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Interactive comment on "Climatic control on the occurrence of high-coercivity magnetic minerals and preservation of greigite in a 640 ka sediment sequence from Lake Ohrid (Balkans)" *by* J. Just et al.

Anonymous Referee #1

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This manuscript details magnetic mineral measurements from the sediments of Lake Ohrid, which serves as proxies for paleoenvironmental and paleoclimatic change. The new cores from Lake Ohrid comprise one of the longest continuous records from Europe, and the analyses and submission advance our understanding of the regional glacial-interglacial climate and environmental history. In particular, the paper outlines evidence for changes in lake mixing and soil erosion processes related to glacial-interglacial changes in temperature, highlighting variability in the relative intensity of different glacials through time. This is an important contribution to our understanding

of regional climate history. The paper further advances our understanding of the iron (paleo)biogeochemistry of deep lake sediments, an important contribution to paleolimnology and biogeochemistry. As such, I feel the paper is suitable for publication in BGD.

I have a number of comments and several more minor comments that I hope the authors will use to improve the paper, detailed below.

Primary comments: The most obvious feature in the data is the large contrast between Unit 1 and Unit 2. This appears in all proxies, and seems linked to changes in the abundance of greigite (accepting the authors' arguments) and the grain size of magnetite. Despite the prominence of the Unit1/Unit 2 shift, after reading the paper it is not at all clear what causes this transition. Page 14228 line 15 indicates that Unit 2 is "overprinted by neo-formation of magnetic minerals", which would suggest that the authors interpret this transition to reflect down diagenesis. Some of the most prominent features of the Unit 1/Unit 2 boundary are the appearance of greigite in Unit 2, but the interpretation of this as "overprinting" is difficult to reconcile with the interpretation advanced in the paper of syndepositional formation of greigite. Moreover, the transition appears quite sharp, and it is not clear how "overprinting" explains the various changes between the different Units. The authors should present a clear explanation for this transition- could changes in sediment or water sources (via tectonics or similar processes) have altered the supply of sulfur to the lake? Or are there other possibilities to explain the variations?

The two proxies for greigite abundance, SIRM/K and GRM/NRM, are not entirely consistent during Unit 2, as discussed on page 14223. The authors conclude that SIRM/K is a more reliable proxy for greigite, but do not explain why. Please explain. Related to this, if SIRM/K is in fact the most reliable proxy for greigite, this somewhat weakens the authors' arguments that increased greigite is climatically controlled. GRM/NRM shows very strong glacial/interglacial relationships. SIRM/K shows much weaker signals- values are higher in interglacials MIS 13 and 15 than in MIS 14, for example, and there are strong peaks at the beginning of interglacials MIS 11 and 9. What explains these patterns?

Line 3, p. 14225. The authors interpret Figure 4C to indicate oxidation of reduced Fe minerals such as pyrite) to magnetite during sample heating of a sample of glacial-age sediment in Unit 1. They then use various calculations of the magnetic properties to show that that the magnetic properties are more indicative of oxidizing than reducing conditions. This interpretation seems robust, except that it does not explain the behavior of the sample in Figure 4C. Can the authors provide an interpretation of these data? It would be very helpful to the authors' argument if high-temperature susceptibility measurements were made on sediments in Unit 2 that they infer do NOT have greigite, to show that they changes they observe are in fact due to greigite and not other high coercivity iron phases.

Line 26-28, p. 14227. The authors suggest that low eccentricity in some "stronger" glacials, such as MIS 10, 12, and 16, drove low primary productivity and TOC accumulation via cooler summer conditions. However, the %TOC data are not incredibly clear on this issue- %TOC appears similar in MIS8 to MIS 10. Moreover, all glacials appear to have %TOC values less than 1%, so I question whether a few tenths of a percent difference in TOC contents is strong enough to drive the inferred changes. Are there other possible explanations?

Technical comments: Line 18, abstract: delete the comma.

Main text Line 3, p. 14217. It is not just magnetotactic bacteria that do this. Fe-reducing bacteria can induce the formation of extracellular magnetite simply by reducing iron.

Line 21-30, p. 14218. It is not really clear how the spatial distribution of elemental concentrations links to the downcore variations in elements from these lines. I suggest deleting lines 23-25, which introduces the confusion.

Line 11, p. 14219. Does XRF-Fe mean X-ray fluorescence intensities of iron? If so,

state so. If not, delete the "Fe".

Line 4, p. 14222. Delete "basically"

Line 15, p. 14220. Francke et al. also provide %carbonate data. Please briefly (a few words) state how TIC, TOC, and TS were measured.

Line 15, p. 14227. What is the evidence that Fe sulfides such as pyrite are present in interglacial sediments?

Line 10, p. 14229. Fine-grained titano-magnetite is likely a small proportion of the terrigenous inputs, not the "main" component. Rephrase.

Figure 1. I suggest including an inset that shows the location of Lake Ohrid within Europe, for readers not familiar with the location and eastern Mediterranean geography.

Interactive comment on Biogeosciences Discuss., 12, 14215, 2015.