To Anonymous Referee #1

1/ Line 6, page 2017: authors have mentioned the occurrence of recent heavy storms, which results in re-suspension processes. This type of event can reworke sediments of surface on several centimeters or meters and then, the paleoreconstitution could be wrong. So, what is the thickness of sediment concerned by this phemomena? Did Stratigraphic analyses reveal some sediments resedimentation? No reference was given to confirm this hypothesis.

Due to the (1) the overall very low sedimentation rate of Lake El'gygytgyn of approx. 4-5 cm/ ka, and (2) a high degree of bioturbation of the interglacial sediments, the redeposited material does not really occur as discrete event layer, but rather contribute coarser material to the overall sedimentation in the central lake basin. However, downcore grain-size data (Francke et al., in prep.) yield systematically higher mean grain-sizes during interglacials indicating that the presence/absence of sediment resuspension and redeposition during interglacials/glacials has a major influence on the sedimentation in Lake El'gygytgyn.
We added a few references showing that storm-triggered resuspension/resedimentation is a common process in lakes.

2/ Line 6, page 2019: In what way can you explain this contradiction? (processes?)

We interpret the K_2O enrichment as a result of cryogenic/physical (as mentioned for feldspar) rather then chemical weathering, which usually would lead to a potassium depletion. We changed the text to clarify this point.

3/ Line 20, page 2012: Is it possible to evaluate the erosion rate related to cryogenic weathering of permafrost?

According to our knowledge, the cryogenic erosion rates have not yet be quantified. For further questions on the permafrost history and alluvial fan dynamics of the El'gygytgyn crater we want to refer to the paper of Schwamborn et al. in this special issue. We added the reference.

4/ Fig 3 : Contrary the volume percentages of silts, sands and clays, there are fewer data on feldspars and quartz proportions in the sediment. Why? How the authors can justify the realization of a map while much data are not represented? Some points need to be added.

Indeed, all measured samples have been presented in the map, but as can be seen from the figures, fewer samples have been measured for bulk mineralogy then for grain-size or elemental analyses. This is due to the insufficient amount of material left for bulk mineralogy, which requires a rather big volume, after the latter two methods were employed.

6/ The explanation for high Hg concentrations on the lake is poor and need absolutely to be more developed. Where Hg comes from? Is it anthropogenic? e.g. Is it related to ore-deposits or industries in the region? Is it possible to remobilize an important quantity of Hg only by fault activity? If yes, can you give some reference? You don't explore the possibility that winds can also transport Hg. The explanation for the maximum Hg concentration observed on two sites on the center is far-fetched. Duplicates need to be made.

We extended the discussing of the mercury distribution, and added more references for possible Hg sources. Indeed, eolian transport of anthropogenic Hg sources have been discussed for other remote lakes (e.g., Lorey and Driscoll, 1999; Cannon et al., 2003; Bindler et al., 2001), but due to the high Hg concentrations and its heterogeneous distribution in both inlet stream and lake surface samples, this explanation is rather unlikely. On the other hand, the OCVB is known not only for its gold and silver resources, but also for its mercury ore deposits, especially in OCVBs outer zone (Sidorov et al., 2011). And tectonically-driven mercury mobilization is also known from other Hg deposits (e.g., Jébrak and Hernandez, 1995).

All Hg measurements have been duplicated and yielded similar results. For the map grid, mean values of the duplicate analyses are shown.

* Technical corrections : 1/ligne 25 page 2009: Other Works exist on this field like :
- Allen et al., 1999. Rapid environmental changes in southern Europe during the last glacial period, Nature 400, 740-743.
- Bartov, et al., 2003. Catastrophic arid épisodes in the Eastern Mediterranean linked with the North Atlantic Heinrich events. Geology 31 (5), 439–442.
- Hazan et al., 2005. The late Quaternary limnological history of Lake Kinneret (Sea of Galilee), Israel. Quaternary Research 63 (1), 60–77.
- Gasse et al., 2011. Hydrological variability in northern Levant over the past 250 ka. Climate of the Past 7, 1261-128. Please add them on the Reference paragraph.

We added Allen et al. and Gasse et al., because they indeed provide important records spanning >100 ka, but omitted Bartov et al. and Hazan et al. due to the rather short time span of 40 and 55 ka they discuss, respectively.

2/ Ligne 9, page 2010: Please write 3,58Ma like for the abstract.

changed

3/ ligne 22, page 2017: change NE-SW by NW-SE

changed

To Referee #2 P. Francus

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.

