Dear Editor,

In the following we're responding to all comments and suggestions of you and the two reviewers, and explain relevant changes in the revised versions. For details see the detailed comments below. Our comments are marked in red.

Based on the your and P. Fawcett's comments, we will add some blow-up figures to better illustrate changes on glacial/interglacial timescales. Furthermore, we will run time-series analyses on the grain-size and Rb/Sr data to strengthen the discussion of the periodicity change during the MPT. In the methods chapter, we will add descriptions of the methodology of the grain-size and TN measurements missed in the previous version.

Kind regards,

Volker Wennrich

Editor comments:
Dear Authors:

To adequately address the comments below I think the paper will need some additional figures and additional time-series analyses need to be carried out and/or reported in the results.

We follow the editors advice and will included additional blow-up figures of selected proxies of the past 1 Ma to better illustrate glacial/interglacial changes. Furthermore, we will add a time-series analysis of the grain-size and Rb/Sr records to illustrate changes in the periodicity at the MPT.

Specific comments:

1. I found it quite difficult to visually follow in Figures 3, 4, and 5 some of the descriptions of the results in reference to the changes in multiple variables between glacial and interglacial conditions. This is simply a product of the very long record and high temporal resolution of the data set. Consequently I recommend adding at least one additional figure that shows only a subset of the data, perhaps a few hundred thousand years, or enough time to span a few representative glacial-interglacial cycles. This would allow the reader to more easily discriminate the changes that are taking place on these time scales.

   We will add additional figures with variations of selected elements and element ratios over the past 1 Ma to enhance the visibility of the glacial interglacial variability.

2. The use of the word "supposed" is overused and not really consistent with the common English usage. I have tried to identify and suggests changes in the text but these should be reviewed by the authors. In many cases "supposed" can be simply eliminated from the sentences in which it occurs. I recommend removing all uses of supposed.

   We follow the suggestion of the editor and replace or eliminated the word "supposed" in case it does not change the meaning.

3. The paper commonly makes the distinction between the Pliocene and Quaternary – one of which is an Epoch or Stage and the other a Period in the geologic time scale. I would choose either one or the other (periods or epochs) when distinguishing time intervals. In other words these time intervals should be referred to as Neogene versus Quaternary or Pliocene versus Pleistocene. I think Pliocene-Pleistocene makes the most sense as this is what is referenced in the title of the paper.

   To not confuse the reader with the mixed use of epoch and period names we changed the term Quaternary to Pleistocene.

4. Introduction: There is not a clear statement of problem or discussion of research questions and their significance. I would like to see more context for why this study is important – be explicit about why Lake E is important in general (and/or why long inorganic geochemical records are important) and how this paper adds to/complements the existing literature. How does this work fit in with existing studies (either focused on Lake E or other quaternary/lacustrine/terrestrial records).

   We will change the last paragraph of the introduction to strengthen the significance and necessity of this paper in the context of the existing publications.

5. Site Description: 3rd paragraph discussion of anoxia in Lake El'gygytgyn is overly simplistic. Dissolved oxygen comes from photosynthesis within the lake, and not just exchange with the atmosphere. (For example some perennial ice covered lakes in the Antarctic dry valleys are supersaturated in DO in surface waters despite having no contact with the atmosphere). Consequently a lack of DO in bottom water is a result of water column stability and non-mixing and the consumption of O_2 in bottom waters due to respiration and oxidation of organic matter.

Although the explanation in the site description was thought to be rather simplistic, we follow your advice and add a more detailed description of the link between perennial ice coverage, organic matter decay, and anoxia.

6. Same paragraph as above: I know the statement about the wind induced two cell current in the lake is based on a reference to prior work, but I still feel like that as written here and elsewhere where this is mentioned it overstates the certainty of this pattern. As I understand it, this pattern is largely inferred from the surface sediment samples whereas there have been no direct current measurements in the lake or numerical simulations demonstrating the occurrence of this pattern. This is not to trying to challenge its existence, I just think it is important to acknowledge that at this point in time the circulation in the lake is largely inferred.

   We will add a comment on the indications for the circulation pattern.

7. Description of composite core profile: This should mention the percent recovery highlighting the good recovery throughout the Pleistocene and less complete recovery of Pliocene sediments.

   We will add the recovery in this chapter.

