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Dear Sir, dear Madam,

I hereby resubmit our manuscript no. cp-2012-205

„Petrophysical characterization of the lacustrine sediment succession drilled in Lake El'gygytgyn, Far East Russian Arctic

by *A. C. Gebhardt, A. Francke, J. Kück, M. Sauerbrey, F. Niessen, V. Wennrich, and M. Melles*

Revisions have been done following the comments by two anonymous reviewers. Comments of the reviewers are given in italics, response in regular letters:

Reviewer #1:

The paper is written partly not very carefully: there are for instance repeating subtitles: The 4.2 title is the same as 4.3; and 5.1 the same as 5.2, very confusing. I also don't understand the Chapter 5 overall title (variability in lacustrine succession), this is unclear, what is meant here, the entire paper discusses these variations, what is special now in Chapter 5?.

- Yes, I am very embarrassed that I did not notice this obvious mistake. It must have happened when copying my text into the CPD template. Titles are now changed to what they should be.

There are also some sections, which are highly repetitive: For instance (I refer as page/line numbers, but I used a pdf that starts at page 1, the online version starts now at p. 351) 14/20-15/5: 12 lines of total and mostly word-by-word repetition of what has been said higher above in the chapter 2.2! There are also highly inconsistent and repetitive paragraphs: what is now valid? reflection or refractions? Vp? 5/22: '...The lacustrine sediments can be divided into two units by means of refraction data; the upper unit is characterized by a seismic velocity of 1550ms−1 and a thickness of about 170 m, the lower unit by 1650 m s−1 and a variable thickness of 190 m on top of the uplift ring structure to 290 m in the surrounding basin (Gebhardt et al., 2006)'. 14/13: 'Seismic reflection data exhibit that Unit I can be subdivided into an upper, well-stratified Subunit Ia and a lower, 15 more chaotic sedimentary Subunit Ib (Fig. 2). Acoustic velocities are around 1550 to 1650 m s−1 for both Subunits, pointing at unconsolidated sediments. So the authors should reorganize all these issues bit to overcome these structural issues.

I checked these paragraphs and removed any repetition. And I tried to write more clearly what was derived from seismic refraction vs. reflection data. Refraction data was used to subdivide into Units I to III and also showed the

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subdivision of Unit I into Subunits Ia and Ib. With a water depth of ca. 170 m, the multiple masks almost everything below ca. 170 m sediment depth in the seismic reflection data, so Subunit Ib is only visible at the very center of the lake and also not in all profiles. In the profiles where it shows up, however, the distinct change in facies (from a well-layered Ia to a more chaotic Ib) is clearly visible. Acoustic velocities of Subunit Ia originate from both types of data, those of deeper (sub)units Ib, II and III from refraction data only.

My main point to be mentioned is the interpretation of the clusters. There are two kind of clusters, 1-3, and I-IV. This should be more clearly mentioned, that these are two different issues. In general I can follow that these clusters represent somehow distinct classes, but I have my doubts whether they are that important, as they either may show obvious changes that have been extensively described lithologically (for instance I don't need a cluster analysis to distinguish the impact breccia). But there is also to much emphasis given to these clusters, and they may be overinterpreted, some example: I don't agree with the remark 18/5-8: '..that these two sediment types do not differ in their petrophysical characteristics..': and would expect from the redeposited layers higher densities than from pelagic sediments. Are Facies F densities surely not higher than those of pelagic sediments? The authors reduce the term 'petrophysical' to their cluster formation, but it might be cleaner just to discuss the pure physical properties. I also don't agree with the statement on 20/25: just the fact that resistivity is rather stable does not mean that petrophysical properties overall are stable. Density and magnetic susc. show in fact large scatter. The clustering might be correctly done, but is clearly bias, and eventually, the cluster forces the variety of properties into a scheme so that some important differences are lost. So maybe a simple discussion of the properties would also be very useful. The complex Fig. 6 does not really contribute more than was already known from classic lithologic interpretation, which reflects the numerous and extensive references to previously published analytical and interpretational papers of The El'gygytgyn cores.

