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Climate of the Past Discussions

Interactive comment on "Modern sedimentation patterns in Lake El'gygytgyn, NE Russia, derived from surface sediment and inlet streams samples" *by* V. Wennrich et al.

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GENERAL COMMENTS

This paper presents a study of the chemical, mineralogical and textural properties of lake surface sediments and the immediate watershed of Lake El'Gygytgyn in order to better understand the dominating transport mechanisms and processes, the sources and also post depositional alteration controlling the modern sedimentation. This is presented to provide important information for the interpretation of Lake El'gygytgyn paleo-record, a unique and outstanding climate record of the Arctic extending to 3.6 Ma. The authors present a comprehensive set of data that support most of the pa-

per interpretations and conclusions. The information presented here is probably not completely fitting the scope of Climate of the Past but is definitively pertinent for publication in the frame of this special issue about Lake El'Gygytgyn. The overall quality of the paper is good, as well as the level of the English language. Figures are carefully designed but could be improved (see specific comments below). However, the paper needs to discuss more thoroughly the mercury dataset; the suggested interpretation is rather weak and not supported by evidences. Many papers in the literature that outline and discuss high concentration of Hg in Arctic lakes are not taken into consideration here (for instance Outridge et al. (2007)). The only papers mentioned here are related to lake Baikal and one from a temperate lake. Moreover, the paper mixes results with discussion, and I found this way of doing very confusing. It would recommend making a clear differentiation between observations and discussion, which is very important for the proper flow of the manuscript. I therefore strongly suggest to reorganize the paper structure to clearly separate both parts. Finally, the authors announce in the introduction that these results are important for the interpretation of the long paleo-record, but the paper only outlines one implication of their findings that can help to understand the paleorecord (e.g. Elements of group I can be used in sediment cores of Lake El'gygytgyn as an indicator for coarser grain sizes). I suggest making more of these links; it will strengthen the pertinence of the paper and make better links with the rest of the project.

Outridge et al. (2007) Evidence for control of mercury accumulation rates in Canadian High Arctic Lake sediments by variations of aquatic primary productivity. Environmental Science and Technology, 41 (15), pp. 5259-5265)

SPECIFIC COMMENTS

P 2009 – l 25: (. . .) and environmental changes (Vogel et al., 2010).

Is Vogel et al. 2010 really a pertinent reference here?

P2010 – l 15-19: The high potential of Lake El'gygytgyn for globally significant paleo-

climate and -environmental reconstructions is confirmed by numerous studies on the lake sediments formed during the past three glacial/interglacial cycles (Brigham-Grette et al., 2007b; Lozhkin et al., 2007; Melles et al., 2007; Minyuk et al., 2007; Nowaczyk et al., 2007; Swann et al., 2010; Asikainen et al., 2007)

This argument is flawed as the references mentioned here are the ones from the El'gygytgyn Science Party members.

P2010 – l 23: Nolan et al. 2003 is missing in the reference list. Fedorov et al., 2012 is not available now. This latter reference is used many times in the manuscript. This is quite unfortunate, so please refrain citing papers in preparation (there are maybe other pertinent ones that are already available in CPD)

P2012 – l 11-12: (. . .) been suggested (Gurov et al., 2007; Nekrasov, 1963), their lacustrine origin is questionable.

Why this is questionable?

P2012- l 23-24: (. . .) Thus, climate-driven variations in permafrost stability are believed to have a major influence on the lake sediment composition.

I agree that permafrost stability will have a major influence, but I do not see why it would have an influence on the sediment COMPOSITION. I would suggest to write: " (. . .) influence on lake sedimentation."

P 2015 – l20-21: (. . .) which are controlled by transport processes, basin topography, bedrock geology, early diagenetic processes and potential tectonic activity. Thus, the surface sediments provide fundamental data about the dominating transport mechanisms and processes, but also about the sediment sources and post-depositional sediment alteration.

In general, I would agree with this statement, but it is not supported by any evidence from Lake El'gygytgyn, since no evidence has been presented yet. I feel uncomfortable with this way of doing because one can believe that you have preconceived ideas. This

sentence belongs more to a conclusion. Moreover, the second sentence (starting with "Thus") really sounds like circular reasoning. I have the feeling that you need to rewrite this part of the manuscript.

P 2016 – l 15-17: Higher silt content (2–63 μm) is observed at the eastern lake shore (Fig. 3b), with maximum values up to 82.3% at the southeastern edge of the basin near the mouth of creek 49 and 50

I would be very cautious with this observation because, according to the map presented here, there is not a single measurement point made in this entire surface area. Might this be the result of an interpolation artefact?

P 2016 – l 20-23: Thus, the high silt content most likely can be traced back to the high flux of fluvial suspension from this major inlet and its northward drift during northern winds along the eastern shore.

Because of the comment above, one need other evidences to support your interpretation, even if this is making a lot of sense. Is this interpretation supported by sub-bottom profiling observations? It would be interesting to elaborate.

P 2017 – l 1-4: The lack of sand and the selective enrichment of very coarse silt also eliminates an origin for this tongue from turbidity currents, whose deposits are rather abundant in the sediment core of Lake El'gygytgyn but exclude a recent event (Juschus et al., 2009).