8. XRF Scanning methodology: Overall I was really interested to read about the empirically determined matrix corrections for Si and look forward to reading more about this in the forthcoming DPER volume. However, why was the correction only applied to the Si measurements? Did you somehow rule out the need for these corrections for the other elements? There needs to be some discussion of why this was done for Si and not the other elements that are discussed here.

   The applicability of the matrix correction procedure for elements other than silicon is widely discussed in the DPER chapter. From that it seems obvious that for the Lake El'gygytgyn sediments the matrix effects on the XRF scanning data are mainly induced and or amplified by the highly variable diatom contents and the porous structure of diatom frustrules. This effects is especially noticeable for Si, and weaker for Al, but further on hardly decreases with increasing atomic number. Thus, Si is the only shown element in this manuscript that requires the matrix correction. We will extend the explanation of the element specific necessity to correct for the matrix effects in this chapter to avoid confusion.

9. Description of Facies: I am not sure that this belongs in your results section since they don't really seem to be a product of this study – you just cite other papers for this source of this info. Consequently I would consider including the description of the facies in the introduction or possibly at the end of section 3.2. If included in the introduction you could use this as a stepping stone for partly explaining why the results of this paper are important. Specifically, the facies were defined primarily based on visual examination of the sediments, whereas this study examines geochemical variations that are associated with changes in sedimentation – thereby helping to determine what within lake or catchment scale processes are producing the different facies.

   We will move the facies description according to the editor's advice to the section 3.2.

10. ITRAX Results: I am a little skeptical of comments that describe particular elements as having more or less variability. Firstly, variability is vague – are you referring to the amplitude or frequency of changes or something else? Furthermore, the amplitude of the variations in the ITRAX data are partly a function of the ITRAX's differing sensitivity to different elements and the varying influence of matrix effects on different elements, not to mention that visualization of "variability" is also largely dependent on how the graph is scaled. Consequently I would temper the usage of "variability" and be more specific in describing the detail/meaning of the variability that is being referenced.

    We will clarify the use of the term 'variability' by more specifically describing variations in frequency and/or amplitude of the signals.

11. Section 4.2.2 (last sentence of first paragraph): All of the minerals described in the surrounding bedrock geology up to this point have been silicate minerals. How then could increased clastic input prior to 3.0 Ma be responsible for diluting the Si content of sediments? If the reduction of Si content was a result of dilution from increased input of non-silicate clastic material then I would expect the Si/Ti ratio to decrease during this interval.

Indeed we see a typical drop not only in Si but also in the Si/Ti ratio prior to 3.2 Ma coincident with elevated values in Ti and K. Simultaneously we see a decrease in the BSi content, although the BSi accumulation rate is at its max (see Meyer-Jacob et al., 2014). This indicates a higher proportion of Si depleted but Ti and K enriched minerals have been flushed into the lake during the Mid-Pliocene, thus diluting the primary signal. In addition, the higher Ca values might have also diluted the Si signal. But we will precise the end of this period in the text from 3.0 to 3.2 Ma, and add a reference to the paper of Meyer-Jacob et al. (2014) to better illustrate this "clastic" dilution.

12. There doesn't seem to be any recognition of the fact that changes in silicon content in the sediments can also be strongly influenced by changes in preservation not just productivity. Perhaps this has been ruled out by other work in the lake, but I still think this should be discussed briefly with reference to the appropriate studies as needed.

    Indeed, changes in the sediment Si content can be influenced by both the production and the dissolution of silicates and especially of fragile diatom frustules in the water column and sediment. But diatom analyses of the past 1.2 Ma (Snyder et al., 2013; this issue) have shown only minor variations in diatom preservation, thus diatom productivity is interpreted as major driver of changes in the silicon content. We will add a comment in the Si discussion.

13. 4.2.3 (second paragraph): This paragraph does not really justify the final sentence – in other words the connection hasn't been made between Mn/Fe ratio and its use a proxy for redox conditions. The sentence in the next paragraph that references Davison 1993 is really what is needed in this paragraph to explain in a general sense the origin of variations in Mn/Fe ratio.

    We agree that this sentence appears strange, and thus, we shift the explaining reference about the higher solubility of Mn to the 2nd paragraph. This clarifies the use of Mn/Fe.

