
- 3) Remanence acquisition (introducing the lock-in issue; position of the section was suggested by Reviewer#1)
- 4) PSV+RPI (description and discussion of the data)
- 5) Correlation (Discussion on reference records, correlation of the data and final age-depth model)
- 6) Implications

In the following, we go through the specific comments one by one. As Reviewer#2 addresses issues in the general comments and in the specific comments we include responses to the general comments of Reviewer#2's in appropriate places of the specific part.

- Abstract: This is really the second paleomagnetic study as the first was published in GRL, although its focus was a bit different (rock magnetism and the magnetic carriers), it's still paleomagnetism.

Reply: Reviewer#2 is absolutely right. We showed RPI approximations of whole-core measurements in the previous study. We will correct this issue in the abstract and in the introduction.

- Line 40: "These variations are expected to be at least partially independent from the global pattern of secular variation (St-Onge and Stoner, 2011; Lund et al., 2016)." Expected is a very strong word, might be better to suggest that there is some evidence to suggest that the geomagnetic field of polar regions may have some unique characteristics. But the authors should be cognizant of the scale of the evidence presented that was both centennial to millennial in scale and associated with directions and neither would be well observed in this study. Additionally, this gets to my point above, as there is a lot of focus on how unique the record should be, yet much of the manuscript is built on comparing the record to a nominally global stack (GLOPIS), maybe they should just focus on that?

Reply: We will use the wording as suggested by Reviewer#2.

-

  Reply: We will use the wording as suggested by Reviewer#2.

  Regarding the suggestion that we should focus only on GLOPIS, we have problems understanding the concern. Reviewer#2 states that there are no other records "that are as homogeneous, continuous, and at such a high resolution" and have therefore "the potential to provide a unique observational perspective on the geomagnetic field, and through magnetic stratigraphy, paleoclimate and environment". As we are in the introduction of the study, which describes the general framework, the significance of the study, possible implications for the future and the objectives, we are convinced that a short description of the relevance of the record is well placed here. However, for clarification we will include the information in the introduction that there are no regional datasets available. We hope that this also serves as an answer for Reviewer#2's comment on the use of the directional data from the general comments ("even though the location and its paleo-geomagnetic potential is a point of emphasis, they really don't discuss the data in that regard very much and largely ignore the directional component that might be more apt to provide insights on tangent cylinder dynamics."). The lack of high-resolution records that extend over the required time span has profound consequences for the presented study. There are, for example, no reference records for PSV correlation available that can be used to recognize regional pattern of the EMF. Thereby, and because of core gaps leading to uncertainty of the declination, a robust evidence-based discussion of the variations of the EMF in the tangent cylinder based on the PSV must wait until more data sets from the region are available. Nevertheless, we are convinced that the directional dataset is worth publishing as it is the first in the region and will enable future studies to discuss PSV variability.

- Line 46: "Lacustrine sediment successions are particularly valuable for studying the magnetic and environmental history in the Arctic, because they often exhibit more continuous and undisturbed deposition with high accumulation rates compared to marine sediments." It's usually the high-resolution part that makes them so valuable, being continuous, long, high res and undisturbed is pretty unique. Overall, I don't think you have to work too hard to sell this as a valuable record.

Reply: Thanks to Reviewer#2 for this clarification. The attributes are used comparatively to accurately describe the differences to marine records.

- Line 134: "In most cases, five consecutive AF demagnetization steps between 15 mT and 80 mT were used to determine the ChRM." Which 5 are chosen is pretty important, especially if it's not consistent and why only 5?. We will touch on this more below.

Reply: Please see the reply to comment on line 255 below.

