Earlier rock magnetic studies on sediments form Lake El'gygytgyn showed large variations in susceptibility in core PG1351. High values of susceptibility were correlated with a higher influx of volcanic material from the catchment, but low values could not be explained by a dilution effect. This study presents a detailed analysis of rock magnetic properties of sediments in core LZ1029-7 through the lower Holocene, Last Glacial Maximum (LGM) and upper MIS3, in which a major fluctuation in susceptibility is recorded. The authors try to use changes in magnetic mineralogy to argue for reductive dissolution of iron oxides in the interval of low susceptibility. They show that there is an agreement between the variation in susceptibility and total organic carbon (TOC), but conclude that more detailed work is needed to clarify the connection between terrestrial input and preservation of the oxides.

General comments:
Several magnetic parameters are used to examine change in the composition, concentration and grain size of the iron mineralogy. The parameters that the authors have chosen should be able to elucidate changes in the iron mineralogy. Magnetic susceptibility will be influenced by both ferromagnetic (s.l.) and paramagnetic iron, whereas remanent parameters at room temperature will only be due to the ferromagnetic phases. Hysteresis properties are used to help delineate composition, grain size and concentration of the ferromagnetic minerals in the short core. These measurements suggest that the composition of the iron oxides is more or less constant for the samples, which were analyzed. Hysteresis measurements also provide a measure of the high-field susceptibility, which can be attributed to the paramagnetic minerals. I have several questions about how this was calculated in the specific comments. Frequency dependent susceptibility is used to estimate the relative concentration of superparamgentic ferromagnetic minerals, although here I have a question for the authors about the method, which they used (see specific comments). Low temperature remanence was observed to aid in the identification of paramagnetic iron phases that have undergone magnetic ordering at low temperature. How unique the identification is, is discussed in the specific comments. In conclusion, all of these methods should help establish what changes are occurring in iron mineralogy, but these changes still need to be assigned to a physical process. It is unclear in the end, what conclusions can be drawn about these processes from the data set.

The specific comments below are points that the authors should consider when revising their manuscript. It is not clear exactly how data was acquired with some methods therefore more information is requested. Other comments are directly related to data interpretation. There are a series of minor or technical corrections listed at the end, which need to be made.

Specific comments:
1. There is some confusion in the manuscript in figures due to the use of χ_{hf} to indicate high frequency susceptibility, but also high-field susceptibility. It is not clear how these terms were derived, which opens several questions about the results. The following are questions about the "definition" and then interpretation or display of results.
    - Low-frequency susceptibility (χ_{lf}) is measured on an AGICO KLY-2 susceptibility bridge. This has a frequency of 920 Hz. The authors state that the χ_{hf} is measured at a frequency of 9200 Hz. How was this measured? Was AC susceptibility measured on the MPMS at a frequency of 9200 Hz? How accurate is this comparison, if one is using different sample size? Or was some other method used?
    - The formula used to calculate χ_{fd} is standard, but the values shown in Figure 9c are extremely high, one may say unrealistically high. If there is a mistake in how this was calculated, then the entire interpretation of superparamagnetic grain size is incorrect, and the discussion needs to be modified.
    - High-field susceptibility is calculated from the high-field (linear) part of the hysteresis loops. From the eight samples shown in Fig. 7, one can make a crude calculation of the high field slope, and this is fairly similar for all samples independent of whether they come from the high, low or transitional bulk

susceptibility areas – the only exceptions are LZ1029-7-49 and PG1351 - 585, whose slopes are higher. This would suggest that paramagnetic susceptibility is rather constant, at least for the samples illustrated, which is interesting if reductive dissolution is occurring. *A priori*, one would think that the paramagnetic susceptibility should increase in sections where reductive dissolution occurs.