14. 4.2.3 (paragraph 3). Acknowledging the fact that I had a hard time examining the details of these changes on the scale of the figures provided, when I look at Figure 4 it looks to me as though Mn/Fe decreases during glacial intervals as a result of Fe increasing, not because Mn decreases. This observation seems to contradict the interpretation that decreased Mn/Fe during glacial intervals is due to increased solubility of Mn under anoxic conditions. In addition the second half of the paragraph goes on to provide a completely opposite interpretation for some glacial intervals where Mn is enriched due to rhodochrosite formations which is also linked to anoxia. So, first you state the Mn/Fe decreaed because Mn becomes more soluble under anoxic conditions and then you go on state that extreme Mn enrichment is also consistent with anoxia as result of rhodochrosite formation. These two conflicting statements need to be reconciled. Also how were peak glacial phases identified/defined. Are these determinations based on the Mn enrichment? If so what is so different about these events that causes Mn enrichment?

    Indeed, details in the Mn, Fe, and Mn/Fe ratio between glacials and interglacials are hardly visible in the Fig. 4, but zoom-in figures of the past 1 Ma that will be added to the manuscript (as discussed above) will better illustrate these changes. But in general, during peak glacial periods visible in the occurrence of Facies A (blue bars in Fig. 4) Fe is rather elevated, whereas the Mn curve exhibit only minor changes. Fe variations in Lake El'gygytgyn sediments during these peak glacials are known to be determined by the dissolution of magnetite and the post-sedimentary formation of vivianite nodules (e.g., Nowaczyk et al., 2007; Minyuk et al., 2014). Thus, the increased solubility of Mn compared to Fe under anoxic conditions presumably hampers the ability of Mn to form stable minerals in the vivianite stability field, and therefore, has a major influence on the Mn/Fe ratio. The post-sedimentary formation of rhodochrosite in the sediments is still puzzling, but presumably the formation of this mineral is limited to periods of slightly higher Eh values, higher pH values (e.g., Koinig et al., 2003) in combination with the availability of carbonate ions in the pore water due to organic matter decay. We will extend the discussion of Mn/Fe to make our interpretation more coherent for the reader.

15. Interpretation (first paragraph): All of the processes describe d here are dependent on local climatic conditions (not regional or global conditions). Additionally, it doesn't make sense that something being sensitive to global climate makes it useful for reconstructing local conditions (as is stated). Instead you might highlight the fact

that your data set (and others from lake E) indicate that local climate/environmental conditions at Lake E are consistent with regional and global scale changes driven by orbital forcing. Consequently this record is useful for examining conditions in the terrestrial arctic during periods of significant global change (or something else along those lines…)

We follow your advice and will change this paragraph in a way that it becomes clear that the changes we reconstruct are triggered by the local climate, which fits regional and global changes. This will indeed further strengthen the importance of the Lake El'gygytgyn record.

16. Section 5.1 (first paragraph): Note that in a bowl shaped basin the area of deposition increases as the basin is filled in. This focusing of sediment in the center of a bowl shaped depression consequently can result in a vertical sediment accumulation rate that decreases over time even in the absence of any changes in the rate of sediment input to the lake. While the change in accumulation rate in Lake E is likely also driven by changes in sediment input it is worth acknowledging this other factor as well.

    We will add a comment on the possibility of sediment focusing at this point.

17. Section 5.1 (first paragraph) I don't understand the sentence that ends with "hardly masked by detrital dilution". It seems to me that if the Si/Ti ratios and TOC signals are hardly masked by detrital dilution (meaning not very influenced by detrital input) then the Si/Ti and TOC records should mimic the BSi accumulation rate.

    We will change the sentence to clarify that both the Si/Ti ratio and TOC in the early lake phase are strongly influenced by dilution with detrital material, an thus, are not consistent with the BSi accumulation rate.

18. Section 5.1 (first and second paragraphs). Please comment on the idea that calcite accumulation does not necessarily require oversaturation of bottom waters. If the calcite is rapidly buried then it can be preserved before it is dissolved. Thus the calcite accumulation in the early part of the record could be a simple result of an increased accumulation rate. Another factor to consider is that Carbonate solubility is dependent on water temperature and if the lake water was warm enough in the early part of the record calcite could have precipitated directly out of solution in the lake water during summer months, once the lake water temperatures stopped reaching some temperature point in the summer (as a result of the long term cooling trend in the region and a shorter ice-free season) this could preclude the precipitation of calcite and instead calcium and bicarbonate ions are exported from the lake as dissolved ions.