- Section 3.4, The ordering of the paper is a bit odd as much of what normally might be in the discussion or even result (background on other records being compared too). The discussion on the reference record is confusing and it doesn't have to be. It would seem to me to be much simpler to just use GLOPIS with an updated chronology (See Obrochta et al., 2014) for how to transfer from GISP2 consistent to GICC05), rather than bringing in all the individual records that using GISP2 chronologies. But, if you are going to explore them, then you should reference them, and look into how their chronologies were developed as well.
- *("The manuscript is structured a bit oddly the records being compared to being discussed in the methods rather than in the results or discussion. Rather than discussing details of the records and their chronologies (which they don't do particularly well, or even accurately, e.g., GLOPIS is not dated by Ar/Ar, although it provides support), they could just outline the basic assumptions in the method. As stratigraphy is a primary focus, a key aspect could be to decide on a central assumption and then go with it. For example, that GLOPIS accurately represent global variation in geomagnetic intensity and by correlating your record to it you can put their time on your record. Essentially that is what is done, but its greatly complicated by incomplete discussions of the records that GLOPIS is composed of.)*

Reply: Please note that we inserted the general comment that addresses a similar question in brackets and italic letters above. Regarding these comments, there are different issues to clarify, which we like to do by the bullet points below.

- Thanks to Reviewer#2 for making us aware that we have discussion related elements in the methodology part. We will shift this section to the section in which we use the reference records for correlation.
- The reference records used are not introduced in the result and discussion part as they are not our results but well-known published records. We will now report on the reference records as we report on any other material used. We will also include the references that were missing.
- To discuss the background information of well-established and published records is not necessarily required. This approach can be observed in several other studies (e.g., Channell et al., 2008 (https://doi.org/10.1016/j.epsl.2008.07.005) Makaroğlu et al., 2020 (https://doi.org/10.1093/gji/ggaa281)). However, we follow Reviewer#2's suggestion and will include a statement in which we assume GLOPIS to precisely show the variations of the EMF. Thereby the age-depth model of GLOPIS and the related records shown can be transferred to Co1401 without further discussions. Thanks to Reviewer#2 for this suggestion.
- We are sorry to have included inaccuracies. It is correct that GLOPIS was not based on K/Ar or Ar/Ar ages. Only the update of GLOPIS was constrained using this dating methods. We will correct the information in the revised version.
- Thanks to Reviewer#2 for making us aware of Obrochta et al. (2014). After studying the publication, we got aware that "the uncertainties and limitations in the correlation procedure of the original authors must be considered". Unfortunately, this means that the required data is not completely available for all records used.
- At first glance, it may seem easier to associate Co1401 to GLOPIS only. Indeed, we tried that first and ended up with different plausible-looking alternative correlation schemes. Thus, we consulted some of the regional records to be aware of the naturally occurring variations that are averaged out in GLOPIS. This was how we received our correlation scheme. Therefore, we are sorry to have to disagree at this point.

- Line 210: Here you start going through quality control and I had a few questions and comments. "First, sections with erratic, discontinuous ChRM directions were assumed to be affected by core disturbances. However, if inconsistent ChRM directions occurred only near the cut edges of the cores, the corresponding RPI values were not discarded," Why are anomalous directions considered independent of intensity, if one is disturbed wouldn't the other?

Reply: We are sorry for the confusion. It is true, that disturbed sections are always disturbed no matter where they occur. 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 Reviewer#1 was also confused and only two samples are affected we decided to discard one of these samples completely due to large offset from the expected directions and keep the complete data of the other sample, though showing somewhat misaligned directions. Accordingly, the text section and the figures will be corrected.

- "Second, samples with unstable demagnetization behavior and those that were almost completely demagnetized in ≤30 mT were considered unreliable." Is there any accounting for this filtering, is it random, is it in particular sections? Having NRM that are that soft would be pretty unusual and would be interesting to know where and why they are found and if there was any rock magnetic reason.

Reply: Thanks to Reviewer#2 for pointing this out. The samples which were demagnetized ≤30 mT are those between 18 and 30 mcd, which were also discarded because of elevated NRM values. We will delete the part of the sentence to avoid ambiguities. Regarding the rock magnetic reasons, we agree with Reviewer#2 that a respective discussion would be interesting. We nonetheless decided that a discussion of the magnetic mineralogy of discarded samples would go beyond the scope of this paper.

