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Interactive comment on "Magnetostratigraphy of sediments from Lake El'gygytgyn ICDP Site 5011-1: paleomagnetic age constraints for the longest paleoclimate record from the continental Arctic" *by* E. M. Haltia and N. R. Nowaczyk

Anonymous Referee #1

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The manuscript "Magnetostratigraphy of sediments from the Lake El'gygytgyn ICDP Site 5011-1: paleomagnetic age constraints for the longest paleoclimate record from the continental Arctic" by Haltia and Nowaczyk presents a fantastic data and is a nice read. Overall I really enjoyed this manuscript, it's nicely done illustrating an apparently well resolved magnetic stratigraphy, reads well, and it is rare to see so much work done on the catchment materials and their influence on the magnetic properties of the sediments. That being said there are a lot of things that could be done to improve the manuscript and some should be done before the manuscript is accepted.

Although I enjoyed reading the rock magnetic section, I found that it in some ways it distracted from the main objective of defining and presenting the magnetic stratigraphy of Lake E. The main point of this section should be to determine whether the rock magnetic variability might influence magnetic stratigraphic interpretations. It is noted in the latter part of the manuscript, "However, as shown by Nowaczyk et al. (2007) and Murdock et al. (2013) using pilot cores from Lake El'gygytgyn, the concentration of magnetite in sediments is mainly controlled by the hypolimnetic redox conditions through large-scale magnetite dissolution during glacials and not simply by detrital input." Page 5093, Line 20-24. This variability is clearly observed in Figure 2, yet its potential impact is only addressed after everything else is presented. This should be mentioned from the beginning and the effect of this, or lack there of, on the paleomagnetic record and magnetic stratigraphic interpretations should be the primary objective of the rock magnetic section. This could be easily addressed by showing the demagnetization behavior from representative samples of the variable lithologies/magnetic properties (high intensity/low intensity intervals) rather than from general locations within each polarity as in Figures 3 & 4. Below ups of the transitions and the demagnetization behavior through those intervals would also help make this case. There are quite a few other little issue below that should also be addressed. As this is the primary chronology for one of the worlds most important paleo archives it really should be presented and assessed as strongly as possible.

Other issues,

Reads somewhat as if it is three unrelated papers, one outlining the magnetic stratigraphy, another the rock magnetic variability within the catchment and how it effects the sediments in a general way and a third on the age variations and sedimentation rates. A more focused presentation around the central topic would improve this. One possibility would be to focus the rock magnetic discussion on how or whether it influences the paleomagnetic record. In this approach the paleomagnetic data would be introduced, the evidence supporting magnetic stratigraphic interpretations including potential rock

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magnetic effects, or how the paleomagnetic signal is not influenced by these, and then the implications of these interpretations and the evidence that supports it.

A little more information on the drilling method, was it all APC or was part of it XCB/RCB drilled as implied by the “discs”?

Would be good to know more about the discrete measurements, assume they were single measurements after each demagnetization step using a pass through system?

The MAD values should be presented somewhere and if the demagnetization steps used to calculate the component differ substantially, that should also be presented.

Specifics Page 5083, line 15, discs are often termed biscuits and I assume that the drilling technique used here was different, though this was not discussed.

Page 5083, line 28, at which lab were the results obtained?

Page 5084, line 7-8, so what was done if the MAD values were higher than 5? Were these the only ones used?

Page 5084, line 9, how was the depth of integration determined? Typically using the width of the response function at half height yields distances of 4.5 cm (e.g.,Weeks et al., 1993) to 7.7 cm (Jackson et al. 2010) depending on the coils and the system.

Page 5084, line 13, change “Unless otherwise is stated” to Unless otherwise stated . . .

Page 5087, line 5, its important to point out that there is significant variability on the meter to 10s of meter scale and that prior work has been done assessing the cause of this variability. This is ignored in this part of the manuscript and it should not be.

Line 10-15, Where in the stratigraphy are the samples that are not well behaved? Do they effect interpretation of polarity, either reversals or for short duration events?

Line 15-17, it would be great if there was figure showing the MAD values. A stratigraphic view of the quality of the magnetization is important

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Line 19-21, “values might be equivocal.” Are those samples considered or not?

Line 23-25, “Only the inclination of the ChRM will be discussed here. . .” it does not mean that you cannot use declination to help your determination of when you go from one stable polarity to the next. A better approach would be to define polarity transitions with an interval rather than just a depth, as there is uncertainty in the determination and polarity transition do take time.

Page 5088, line 12-14 “. . . data are partly unreliable because. . ..” You could us gray in the figures (instead of black or white) to denote intervals of indeterminate polarity.

Line 18, Are ages from Ogg and Smith (2004) consistent with those derived using LR04? Not an issue until you compare results, but could be important at that point.

Line 23-25, “ . . .which may represent the Olduvai precursor. . .” down core rock magnetic evaluations are required to evaluate the fidelity of these important features. There are other features (below the Brunhes/Matuyama boundary) in the inclination record that are not considered, so why should they be considered to be robust and not an artifact of coring induced overprints, disturbance, rock magnetic complexity, etc.

Page, 5089, line 1 “These represent either sediment disturbances and/or coarse-grained mass movement” This is not an either or question, so which is it?

Line 18 “. . . vaguely determined. . .” If error bars were used or an interval to define transitions, vaguely would have a meaning.