    Indeed the calcite story of Lake El'gygytgyn is still a miracle. Although, observation on the core samples indicate that the calcite occur as a cement of the clastic lacustrine sediments, thus indicating a rather post-depositional origin than a precipitation from the water column. Since the carbonate ion concentration can assumed to be not limited due to organic matter decay at the lake bottom especially in the Pliocene section (as visible in enhanced TOC accumulation rates in Meyer-Jacobs et al. (2014)) we assume the calcite formation to be limited by the availability of Ca. In addition, the pH of the modern bottom water of Lake El'gygytgyn is circumneutral, so we assume carbonate dissolution effect have played only a minor role.

19. Section 5.1 (last sentence of paragraph 2). If according to Sauerbrey et al the MMDs primarily originate within the lake please explain how the occurrence of MMDs is linked to landscape stabilization.

    We will add an explanation on the presumable linkage between landscape stability, sediment availability and flux to the lake, and potential sediment overload as trigger for the MMDs.

20. Section 5.2 (paragraph 2). The interpretation of the BSi content and other variables during peak interglacials does not make sense to me in relationship to the sediment accumulation rate (again this might be influenced by the scale of the graphs and the difficulty of examining fine scale details when the entire record is plotted together). Nonetheless, it looks to me as though during super interglacials the sediment accumulation rate is significantly reduced. In fact many of the super interglacials correspond to minima in the accumulation rate record. This implies that the minima observed in Ti, K, Rb, and Ca are caused by a reduction in clastic input and NOT an increase in productivity and BSi accumulation. If this was purely a case of increased productivity diluting the

clastic signal then I would expect the accumulation rate during super interglacials to actually increase. In addition, if warm climatic conditions promote both productivity within the lake and increased weathering rates, why would interglacials be characterized by decreased accumulation rates – perhaps part of this story is related to preservation of BSi or OC under different conditions.

You're right that many of the super interglacial occur during periods of low to moderate sedimentation rates (not accumulation rate, this is rather important). But if you look into the paper of Meyer-Jacob et al. (2014), it's obvious that although the sedi rate is lower during peak interglacials, the BSi accumulation rate increases, leading to an enhanced dilution. But on the other hand, the denser vegetation cover reconstructed for the super-interglacials (Vogel et al., 2013) in combination with a presumably reduced permafrost activity in the lake catchment might have also caused a reduced influx of detrital clastic material into the lake. We will add this alternative explanation to this paragraph. But as previously mentioned, we can exclude a larger effect of BSi dissolution during the peak interglacials.

21. Section 5.3: The periodicity of the record is not evident in the data as plotted in the included figures – specifically it is not clear if there is a regular period, and if so what that period is or if the periodicity is consistent through the record. There needs to be some reporting of actual time-series analyses within the results section to demonstrate that the periodicity and observed changes are real. I would like to see the results of a wavelet or spectral analysis to actually define dominant periodicities and their significance. This should be addresses by including at least one more figure which shows these results and additional supporting text within the results section. While I think these results are needed before one can draw any conclusions about changes in the frequency of glacial/interglacial variability I am also cognizant of the fact that many of the variables which are being interpreted here are the same data that were used to develop the age model by tuning to the LR05 stack – thus there is likely to be an inherent periodicity that is a function of the tuning and which may not actually be real. I am not sure how best to evaluate the time series in light of this, but I think there absolutely needs to be a more rigorous/quantitative analysis of time series variability before any discussion of periodicities in the record.

Originally, in this paragraph we referred to the periodicity calculated for grain-size data from Lake El'gygytgyn of the past 2.6 Ma (Francke et al., 2013). The change in the periodicity during the MPT will become more obvious in a blow-up figure. But if this is necessary to better illustrate the periodicity, we will add a quantitative time-series analysis of the grain-size and Rb/Sr data. Since neither the Rb/Sr nor the mean grain-size have been used for developing the age model, the interpretation of the periodicity is still valid.

22. Section 5.3 (paragraph 2): The meaning of glacial intensity is very vague and stating that glacial intensity intensifies is also not very informative. Does intensity refer to the duration of the glacial periods, the absolute minimum temperatures experienced during the interval, the size of local or continental ice, something else, or some combination of these and other factors? Please define intensity and be more specific about what changes are actually represented.

Indeed, with the existing data it's rather difficult to determine the major parameters that triggered this 'glacial intensification'. Since the total duration of the glacials has not changed much until the MPT, the signal in the Lake E record presumably mirrors some sort of combination of minimum temperatures that fell below a critical threshold, a prologation of the peak glacial periods, and likely larger variations in the glacial/interglacial temperature amplitudes. These changes in the local climate might have been triggered by an increase of local and/or Northern hemisphere ice caps (e.g., Shuster et al., 2005) during this period. We will extend the explanation in this paragraph.