- "Third, samples at the end of core sections with significantly lower NRM values than adjacent samples were discarded because likely affected by oxidation of unstable remanence carriers." Why is that the reason and if they are that unstable then it makes you worry about the age of magnetization and the other data too. The overall data supports a quality record, so maybe it's just disturbance? And as a general comment, you don't have to explain everything.

Reply: As we describe the reasons for discarding samples in general, the presence of unstable sulphides, as they exist in the upper part, is included here. In the revised version, we will clarify this issue to ensure that readers do not doubt the quality of the record.

- Line 223: "Sample selection resulted also in a reduction of the already low scatter of NRM,χ, ARM, and IRM values (Fig. 2)" Is there any accounting for what was removed?

Reply: In this sentence we refer to Fig. 2, because the requested information is part of the figure. As explained in the legend of Fig. 2, removed samples are indicated in red. In addition, you will also find a detailed list of all discarded samples and their measured values in the data documentation on Figshare. We hope Reviewer#2 will regard this to be sufficient.

- Line 227 "Below 6.7 mcd, the anticorrelation might be explained by a sorting effect, with finer detrital material originating from more vegetated areas." They don't have to explain the observation, however, if they do then they should have a mechanism with references behind it, or at least provide a detailed description of what they mean, as I did not follow what they were getting at. Potentially more important, it might be useful

to see if these difference in anyway correspond to the changes in NRM coercivity they talk about below.

Reply: We disagree with the comment that we don't need to give possible reasons for such an observation. Regarding the explanation, we will revise the section to make sure all readers understand the mechanism we propose.

- Line 255: "In the PCA, the ChRM is defined by the steepest part of the AF demagnetization curves, which was generally at higher AF fields in the upper part of the core than in the lower part" Why the steepest part? What does it mean that the components are defined from different parts of the vector? One would think such systematic differences could be problematic for relative paleointensity which requires consistency through the studied interval (its relative). MAD values are a very useful metric to assess quality of the magnetization and are best if applied using a constant interval through the studied record. You might think of an iterative approach where in a second iteration you define the optimal component using a few less steps than were used in the blanket MAD value component assessment.

Reply: We demagnetized the samples in 12 consecutive steps. As we describe in our study, the first three steps up to 15 mT deleted a "small viscous overprint" that occurred in some samples. On the other hand, measurement noise has a larger effect when the magnetization gets weaker (highest 2-3 AF steps, please see Fig. 4 for examples). Thus, the PCA was determined by the steps in between, in which the signal is most stable. The steepest part of the demagnetization curve was used to capture most of the magnetization of this range. Finally, we intended to use those data that result in lowest MAD values. We will explain that all in greater detail in the revised Version. Besides we like to state that we are grateful to have been made aware of this passage once again. This made us realise that, in order to minimise MAD, we had not included the full AF range used for the RPI proxies in the PCA. This will now be corrected. Although the changes will probably be tiny we are happy to adjust the PCA. Regarding the number of steps used, usually a number of 5 consecutive measurements is regarded sufficient and does not need to be discussed further. With this answer, we hope to have sufficiently answered your general comment ("which 5 will be selected"(...) and why only 5") as well as the question "why the steepest part?". Please note that a problem for RPI determination cannot result from this procedure, since we consistently used the partial NRM (pNRM) between 30 and 50 mT demagnetization steps (4 steps) and corresponding normalizers. This is already described in the methodology part. Finally, Reviewer#2 is right, that we missed to report on the MAD. This will be done in the revised version. We have a mean MAD of 1.4 for the sample set, which we missed to mention. Thanks to Reviewer#2 for making us aware of this fact.

- Line 261: Beside GAD, it might be useful to look at the modern field and see how that compares. And for comparing with the average inclination it, might be best to exclude excursions as is commonly done.

Reply: Thanks to Reviewer#2 for this suggestion. We will include a recent value of inclination and declination and will only state the mean and medium inclination values excluding the geomagnetic excursion.