- I clarified that these are two different sets of clusters. Yes, they do describe clusters that follow the lithological description. Nevertheless, lithological description so far was based mostly on visual description of the cores, along with some measurements of basic parameters. The visual classification not verified before except of the measurements in the pilot core (Frank et al., same special issue). So this current manuscript bridges the gap between visual description and statistically reliable clustering.
- Facies F comprises a wide variety of different mass movement deposits: turbidites, grain-flow deposits, debrites, slumps, and slides. All these deposits are highly diverse in their physical properties. Just to give an example, the turbidites commonly have either high- or low-density basal sandy units (1.3 to 1.6 g/ccm) and generally high-density silty parts (1.5-1.8 g/ccm) and low-density clay tops (1.2-1.4 g/ccm) (Sauerbrey et al., this special issue, in review). Densities are thus highly variable. All things considered, Facies F is not characterized by high densities.

Other key comments: 21/1-6: This statement is wrong. Large catchments also have always the same catchments lithology. The cores show also variable lithology, so again, a same cluster does not mean same lithology! Catchment alone only represents the detrital fraction, we know from lithologic analysis that the authigenic fraction of the sediment is at least as important, or at least more diagnostic on past environmental changes.

- I rewrote this sentence to be more understandable. Large catchments can have different types of lithology in different areas. To give an example, the lithology of around the Nile delta is certainly different from the lithology around the springs of the White and Blue Nile.

Facies D: the statement '... Laminae are characterized by distinct lower boundaries and a coarsening upward sequence from silt to clay with a higher total clay content than in Facies A....' sounds very much like a turbidite to me. The authors mention enhanced fluvial input. Are these flood-induced turbidites, i.e. underflow or hyperpycnal flow deposits?

- Facies D was only briefly described in Brigham-Grette et al., 2013; here it is stated „Repeated deposition of graded silt and clay laminations suggests repeated pulses of sediment delivery to the lake due to variations in fluvial input and stream competency." This facies is not further characterized in Brigham-Grette et al., 2013 (nor anywhere else), and its characterization is not the main focus of our manuscript. Further investigation of this special facies is needed to define its exact depositional regime.

Seismic units II and III are masked by mutliples? But where are they? Or are these lithologic units? (How can you define seismic units, hen they are fully masked by the mutliple?) They are not even shown on the data. Or are they based on refraction data? I also don't see so well the Ia-Ib unit boundary on the seismic figure. The seismic sections should be shown larger with better resolution to give credit to the data. This comment also refers to: 15/10: '... Subunit Ia conformably overlies Subunit Ib with a clear and distinct boundary in between....'. So how is the boundary then recognized? Change in seismic facies?

The identification of seismic units II and III is based on refraction data solely (Gebhardt et al., 2006). In the seismic reflection data, these two units however are masked by the multiple. See also the first paragraph on p.2 of this "letter to the editor" for more explanation.

I am sorry for the low resolution and small size of the seismic profiles. They were definitely reduced by the publisher for CPDs special page size. The initial files were sufficiently large and resolution was high enough. Hopefully there will be a full resolution version of the figures available in the final manuscript.

18/18: K and Th are indicative of clay minerals, not grain size! I cannot follow the discussion of grain size K and Th contents!

- K and Th are certainly indicative of clay minerals, but clay minerals normally are found exclusively in the clay fraction of the sediments. K and Th therefore can be used to trace the clay fraction of the sediment. This is frequently done with downhole logs.

Detailed comments

- for the smaller comments, changed where appropriate and where sentences were not already corrected by Reviewer #2.

10/13: '...were only approximately 33 mm thick, which is beyond of what our Geotek MSCL can measure reliably...' It is surely not ideal to measure GRAPE on split core. But the authors mean with 'beyond' that 33 mm is large enough, correct?. The sentence reads as if the cores were to thin to be measured reliably.

- I mean that the cores were too thin to be measured reliably (in terms of their thickness! Not for the other parameters) on the Geotek. This is why we chose the approach with density measured on the Geotek and thickness on the ITRAX core scanner. I did not reword this sentence.

11/5ff.: But defining the composite section often changes total length of cores as one jumps from one hole to the other. This cannot be compensated by a constant shift. Was that a significant issue?

- Having already done this for another deep drilling project, we were highly aware of this issue. Segments of the composite profile were carefully chosen and "shrunk" with a correction factor if necessary. The composite profile is not longer than the cores of the single holes, so this was no issue in this project. A constant shift was only applied where the pilot core was spliced in (uppermost ca. 5 m) to account for a different water depth at the pilot core and at the deep drilling core site.