23. Section 5.3 (paragraph 3): It is not clear what is meant by "diatom concentration as diversity." Concentration and diversity are two very different variables when talking about diatom results. Which is changing, or is it both. Please clarify.

If you check the MS carefully you will recognizes that the text in section 5.3 (paragraph 3) says "diatom concentration and diversity", not "… as diversity". Indeed, Snyder et al. (2013) reported a rise in both the concentration and the diversity in MIS 31 and 11.3.

24. Conclusion 4: Again, the meaning of "glacial intensity" is not clear nor is it clear how it is "intensified"

See above.

25. Conclusion 5: I don't think that you can draw any conclusion about the cyclicity of the data as presented. The periodicity is not evident in the figures as presented nor is there any quantitative analysis of periodicity in the time series.

    See response to question # 21.

26. Table 1. Please clarify the particle size variables – are these size fraction percentages?

    Indeed, the particle size variables used for the correlation matrix are percentages of the given grain-size classes. We will add a explanation to the table caption. Furthermore, we will add a paragraph in the method chapter about the origin of the grain-size data.

27. Figure 1. Change "supposed talik" to "inferred talik"

    We will change this.

28. Figure 3: TIC record is obscured behind Ca record. I would move the LR05 (or is it LR04? It is referred to both ways in different parts of the paper) curve to either the bottom or top of the figure since these are not your results but are plotted for comparison purposes. I would place Ti, Si, Si/Ti plots one over the other since these are all related and it easier to interpret when the are in sequence as opposed to having other curves plotted in between them. Finally, where there are gaps in the data I think it would be accurate to show gaps in the plots as opposed to connecting all of the data points with lines. Specifically I note the interval around 3.0 Ma where I believe there is a significant gap in the record but as plotted it just look likes this was a period of constant data. (This last comment applies also to figures 4 and 5).

    This TIC record is not really obscured but cut at the level consistent with the detection limit of the analytical method. We will re-arrange the graphs according to your advice, and will mark data gaps by bars. The term "LR04 stack" is used in consistence with the original term in the paper by Lisiecki & Raymo (2005).

Review #1 by L. Löwemark:

Review of "Pliocene to Pleistocene climate and environmental history of Lake El'gygytgyn, Far East Russian Arctic, based on high-resolution inorganic geochemistry data" by Wennrich et al. submitted to Climate of the Past. The authors rely primarily on downcore variations in a selection of elements (and element ratios) measured by Itrax XRF-scanning to reconstruct environmental and climatic changes in a composite core from the El'gygytgyn impact crater lake in NE Siberia. The study is well written, the topic suitable for Climate of the Past, and of great interest not only to the paleo-Arctic community, but also to the general paleoclimate community.

Specific comments to the manuscript:

P5902, line 10: I am not a native speaker, but I find the phrase " highly masked" strange. I understand what is meant, but it feels awkward.

We will change the phrase to "overprinted".

P5902, line 27-28: I think one sentence explaining why TOC & TN increase can be attributed to the intensification of the glacial intensity would be appropriate.

We will add some explanation on the relationship of TOC & TN increase and glacial intensity in the introduction.

P5902, Intro, line 13. I think an "is" is missing between "as" and "often"

Will be changed.

P5908 3.4 TOC, TIC, line 11: What is meant by "in aqueous suspension"?

A certain amount (20-50mg) of sediment is suspended in 10 ml of water using a disperser. Both TOC and TIC are measured in the suspension by combustion at 1150° C (TOC) and 650°C after pretreatment with H_3PO_4, respectively. We will add this to the method description.

P5910, line 10: The interpretation of Ti as an Aeolian proxy by Yancheva et al. has been challenged (I think on solid grounds) by Zhou, H., Wang, B.S., Guan, H., Lai, Y.J., You, C.F., Wang, J. and Yang, H.J., 2009. Constraints from strontium and neodymium isotopic ratios and trace elements on the sources of the sediments in Lake Huguang Maar. Quaternary Research, 72(2): 289-300. Yancheva, G., Nowaczyk, N.R., Mingram, J., Dulski, P., Schettler, G., Negendank, J.F.W., Liu, J., Sigman, D.M., Peterson, L.C. and Haug, G.H., 2007. Yancheva et al. reply. Nature, 450(7168): E11-E11. Zhang, D.e. and Lu, L., 2007. Anti-correlation of summer/winter monsoons? Nature, 450(7168): E7-E8. Zhou, H., Guan, H. and Chi, B., 2007. Record of winter monsoon strength. Nature, 450(7168): E10-E11.