- Line 266: Why does the cumulative error in dec increase, isn't it the same for each unoriented drive.

Reply: As explained in the study, the cores are not oriented to the North direction, nor we have overlapping cores except from the uppermost 2 m. As described, we assume that the uppermost samples carry approximately the direction of the EMF today. Therefore, the uppermost core was rotated to the recent field value. The direction of the samples in the subsequent cores depends on the orientation of the respective core above. Coring and

sampling gaps might cause some degree of uncertainty about how sample directions change from one core to the other. Thus, according to the assumption made, it is a case of error propagation. We will include these keywords in brackets to make every reader aware of it.

- Line 268: "In a few cases, sudden changes in declination also have been detected withincores. Here, core parts are probably twisted against each other." If the inclination is steep that might be expected, otherwise the deformation you describe would be problematic for the intensity record as well. And these particular features might be worth nothing as they are saying something about the field

Reply: Here we have to disagree. A simple rotation of a core segment on a laminar surface does not result in any changes in the RPI. It merely corresponds to the rotation of individual core segments in relation to each other. A movement could only lead to deformations and thus to disturbances of the RPI if the movement surface would be located within a sample. This is not the case. For clarification we will add this information.

- Line 269: "Because of these problems, a pole wander curve was not calculated for Co1401." These are typical issues that must be dealt with in such reconstructions, although not optimal, if desired such reconstructions could be made, it just increases the uncertainty. Also, it's not a polar wander curve, but rather a virtual geomagnetic pole (VGP) path that could be calculated.

Reply: Thanks to Reviewer#2 for the correction in the use of the expression. We will correct this. We do not agree with the statement that it would be possible to create a VGP path if desired, since a VGP path on the basis of the available data set is subject to far too much uncertainty to be valid. This is due to the mechanism of error propagation (see reply to comment on line 266) and the dimension of the core gaps (described in section 3.1 and fully documented in figshare).

- Line 275: "The RPI of Co1401 shows …" At this point you are still making a case that normalized remanence record dominantly represents changes in geomagnetic intensity, as such it would be better to refer to it as normalized remanence until a strong case is made that RPI is preserved.

Reply: As Reviewer#1 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.

- Around line 277 you begin discussing the upper sediments. As you have previously indicated that these are magnetic mineralogically problematic, it might be easiest just to say that and say they, without much additional work, are not suitable at this time for relative paleointensity reconstructions.

Reply: Please see the reply on the comment to line 375 below

- In the paragraph starting at line 284, you begin discussing the low normalized remanence intervals. Would be nice to have a blowup of this depth. You say "Age and pattern of this event coincide with those of the Laschamp geomagnetic excursion (e.g., Laj et al., 2006; Li et al., 2018; Simon et al., 2020)", but you have not yet defined the age of these sediments, and not sure what you mean by pattern" At this point and really throughout the record it might be useful to compare with the Black Sea and Baikal records. When it comes to the discussion of Mono Lake, you should define how and why you pick that intensity low as the one you give that name too, as well as the ambiguity associated with that event that was recently discussed by Korte and others.

Reply: As suggested we will include an additional figure that shows the low normalized remanence intervals to make it clearly recognisable. Regarding the issue with the word age, we like to make Reviewer#2 aware that a basic chronology was established in a previous section already. In this section a ^{14}C age is introduced close to the proposed geomagnetic excursion. However, we will re-address this fact in the manuscript to remind readers and ease understanding.

Regarding your suggestion to compare our record to the Black Sea record and the lake Baikal record: Please note that the lake Baikal record is shown in Fig. 7 of this study and that it is explained that the sedimentation rates vary widely, the age-depth model is not up to date and the data set is not reported in such a way that one can easily transfer the RPI to a recent age-depth model. In the case of the Black Sea records we assume you refer to the stack provided by Nowaczyk et al. (2013). The 50-year stack includes some of the 16 cores whose data are only individually documented by Liu et al. (2020) (https://doi.org/10.1594/PANGAEA.919446). Unfortunately, the data of the 50-year stack are not documented, which is why the dataset was not included. Finally, thanks to Reviewer#2 for making us aware of the lack of discussion regarding the Mono Lake. We will expand the corresponding text section.

