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Comment on gchron-2021-12

Anonymous Referee #2

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-RC2, 2021

Overall: The manuscript "A 62-ka geomagnetic palaeointensity record from the Taymyr Peninsula, Russian Arctic" by Scheidt et al presents a very interesting paleomagnetic data set from a first of its kind late-Pleistocene through Holocene sedimentary archive from a lake north of the Arctic circle in Siberia. As there are few high quality paleomagnetic records north of the Arctic circle and essentially none that are as homogeneous, continuous, and at such a high resolution, these data have the potential to provide a unique observational perspective on the geomagnetic field, and through magnetic stratigraphy, paleoclimate and environment as well. For such paleomagnetic studies you essentially have two choices, you can use the paleomagnetic record for stratigraphy, or if you have an independent chronology, you can use it to study the geomagnetic field. It's possible to do both if you can make a strong stratigraphic case first, then you might discuss some of the unique observations that are not controlled by the stratigraphic decisions or are otherwise well supported by other constraints. The primary focus of this manuscript is stratigraphy, however they also discuss the paleo-geomagnetic implications, especially those associated with it northerly location and the potential influence of tangent cylinder dynamics. The difficulty is that these are not clearly separated which makes things a bit problematic. Additionally, 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. So I guess what I am saying here is that they should focus on the stratigraphic aspects first, if successful and it should if they are rigorous about it, then pivot to the geomagnetic aspects after. Doing so would provide a manuscript better fitting of the data it writes about.

The manuscript is structured a bit oddly with 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. Jut focusing on GLOPIS, its chronology and how its developed, and your correlations to it, the chronological implications and how those relate to your other independent chronological constraints would be a good first step. It looks like that would work well and therefore to a first order global intensity variations would explain first order intensity variations at your site, which is pretty interesting in and of itself. If that was done first, and then a second iteration to look for differences they might then be able to explore some of the unique aspects of the record. As written, it's all kind of intertwined and really hard to decipher. Discussion occur before observations and inaccuracies in the discussion are too common.

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Additionally, regardless of your stratigraphic or paleo-geomagnetic emphasis, you have to make a case that paleomagnetic variations you observe are geomagnetic in origin. An earlier paper discussed the rock magnetic attributes of the core, demonstrating its suitability for paleo-geomagnetic reconstructions from the Pleistocene part of the record, while indicating that the Holocene record was characterized by a somewhat more complex magnetic mineralogy that might make paleo-geomagnetic interpretations difficult and therefore the interpretation they make on that part of the record are less likely to valid. Not having to include all the rock magnetic data certainly shortens the contribution and makes getting to the point easier. However, they never really circled back on this difference in magnetic mineralogy and its potential impacts (details below) on some of the observations that are being made, like coercivity of the component magnetization and correlation with organic carbon or even the Holocene paleomagnetic record which they state is unsuitable, but still use it for the chronostratigraphy.

Specifics:

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.

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?

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.

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.

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.

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?

"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.

"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.

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?

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.

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.

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.

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

Line 268: "In a few cases, sudden changes in declination also have been detected within cores. 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

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.

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.

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.

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.

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.

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.

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.

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.

“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.

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

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