I would argue that the presence of a turbidite would not have such a grain size spatial signature. As outlined by Juschus et al. (2009), "T" layers show graded bedding and spatially varying grain-sizes, and this should be reflected differently in your grain-size map. I would also rewrite the end of the sentence as follow: "but such events are absent since [here put a date] ka BP.

P 2017 – l 14-15: (. . .) further transport and dispersion of the material into the deeper lake basin via intra and/or underflows.

How do you know for sure these currents exist? Do you have monitoring evidences of these currents?

P 2017 – l 19-20: (. . .) by the triangular, funnel-shaped morphology of the southern shore of Lake El'gygytgyn (Fig. 1b), (. . .)

I'm sorry, but I can't see the funnel shape of the southern shore in Fig 1b.

P 2017 – l 22: (. . .) NE-SW oriented ridge structure obvious in the bathymetry (Fig. 1b)

Well, the tongue in the grain-size distribution is a lot more pronounced than the topographic high, the latter having a restricted spatial extension.

P 2018 – l 15: one would expect a new (sub) section here. Once again, present the facts first, and then describe what they mean.

P 2018 – l 17-19: you have very clear groups of elements. Why don't you plot the principal components scores in Fig 6?

P 2018 – l 24-25: (. . .) by the relatively homogeneous distribution of group I elements in the sediments of the inlet streams and the source rocks around the lake.

This is rather confusing. On one end, you try to convince the reader that the distribution of group I elements is homogeneous, and on the other hand, you present data that shows significant spatial differences on Fig 6a, and Fig 3e. More specifically, the stream composition seems to be quite variable for K2O as you explain a few lines below. Please try to reconcile the two interpretations.

P 2019- l 11-12: Elements of group I (e.g. K, Na, or Sr) can be used in sediment cores of Lake El'gygytgyn as an indicator for coarser grain sizes (Wennrich et al., 2012; Francke et al., 2012).

It is unfortunate that these papers are not available. This issue should be addressed somehow.

P 2019 – l 16-19: again, you have very clear groups of elements. Again, why don't you plot the principal components scores in Fig 6?

P 2020 – l1: (. . .) and, to some degree, the medium silt (7–16 μm) and total silt fraction (Fig. 5b),

This is a little bit of a stretch to associate median silt and total silt fraction to elemental Group II, especially if you consider the scores of TiO2 and silts.

P 2020 - l 1-3: (. . .) which is also supported by similarities in the spatial distribution patterns of silt (Fig. 2b) and Cr (Fig. 6b).

I would argue that patterns are not similar: indeed differences in Cr concentrations are very important (150 mg kg-1 in the southeastern corner compared to the center of the lake, ca. 30 mg kg-1), compared to the difference in grain size (30% only). The strong gradient in Cr concentration can potentially mask more subtle spatial variations elsewhere. Moreover, you need to be very cautious in your interpretations because of the lack of measurement in this area (as stated above).

P 2020 - l 12: (Francke et al., 2012).

Please do not refer to papers in preparation

P 2020 – l 12-15: Nevertheless, the non-ambiguous results from this study on the surface sediments suggest a particularly high sediment supply to the central lake during cold stages from the southeastern lake catchment (i.e. the Lagerny Creek).

I'm sorry, but I think that your data don't support your interpretation. According to your fig 3, the coring site does not seems to be fed by clays and fine silts rich in Cr, and Fe originating from the southeastern catchment. Moreover, your spatial patterns are only valid for interglacial periods, not for glacial times.

P 2020 – l 23-24: (. . .) suggesting that group III elements are partly bound to organic matter.

Could they also be bound to clay?

P 2020 – l24-26: Iron shows some similarities to the pattern of group III elements, but the signal is highly overprinted by an enhanced input of Fe-bearing minerals from southeastern inlet streams.

If you do not show the Fe map, and do not use this information for further interpretation, this sentence should be removed.

P 2021 – line 1-2: as magnetite (Fe3O4), which occur in considerable amounts of up to 6.4% (Table 1).

Magnetite accounts for less than 1% in Table 1. Please explain this discrepancy.

Table 1: authors differentiate between different types of clays. Usually, clay minerals can only be identified using oriented samples. It seems it is not what you have done here, at least from what it is explained in the methods sections. It is an important point because it is usually very difficult to obtain robust relative proportions of clay minerals versus other minerals. Is there any control that you have done to verify your results? Maybe should you present a few XRDs plots to better convince the readers?

Figure 1: Figure is too small. Location of bedrock samples is quite difficult to see.

Figure 2: I'm not sure Figure 2 brings any significant information. I suggest to remove it.

Figure 3: The figure is nice but it should be larger with larger fonts. According to this figure, the number of sites for elemental analysis and mineralogical analyses is not the same. It should be said somewhere in the methods section. Interpolation method used to draw lines should also be indicated.

Figure 5: Labels are too small; please increase either the size of the figure or the font size, or both.

Figure 6: The figure is nice but it should be larger with larger fonts. Interpolation

method used to draw lines should also be indicated.

TECHNICAL COMMENTS

All sub-figure captions in the text (a, b, c,...) are in low case while they are in capital letters in the figure themselves. Please be consistent Other comments have been made in the annoted pdf file in attachments.

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/8/C942/2012/cpd-8-C942-2012-supplement.pdf

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Interactive comment on Clim. Past Discuss., 8, 2007, 2012.