- Section 4.4 Remanence acquisition in Lake Levinson-Lessing: Lines starting at 315: Lots of discussion about lock in, but you make few observations. Would be better if they focused on the observations then use our understanding of the issue to facilitate discussion.

Reply: There is probably no question that the lock-in issue is important in this study. We explain at the beginning of the section why it is not possible to make own estimates. Thus, we think it makes sense to look at studies with comparable settings and then present the similarities and differences to our data step by step. Please note that we follow the suggestions of Reviewer#1 and Ian Snowball to improve this section.

- Section 4.4 RPI correlation and implication for Co1401: Some discussion on the development and adjustment of the GLOPIS age model would be very relevant. In the methods this is incorrectly attributed to K-Ar, and Ar/Ar, although that provides some constraints, that is not how this chronology was developed. Additionally, if you are going to talk about the individual records, you might want to discuss their chronologies and references those studies.

Reply: Please see the reply on the comment to section 3.4 above.

- Lines 335 "Due to the problems with reference datasets" what problems are those? Additionally (lines starting at around 341), it would be better to treat the Holocene separately, rather than "As a preliminary result, the range <10 ka is therefore only correlated with the virtual axial dipole moment (VADM) of GLOPIS-75-GICC05 that show a general trend of the intensity variation of the last 10 ka (Fig. 5).", if you are unsure of the record "the presence of greigite associated with the intensification of RPI variation in Co1401 requires a more detailed investigation of the magnetic mineralogy of the individual samples before the RPI can be considered reliable." then don't correlate it to anything as a global reconstruction is no more likely to be reliable than a regional record.

Reply: Following Reviewer#2's suggestions, we rephrased the entire section. Thus, it will be much clearer in the revised version to where we refer to. Regarding the remaining issues of this comment, please see the reply to the comment on line 375.

- Starting at Line 355: "The correlation procedure started with the Laschamp geomagnetic excursion, which is tie point (TP) 7 (Table 3)." Provide more detail, what

depth, why you think it's the laschamp etc.

Reply: We like to make Reviewer#2 aware that the depth of all tie points is shown in a comprehensive way in Table 3. Presenting the age and depth data in this way has the advantage that all the data can be grasped at one glance and allows for quick reference after the first reading of the study. In addition, the discussion of why this RPI low is suggested to be the Laschamp geomagnetic excursion has taken place in the previous section on PSV and RPI. Following Reviewer#2's suggestion, that section will be extended.

- "TP 6 was set to the RPI low of the Mono Lake geomagnetic excursion" You are correlating an rpi low at xx depth to an rpi low at xx age in GLOPIS, but how do you know it's the Mono Lake excursion, maybe say something like, the paleointensity low at xx depth was correlated to a paleointensity low associated with Mono Lake excursion as reported by Laj et al.

Reply: Thank to Reviewer#2 for making us aware that we did not use the correct wording. We will change this.

- Around line 375, why tune within the Holocene as I thought that magnetic mineralogymake that part of the record suspect.

Reply: We have learned from Reviewer#2's comments that the order of our arguments and the wording result in misunderstandings regarding the expected influence of greigite in the upper part of the core. We will thus make changes in all related text sections to make the reader understand that we do not expect this part to be unreliable nor unsuitable to the method. Instead we will highlight the much larger opportunities for discussion of the Holocene part in an own extensive study, which is actually already on its way. In addition, we like to explain here that we decided to present preliminary results to provide a chronology of the complete record to follow-up studies.

Much of the discussion should be rewritten, and on and on and on.

Reply: We have taken over all reasonable suggestions from Reviewer#2. We thank Reviewer#2 once again for his detailed review, as the adjustments significantly improved our work.