- Fig.10: Forster et al. (1994) used this plot based on frequency dependent susceptibility, and not field dependent susceptibility. Therefore one needs to plot the difference between the measurement made at 970 Hz on the KLY-2 susceptibility bridge fand the measurement made at 9700 Hz. The intercept on the low-field axis then gives the frequency independent fraction. Forster further measured his samples in three temperatures: room temperature, dry ice and liquid nitrogen, and see a progressive decrease in the SP contribution, as should be expected. This is an excellent test to verify if the differences between low- and high frequency are actually arising from SP grains. The authors may want to consider trying this test on their samples.
- The authors state that they measured AC susceptibility at the IRM (Univ. Minnesota). If they have AC susceptibility, plotting the real and particularly imaginary parts as a function of temperature would be a good test for SP. One should see SP blocking in with deceasing temperature, hence a decrease in the imaginary susceptibility. Another parameter that could be interesting to see would be ARM/IRM, which is sensitive to grain size change if composition is uniform, but this would require additional measurements

2. Hysteresis parameters – remanent and coercivity ratios: variations in the magnetization and coercivity ratios are very small and show that the magnetic mineralogy is very constant in composition (perhaps one sample has a slightly lower coercivity ratio). The values are exactly what Day predicted for his linear mixing model, as shown in Dunlop (2002). These results also suggest that there is not a large difference in magnetic grain size, which needs to be considered when invoking a reductive dissolution model. Therefore, the rock magnetic data does not support reductive dissolution, at least based on the data shown. Taken alone, they suggest that fluctuations are purely due to fluctuations in concentration. This conclusion, however, does is difficult to reconcile with the sedimentation rate, which appears to be rather similar in the LGM and Holocene.
3. The low temperature remanence experiments nicely demonstrate the presence of magnetite in all samples. Interpretation of other phases is highly speculative, and I would question whether rhodochrosite, siderite or vivianite are responsible for the observed increase in remanence. Friederichs et al. (2003) showed in their remanence vs temperature curves that the bifurcation point between FC and ZFC treatment was at or just a little under the temperature in which magnetic ordering starts. This is also the temperature in which the inverse susceptibility departs from linear behaviour. Looking at the ZFC and FC curves in Fig. 8, the bifurcation between the ZFC and FC curves occurs at a much higher temperature than the ordering temperatures of rhodochrosite, siderite and vivianite. This is particularly well seen in the derivative curves, where it appears that the bifurcation point is close to the Verwey transition. I do not know what this would mean, but the behaviour is not typical for a paramagnetic mineral that starts to undergo magnetic ordering.
   Another possible candidate could be Fe-hydroxide, which may show a range of blocking temperatures, dependent on clustering, particle size, or degree of crystallinity. An increase in the amount of iron hydroxides could be expected if magnetite is reductively dissolved.
4. In general it is interesting that the values of susceptibility and other parameters in MIS3 and the Holocene are very similar – a point on which the authors may want to comment.

Minor comments or technical comments:

pg 4566

Abstract

line 8: remove "an", provide ~~an~~ insight . . .

line 12: Hysteresis parameters ~~defined~~ indicate that the majority of magnetic minerals . . .

Introduction

line 25: use of word "magnetic" minerals. All minerals are magnetic. If the authors are implying "ferromagnetic" minerals, then this would be a better adjective. (see also line 9, page 4567)

pg. 4567

lines 2, 3, 4, 5, 7: following references are not in reference list, or are not properly cited:
Evans 2001; Maher, 1992 (should be Maher and Thompson, 1992), Nawrocki et al., 1996; Vlag, 1999; Evans, 1998; Evans, 2003 (should be Evans and Heller, 2003?)

pg. 4568

Background

line 17: add when impact occurred at this point in text

line 22: when did formation start? was the lake ice free for past 3.6 Myr?

line 17: Belyi and Chereshnev, 1993 missing in reference list

line 25: Melles et al., 2005 missing in reference list

pg. 4569

line 1: – the core under study here – It is not clear to which core is being referred, suggest rephrasing – the latter being the core under study here –

General Geology

lines 9 -10: replace "–" with commas; add comma Rock types, including . . .