As mentioned earlier, we extended the discussion of the mercury distribution and added more and more variable references. However, a primary production dependant Hg accumulation as mentioned by Outridge et al. (2007) might support the good HG-TOC correlation shown in Fig. 7, but cannot explain the extraordinary Hg maxima in the lake center. Further references for the high geological Hg background within the OCVB and tectonic Hg mobilization are given to strengthen the suggested hypothesis of a neotectonic Hg source.

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 re-organize the paper structure to clearly separate both parts.

We followed the argumentation of P. Francus and re-organized the manuscript to separate the results and the subsequent discussion.

Finally, the authors announce in the introduction that these results are important for the interpretation of the long paleorecord, 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.

We would be happy to add more of these links, but run into the same problems as with the other papers of the special issue in prep. Some more links will become obvious once all paper of the special issue become available.

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

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

The comment is reasonable, and so we replaced Vogel et al. 2010 by some more prominent references (...)(e.g., Allen et al., 1999; Gasse et al., 2011; Cohen, 2012).

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.

We cannot follow this argumentation, since these publications compare the results of the Lake El'gygytgyn record i.e. to the marine isotope stack or the NGRIP ice core record, and thus, demonstrate its global significance.

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)

Indeed, Nolan et al. 2003 is a mis-citation, but is Nolan et al. 2002. We corrected this in the text. Fedorov et al., 2012 is now available in CPD. We updated the reference list.

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

Actually, more recent geomorphological studies (Glushkova and Smirnov, 2005) could not identify typical beech pebbles at these high levels, and suggested a possible periglacial origin of these features. We changed the text to
(...) *their lacustrine origin has not yet been finally confirmed (Glushkova and Smirnov, 2005; Juschus et al., 2011).*

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."

We followed this advice by the reviewer and removed the word 'composition'.

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.

After re-organizing the manuscript, the first sentence is more an introduction to the discussion chapter. We suggest to leave this first sentence since the points listed here just present the titles of the subchapters. But we agree that the second sentence is some sort of unnecessary repetition of the first, and thus, was deleted.

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?

There is indeed a single sample just between creek 49 and 50 with this maximum value of 82.3% silt, but I agree that the sample distribution in the region is rather poor. In the discussion of the elevated silt contents we added the possibility of an interpolation effect.

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.

We discussed the silt cloud more carefully, and added the possibility of an interpolation effect (see above):

(...) The silt cloud along the eastern shore (Fig. 3b) might be the result of the northward drift of this fluvial suspension during northern winds, but due to the poor sample resolution in this region, interpolation artifacts cannot be ruled out.

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.

We follow your argumentation, and changed this sentence to: (...) Turbidity currents, whose deposits are rather abundant in the sediment cores of Lake El'gygytgyn but haven't been observed for the last 3,200 uncal. ^{14}C years, usually cause a typical graded bedding of the sediments with a coarse base and a fine silt/clay cap (Juschus et al., 2009, Kukkonen et al., 2012). The lack of the typical sediment structure of the recent sediments and the missing fine cap (Juschus et al., 2009) also eliminates a possible origin of the coarse tongue from turbidity currents.

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?

We actually don't know for sure about these currents, but the obvious disappearance of the suspension clouds is a strong indication. Unfortunately, we have no thermistor or sedimentation trap data of this event.

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.

We removed the term 'funnel-shaped'.

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.

This was just a speculation because if the similar orientation of both features. So we formulated this sentence very carefully (...) may imply (...).

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

Changed, see above.

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

In principle it would be easy to plot the scores of the PCs instead, but this would make it impossible to show the surface sediment, inlet stram and bedrock data in one figure, since the latter two were not included in the PCA. We added a comment to the methods chapter that the PCA were run on surface sediment data only.

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.

Right, this looks quiet confusing. But most of this irritation can be traced back to a misleading detail in figs. 3 and 6. Since we haven't analysed every of the 50 inlets, we plotted the ones that have been measured, with a thicker line style according to the colour code of the legends, but left the other in a thinner style and light blue. Obviously, in the small size the figs were plotted, these thin blue lines can be misinterpreted as light gray, and thus low i.e. K_2O or feldspar values. We added a comment to the figure caption and increased the line width of the analysed samples to make it easier to differentiate. Indeed, K_2O concentrations of the inlet are rather homogeneous, but the bedrock samples show a clear dependency to the petrology. We extended the explanation in the results chapter in the following way.