We will removed this use of Ti as aolian proxy.

P5912, line 23: change "well correlate" to "correlate well"

Will be changed.

P5912, line 27-28: Awkward sentence: "Further-on, strong short-term fluctuations that can be attributed to glacial/interglacial variations." Something is missing in this sentence.

This sentence will be changed to "Further-on, strong short-term fluctuations can be attributed to glacial/interglacial variations."

P5915, line 3-8: Are the element ratios molar-ratios, weight-ratios, or count-ratios?

The shown ratios are count-ratios.

P5916, line 17: remove “an” before “interglacial/glacial timescales”

Will be removed.

P5917. line 23: what is meant by “hardly masked”?

We will change the sentence to clarify that both the Si/Ti ratio and TOC in the early lake phase are strongly influenced by dilution with detrital material, an thus, are not consistent with the BSi accumulation rate.

P5918, line10: The M2 cooling event has not been mentioned before and isn’t really explained. The manuscript would benefit from a brief paragraph explaining the significance of this cooling event.

We will add a paragraph on the M2 cooling event.

P5918, line 14: “seams” should read “seems”?

Will be changed.

P5920, line 1: What is mean by “addressed” in the sentence “Facies A sediments are addressed to sedimentation processes under a perennial ice cover (Melles et al., 2007, 2011) implying that during MIS 104 for the first time mean annual temperatures at the lake fell below a critical threshold of 5.5±1.0 C below modern that is required to initiate multi-year lake ice and to eliminate oxygen exchange 5 with the atmosphere (Nolan, 2013).”

The lamination of Facies A is interpreted to have been preserved due to sedimentation under a perennial lake ice. We will change “addressed” to “linked”.

P5922, line 17: Why does the sentence start with “Their”? Whose results are meant?

The results of the high-resolution inorganic elemental analyses is meant.

P5923, line 1-2: I don’t quite understand why the permafrost would develop AFTER the cooling event, wouldn’t climate become warmer after a cooling event?

The permafrost onset is actually interpreted to have happened during M2 (will be changed in the text). Surprisingly both the geochemistry and pollen data indicate that the permafrost remained in the lake catchment even under warmer conditions that returned after the M2 event. Likely, the timing of the onset also correspond to the termination of hydrothermal activity in the crater, which can persists up to 250,000 of years as reported from the Ries crater (Arp, G., Kolepka, C., Simon, K., Karius, V., Nolte, N., Hansen, B. T. (2013): New evidence for persistent impact-generated hydrothermal activity in the Miocene Ries impact structure, Germany. Meteoritics & Planetary Science, 48 (12):2491-2516. doi: 10.1111/maps.12235.).

Figure 3: I guess “matric” should read “matrix”?

Will be changed.

Figure 5: I may be helpful to popint out that the scale for Rb/Sr is “upsidedown, as the casual reader may fail to notice this (as I did when I tried to understand why the curves seemed to go in the wrong directions…).

We will notice this in the figure caption.

Review #2 by P. Fawcett:

General Comments

This manuscript describes the paleoclimatic history of Lake El'gygytgyn using geochemical analysis via scanning XRF as the primary proxy analyzed. By analyzing a few critical elements and elemental ratios, the authors document downcore variations in lake productivity, changes in detrital input to the lake (and possibly catchment weathering changes), and variations in lake bottom redox conditions which are related to ice cover, lake depth, and bottom current conditions. Given the long timeframe of the Lake El'gygytgyn core (Pliocene to recent), the authors are able to document significant changes in climate from the Pliocene to the modern, including documentation of the onset of northern hemisphere glacial cycles and then the familiar change in the dominant orbital frequency from 41 ka to 100 ka across the mid-Pleistocene transition. The authors also provide additional evidence for the already documented super-interglacials in the Lake E core, although the elemental ratios used for this do not only occur during these extraordinary warm events. The paper is comprehensive, well written and organized (with some minor English errors), and will be of great interest to the paleoclimate community. In sum, this is an impressive study that is well worth publishing in Climates of the Past.