14/17: '...Subunit Ia has a relatively flat surface (YOU MEAN THE LAKE FLOOR?) in large parts of the basin, but the bathymetry is sometimes rough in the more proximal areas where mass movement deposits occur frequently in the upper layers or on top of the sediments'. WHAT IS MEANT WITH 'ON TOP OF THE SEDIMENTS?

- this sentence has been reworded.

15/18: How are these faults related to uplift structure? unclear how this structure produces faults.

- added reference

Reviewer #2:

- All hand-written comments/spelling corrections/grammar corrections were included in our manuscript. Thank you for this laborious work, this is very much appreciated and helped to improve the manuscript a lot!

The principal quantitative synthesis approach is a type of cluster analysis (k-means), which is one reasonable approach to quantifying downhole lithology and deriving inferences accordingly (PCAS is more commonly applied but this works fine). The specific purpose of the statistical analysis should be explicitly stated early on in the paper however.

- We agree that PCA would also be appropriate, but we have chosen the approach through cluster analyses for this rather limited dataset. PCA will be carried out once the full geochemical dataset will be published and subsequently available for further (statistical) analyses. We added more emphasis on the reason for choosing statistical tools in this paper.

It is not clear if the seismic reflection data presented were single-fold Bolt airgun records, or multifold GI gun data....this should be clarified.

- This information was added to the figure captions of Figs. 2 and 3.

It would be useful to the reader if the authors could post detailed ages directly onto the seismic reflection profiles at the drill site. A zoomed-in image of reflection seismic data at the drill hole with this age info would be helpful.

- Meanwhile the age model manuscript has been submitted to this same special issue, so we can add more ages. This however was done in existing Fig. 3 where already in the earlier version some age information was shown. A more detailed study between seismic reflection data, high-resolution echosounder data and core/downhole data vs. time will be the focus of a future paper.

The standard approach for directly correlating reflection seismic data to drill holes is to generate synthetic seismograms using density and velocity data. Although their downhole tool failed during operations, velocity data from the whole-core logs should be available, and following data conditioning could be used to tying the drill hole to the seismic data. I recommend this be considered in the context of this paper.

- We completely agree! Unfortunately we neither have acoustic velocities from the downhole logging data, nor did we successfully measure p-wave velocities with the Geotek standard MSCL. So we solely can rely on the seismic velocities derived from both the seismic refraction and the multi-channel seismic reflection data. We are however confident in this approach, because the modeled sediment/bedrock contact from refraction seismic data was well confirmed by findings during drilling. Additionally, we used the core vs. downhole magnetic susceptibility signal in the lower part of the hole where core recovery was lower to tie cores to their exact position in the downhole logs.

The U-peaks are intriguing. Is it possible there is a relationship between U and high-TOC intervals? This cannot be determined from figures as presented....please consider including downcore TOC along with U on Figure. 3.

- TOC values are now shown along with the U percentage in Fig. 3. Unfortunately, in the depth range where enhanced U values were measured, not many TOC data are available. Nevertheless it is visible that enriched U percentages are not accompanied by high TOC values. U likely is not significantly bound to organic material in these parts of the record.

4.2 and 4.3 have identical subtitles (also 5.1 and 5.2).....please change/clarify each section.

Yes, I am quite embarrassed. This must have happened when I copied the text and titles into the CPD template. Titles are now corrected.

The conceptual model of colder periods of high ice cover producing enhanced siliciclastic inputs seems a bit problematic; perhaps given the high resolution of most of these data sets this could be refined?

This conceptual model was established by Melles et al. 2007 and further developed by Melles et al. 2012 using this high-resolution data set. It is not one of the goals of our manuscript to further refine the conceptual model; this will be the issue of future manuscripts that combine a wide range of parameters measured on the El'gygytgyn record such as petrophysical, geochemical and biological parameters. Nevertheless, the conceptual model does not suggest enhanced siliciclastic input during periods of high ice cover, but less dilution by biogenic input, resulting in a higher siliciclastic percentage.

I would be pleased if the manuscript could be considered for publication with these changes made.

Best regards,

Catalina Gebhardt