line 13: replace "explained" by "described"

line18: replace "vast majority" with "major source"

line 24: remove "at Lake El'gygytgyn"

line 25: Nolan et al., 2003 missing in reference list

line 26: add references at the end of the sentence

pg. 4570

Previous magnetic analyses

line 10: replace "occurrence of" with "erosional input from"

line 21: replace "revisit" with "re-examine"

pg. 4571

Lake sediment core LZ-2907-7

line 21: replace "–" with commas, and replace "repeat" with "resample"

pg. 4572

Chronology

line 8: remove . . . ~~, one of which . . . Fig. 4)~~.

line 10: better to use pers. comm.?

pg. 4573

Hysteresis

line 20: replace "collected" with "measured"

pg. 4574
Low temperature magnetic properties
lines 2-3: replace "–" with commas
lines 4-5:What field was applied to give the samples and IRM before cooling, was it also 5T?
line 10: Do you really mean 2.5 mT and not 2.5 T for FC experiments?

Organic geochemistry
line 14-15: Do you mean Preliminary analysis of the organic geochemistry was undertaken as a guide for further magnetic sampling?

pg. 4575
Bulk $\delta^{1}3\ C_{org}$
lines 7-8: replace "evaporated to dryness" with "dried by evaporation" or simply "dried"

Results
lines 15-18.: How were the results from the automated susceptibility logging correlated to a bulk(?) susceptibility? Or is there no calibration?
lines 23-24 and Fig. 5: with sub-sampling, one is measuring discrete values. In this case, one should plot the data as discrete points, i.e. do not connect the points, but leave as a visible symbol. It does not make sense to say the data is "smoothed" – there is no data in between. Here you should only comment on the fact that the discrete measurements show the same trend as the continuous measurements

pg. 4576
line 5: Dunlop did not explain or propose parameters, he only modified the boundaries between the SD/PSD;MD fields.
line 12: 0.1–20μm (micro not nano)

pg. 4577
line 6: Lehmann et al., 2002 missing in reference list

Discussion
lines 21-24: The values do not vary greatly among the samples so one should write that the coercivity and remanent ratios are similar and fall in the PSD range of grain sizes, but may indicate a mix of SD and MD grain sizes as shown by mixing curves for TM60 titanomagnetite in Dunlop, 2002 (fig. 12 cont in Dunlop paper).

pg. 4578
line 6: there is no figure 12; also see specific comment with respect to Fig. 10. What you plot in Fig. 10 is not related to frequency dependent susceptibility.
line 7: Forster et al., 1994 missing from reference list.
line 26: replace "lessening effect" with "decrease in erosional input"

pg.4580
lines 1-15: a plot of χ/TOC may be interesting to show to look for variation or lack of it – could show this rather than Fig. 9a.
lines 17-18: It is not easy to see Morin transitions in sediments when monitoring susceptibility versus temperature, so does not prove that there is no hematite. But I agree that if hematite is present, it is only a minor phase.
line 29: Wolfer etal., 2011 missing in reference list

References:
Demory et al.: check for extra space after colon in title
Geiss et al.: journal name Sci.
Langereis et al.: Mediterranean should be capitalized
Maher: Quaternary should be capitalized

Figures

General comment: font size is extremely small on several figures. Is it not possible to increase the size?

Figure 1: figure 1a is identical to the one used in Nowaczyk et al., 2002. Should it not be cited in figure caption?

Figure 3. Add symbols where you actually have an age determination – it is not clear how many tie points there are.?

Figure 5: add discrete points rather than line for individual samples

Figure 7: one only needs to include Mass magnetization rather than the two different scales. This and Figure 8 are good examples of unreadable plots.

Fig. 9. Have reversed labels for magnetization and coercivity ratios in Figure 9a. Caption for Figure 9c is missing. Figure 9a is not that informative. One could just as easily label point in Fig. 6 or label the cloud of points and then the outliers individually.

Figure 10. see specific comment