Section 4.3, wonder if this section that builds upon prior work (or at least it should) might be better prior to the magnetic polarity results?

Page 5091, line 18-20 “. . .comparable to that generally found in igneous rocks. . .” this only applies to the high intensity intervals, which are only separated by a few meters from low intensity intervals and therefore not a general comment about the amount of magnetite.

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Line 25- “The variable lithology and the variable degree of physical and chemical alteration of the investigated source rocks is reflected in the highly variable concentration of magnetic minerals in the analyzed rock samples, and it is also characterized by _LF vs. SIRM bi-plot, which visualizes variations in the mineralogy, concentration, and grain size of magnetic minerals (Fig. 10).”

This is the crux of the issue, should be stated and dealt with earlier in the manuscript and why care needs to taken for the magnetic stratigraphic interpretations that are based on the assumption that the magnetization accurately reflects the behavior of geomagnetic field at about the time of sediment deposition.

In addition, this contradicts previous work and as stated a couple of pages below “However, as shown by Nowaczyk et al. (2007) and Murdock et al. (2013) using pilot cores from Lake El’gygytgyn, the concentration of magnetite in sediments is mainly controlled by the hypolimnetic redox conditions through large-scale magnetite dissolution during glacials and not simply by detrital input.” Page 5093, Line 20-24,

Again the question is, does this influence the magnetic stratigraphy.

Page 5092, line 1-5 “Smaller magnetite grains are. . . ” Take a look at Ozdemir et al., 1993 and Smirnov and Tarduno, 2000 for an alternative interpretation.

Line 6-15, The discussion here is based on an assumption that the magnetic separation and SEM work being illustrative of the dominant process, not just a process that is going on. These methods are often biased towards larger grain-sizes so advocating such a complex method as the reason why the hysteresis data suggest smaller grain-sizes may not be justied.

Line 26-30, “Interestingly. . .” SIRM/klf is not often thought to be a magnetic grain-size ratio and especially when dealing with maghematization as that typically gives higher SIRM values (Ozdemir et al., 1993 and Smirnov and Tarduno, 2000). ARM/klf is an alternative for grain-size that would be worth looking at.

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Page 5093, line 6-8 ".. a quarter of" the "samples indicate more pronounced contributions from magnetic minerals with harder coercvity. . ." might this reflect changes in magnetic concentration, with the hematite more apparent during intervals influence by reductive diagensis and reduced magnetite contribution as stated below. "However, as shown by Nowaczyk et al. (2007) and Murdock et al. (2013) using pilot cores from Lake El'gygytgyn, the concentration of magnetite in sediments is mainly controlled by the hypolimnetic redox conditions through large-scale magnetite dissolution during glacials and not simply by detrital input." Page 5093, Line 20-24,

Does maghemite survive in the reduced intervals and how do these influence magnetic polarity boundaries or do they influence magnetic polarity boundary determination??

Overall, suggest they focus on the lake sediments. Its nice that they show that these sediments are generally consistent with a detrital origin, but more than that is really not relavent to the main point of this paper being magnetic stratigraphy. Therefore suggest that some of the discussion of the processes and differences between lake and catchment magnetic properties be placed elsewhere.

Discussion

Page 1094, line 15- "These magnetostratigraphic tie points form the chronological frame for aligning (tuning) the different sediment climate proxy parameters with respect to orbital changes, which refines the temporal resolution of the sediment chronostratigraphy (Nowaczyk et al., 2013)."

This illustrates the importance of getting this right and the care that should be taken with our assumptions of the age of magnetic reversals, there durations and there positions relative to isotopic stages which is clearly a work in progress (see Channell et al., 2010 for a good example).

Page 1097, line 20- ". . . the position of lake El'gygytgyn may decrease. . ." There are a myriad of reasons for why something might not be recorded, but a longer duration for

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reversals is likely not one of those as that would have the opposite effect.

Line 24, what is the rock magnetic variability around these?

Line 26, "... more scattered inclination record...." What about the quality of the cored material, there is little discuss about that and coring disturbance along with drill string induced overprints are common reason to have low quality results associated with deeply buried materials.

6. Conclusion

Page 5098, line 8, "Remanence is carried by partly maghemitized titanomagnetite..." Whether this is consistent throughouyt the record is not well demonstrated. The fact that there are two populations of lake sediment on the day plot (Fig. 11) is not discussed and should be.

Tables 2 should include references 4, I would suggest against listing Intra-Jaramillo and Olduvai precursor in the same table as your polarity reversal boundaries as they cannot be used to provide age control, but are rather observations of potential geomagnetic features of interest. Also, I would suggest you come up with a $\pm$ for the depth interval and if possible tie it to the core, section and interval depths.

Figures. 2 Would be great to see intensity after at least one demagnetization step as well as the NRM and ideally this plot would also include inclination, declination and MAD values.

3 and 4, would be great if these were associated with representative lithologies (high and low intensity) and around intervals of interest as well as covering the core material. Nice if location from where these were taken were shown in Figure 2.

5. Optimally VGP latitude, at least around transitions, should be calculated and polarity determinations based on that. Would be nice for the reader to have the GPTS shown as a panel along side. Blowup of the transitions would also provide the reader with a clearer understanding of the transitional interval and how precisely it is or is not defined.

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11. Why does Parry (1980) line work better that Dunlop's updated versions

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