(...) Lower K_2O concentrations are also indicated at the southeastern shore, but are represented only by a single value (Fig. 6a). Bedrock samples show a clear differentiation of group I elements in the different formations, with low K_2O concentrations of 1.2-2.2 % in the basaltic to andesitic rocks of the Koekvun' Formation, but distinct higher values in the rhyolithic source rocks of the Pykarvaam (3.3-4.4 %) and Ergyvaam Formations (4.7 %; Fig. 6a). In contrast, the inlet stream samples show a rather homogeneous distribution of group I elements, with rather high K_2O concentrations ranging between 4.5 and 8.5 %.

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.

This is a general problem of cross citations within this kind of special issues. Earlier manuscripts in theory cannot cite any of the other paper within the special issue until they are submitted. We changed all the papers in our special issue not yet available in CPD to "Climate of the Past, this issue, in prep.". But it needs a decision of the editorial board how to handle citations of papers that are postponed for some reasons.

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?

See explanation above.

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.

We agree that the correspondence to the medium silt and total silt fraction is rather vague, but we included total silt to compare it to the grain-size distribution patterns, which, in our opinion, is obvious.

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).

We modified this paragraph to discuss both similarities and differences in the signals, especially with respect to possible interpolations artefacts.

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

Please do not refer to papers in preparation

Changed to (Francke et al., in prep.). See explanation above.

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.

Your comment sounds reasonable, and so we removed the questionable sentence.

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

Could they also be bound to clay?

You're right, incorporation of heavy metal into crystal lattices of clays is also a common process, and we added this option and some references (Lepane et al., 2007).

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.

We removed this sentence.

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.

The structure of the sentence is misleading, and thus, was changed. The 6.4% actually refer to Fe-oxides and hydroxides in total, and magnetite was just mentioned as one example. But since magnetite occur as a separate column in Table 1, this doesn't make sense. And the maximum value of 6.4% is right, but in table 4 samples from the 4 lake sections were averaged leading to the lower value of 4.4%. So we removed the reference to table 1 at this point. To clarify this, we also added the Changed the table caption of Tab. 1 to

(...) Averaged bulk mineralogy composition.

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?

As mentioned in the methods chapter, we used the quantification method according to Vogt et al., 2002, which is not a single-peak but a full-pattern method that can indeed differentiate between different clay minerals. For further details on the method we want to refer to this publication.

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

We increased the figure size so all details are clearly visible.

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

We prefer to leave this figure to better illustrate the sample type.

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.

Indeed, fewer samples have been measured for bulk mineralogy then for grain-size or elemental analyses. We added a comment to the methods chapter, and added an explanation of the interpolation method as well.

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

Changed

Figure 6: The figure is nice but it should be larger with larger fonts. Interpolation method used to draw lines should also be indicated.

Font size changed, and the interpolation method was added in the method chapter.

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

Changed to lower case

References to the author comment:

Bindler, R., Renberg, I., Appleby, P. G., Anderson, N. J., and Rose, N. L.: Mercury Accumulation Rates and Spatial Patterns in Lake Sediments from West Greenland: A Coast to Ice Margin Transect, Environmental Science & Technology, 35, 1736-1741, doi: 10.1021/es0002868, 2001.

Cannon, W. F., Dean, W. E., and Bullock, J. H.: Effects of Holocene climate change on mercury deposition in Elk Lake, Minnesota: The importance of eolian transport in the mercury cycle, Geology, 31, 187-190, 2003.

Glushkova, O., and Smirnov, V.: Highest Lake Terraces, in: The expedition El´gygytgyn Lake 2003 (Siberian Arctic), edited by: Melles, M., Minyuk, P., Brigham-Grette, J., and Juschus, O., Reports on Polar and Marine Research, 85–88, 2005.

Jébrak, M., and Hernandez, A.: Tectonic deposition of mercury in the Almadén district, Las Cuevas deposit, Spain, Mineralium Deposita, 30, 413-423, doi: 10.1007/bf00196401, 1995.

Lepane, V., Varvas, M., Viitak, A., Alliksaar, T., and Heinsalu, A.: Sedimentary record of heavy metals in Lake Rõuge Liinjärv, southern Estonia, 2007, 221-232

Lorey, P., and Driscoll, C. T.: Historical Trends of Mercury Deposition in Adirondack Lakes, Environmental Science & Technology, 33, 718-722, doi: 10.1021/es9800277, 1999.

Sidorov, A., Chekhov, A., Volkov, A., and Alekseev, V.: Metallogeny of the inner and outer zones of the Okhotsk-Chukotsk volcanogenic belt, Doklady Earth Sciences, 439, 949-954, doi: 10.1134/s1028334x11060122, 2011.