Specific Comments

In the abstract, both TOC and TN values increase after 1.6 Ma in response to more intense glacial cycles. How is this relationship thought to work? (A sentence or two in the abstract would be helpful related to perennial ice cover and bottom anoxia.)

> As mentioned in the comment to reviewer 2, we will add a short explanation on the relationship of TOC and TN and a glacial intensification.

The exceptionally warm interglacials (e.g. MIS 11c, MIS 31 etc.) are documented by extreme Si/Ti values (along with lows in other elements) and correspond to previously documented super-interglacials (e.g. Melles et al., 2012). However, there are additional interglacials such as MIS 9 where peak Si/Ti values are as high as during the documented super-interglacials but are not labeled as such. (There are a few other examples – one at ca. 2.6 Ma and possibly MIS 17). The manuscript should include some explanation of why these additional intervals of high Si/Ti and/or low values in K, Ti etc. are not also considered to be super-interglacials.

> The super-interglacials are not defined only on geochemical proxies but also on the coincident occurrence of Facies C and exceptional pollen spectra. We will add some explanation on the definition of the super-interglacials.

The elemental abundance and ratio data are presented in their entirety in three figures (figs 3-5), which have a rather small format. Some of the main features are evident in this presentation; however, a lot of detail is lost as more than 3 million years of high resolution data are compressed into a small space. Is there any other way to format the figures (additional panels etc.) that would help to better convey this information? For example, it is hard to see the change from 41k glacial cycles to 100 ka cycles given the timescale.

> As mentioned before, we will add one or two blow-up figures of selected proxies over the past 1 Ma.

In the discussion, there is a vague explanation for changes seen at 1.8 Ma (e.g. p. 5920). While I understand the cooling of the Beringian landmass, it is not clear what you mean by a drop in ocean-land moisture transport. Do you mean less precipitation? If so, what is the postulated mechanism that relates to oceanographic changes reported at this time?

> The speculated drop in ocean-land moisture transport presumably works over a drop in precipitation, but so far we have no direct or pollen-derived indication for this. We will add some more space on potential mechanism in this paragraph.

Technical Comments

In many places in the text, the word “supposed” is used in the context of “is thought to” – I would replace the former with the latter as it removes confusion with the verb “supposed to”. Alternatively, the term “hypothesized” could be used in place of “supposed”.

As already mentioned in the comment to the editor, we will replace “supposed” by “hypothesized” or “though to”.

p. 5902, line 26. “whose results reacts highly sensitive” does not make sense – rephrase this. (i.e whose results are highly sensitive)…

Will be rephrased.

p. 5905, lines 13 and 16 use “were” instead of “where”

Will be changed.

p. 5905, line 27: use “enables non-destructive measurement of... ”

Will be changed.

p. 5907, line 10-11: use “The mathematical model was tuned ... ”

Will be changed.

p. 5907, line 10: use “The total carbon and total inorganic carbon contents... ”

Will be changed.

p. 5907, line 19. When you say “postulated age of the crater”, isn’t this precisely known? (It appears to be given the small error associated with the age.) Why not just say the “age of the crater”.

We will remove the word “postulated”

p. 5909, line 12: Assigned might be better to use than attributed (in terms of facies).

We follow your advice and use “assigned”.

p. 5909, line 20: Here, use “attributed” in place of “addressed”.

Will be changed.

p. 5911, line 22: “supposed to cause a lacking grain-size dependency…” is awkward – try rephrasing.

Will be rephrased.

Line 25: use “rather steadily decreases” in place of “rather constantly decreases”

Will be changed.

p. 5911: Here you give several XRF counts that will not be very meaningful to most readers. Why not describe the trends more qualitatively?

We use the XRF count rates to illustrate the differences between the extraordinary high Ca values prior to 3.25 and the much lower values after 3.25 Ma.

p. 5915, line 28: Use “preferentially” in place of preferably.

Will be changed.

p. 5917, line 14: Use “seen” instead of “visible”.

Will be changed.

Line 23: Do you mean “the signal is masked by detrital dilution”? With the “hardly masked” usage, this is confusing. p. 5920, line 1: Use “related to” in place of “addressed to”.

As already mentioned before, we will change the term to “are strongly influenced”.

p. 5920, line 26: What is the “MS” referred to here? Is it magnetic susceptibility? If so, define more completely.

As defined on p. 5908 l. 22 MS is defined as abbreviation for magnetic susceptibility.