

Interactive comment on “Chronology of Lake El’gygytyn sediments” by N. R. Nowaczyk et al.

H. W. Arz (Referee)

helge.arz@io-warnemuende.de

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1. Does the paper address relevant scientific questions within the scope of CP?

Constructing a detailed chronology for any paleoenvironmental archive, as in this case the sediments from Lake El’gygytyn, is a major prerequisite for concise paleoclimatic/paleoenvironmental interpretations. The paleomagnetic/multi-proxy-tuning approach presented by the authors definitively represents a relevant scientific contribution for CP

2. Does the paper present novel concepts, ideas, tools, or data?

Accordingly, the ms presents novel age models for the El’gygytyn ICDP cores and provides details on new logging and correlation tools developed by the authors.

3. Are substantial conclusions reached?

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Yes. Conclusions confirm and partly allow for more accurate chronological assignment of paleomagnetic reversals, thepra deposits etc. The results also underline the orbitally controlled environmental history of this region throughout the last 3.6 million years.

4. Are the scientific methods and assumptions valid and clearly outlined?

Yes. However, my impression is that the presentation of the methods are slightly unbalanced. Several data sets used in this paper are from already published papers or manuscripts in preparation and methods are explained on very different levels of detail. Since I am not shure about the policy of CPD concerning the referencing of unpublished (in prep.) contribution this point might have to be considered (e.g. the paleomagnetic data in Haltia and Nowaczyk)as well. I think that the extensive description of the spectrophotometer measurements, and here especially the explanation of colour spaces (XYZ, L*a*b*) including figure 2 are not required for present work since they are well documented elsewhere. Reading chapter 2.3 it is not clear to me, whether the composite has been (re)created for the specific purpose of this paper or has been completed in an ICDP group effort before and documented in detail already elsewhere. If latter is the case one might compress this chapter, if not one should clearly identify the update of the composite with accompanying tables/datasets etc. Proceeding to chapter 3 (results)it is somewath difficult to reach the point of understanding, how the proxy (mag. sus, TOC, color, XRF ratio, bio.silica etc.) to reference records (isotope stack, summer insolation) relationship is working from the mechanistic point of view. I suggest therefore, to explain briefly how the different proxies are interdepending and how they change in response to the reference "forcing". This could be visualized in a schematic graph or table... In a next step the iterative tuning could be developed. Here on page 3072 (lines 10-15) I suggest some minor correction. It is not very surprising that summar insolation that is predominantly controlled by the shortest orbital cycle, the precession, shows stronger variability than the isotope stack. In terms of paleoclimatology this is so obvious that it not necessarily needs to be stated. Continuing in the lines it reads as if there are local insolation minima (which in fact are hemisphere

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wide at this latitude) and other insolation minima (not local?). Deviations from the general expected interdependence are discussed at the end of this chapter. This is rather discussion belonging to the discussion chapter. I completely agree with the comments of Kodama at the point where he suggests time series analyses of the proxy series before and after the detailed tuning. I even would go one step further by suggesting bandpass-filtered data (at the orbital bands) to verify and improve the tuning to the isotope stack and insolation data.

5. Nonetheless, the results are more than sufficient to support the interpretations and conclusions

6. While the description of experiments and calculations is complete and precise, reproduction would require the full accessibility of the proxy records and the >600 tie points, that could be made available either as supplemented data tables or through e.g. pangaea.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes, they do. Here again, one should raise the questions on references to unpublished (in preparation) articles.

8. Does the title clearly reflect the contents of the paper?

Yes. One could add some words to the title to clarify what type of chronology is meant.

9. Does the abstract provide a concise and complete summary?

Yes. In line 9 (page 3063) I would not call the proxies "stratigraphic parameters" but environmental/depositional proxies/parameters that are used for stratigraphic purposes.

10. Is the overall presentation well structured and clear?

Yes, except for the points I raised at point 4 of the review.

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11. Is the language fluent and precise?

Yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Yes. Please see comments at point 4 of the review. In fig. 4 I suggest to plot the isotope stack not overlapping with the insolation curve. In addition, one minor comment to the chapter dealing with the tephra layers. Their updated age assignment presented in table 3 would very much profit from some short discussion/speculation in the text about their origin etc as far as this is possible.

14. Are the number and quality of references appropriate?

Yes. However, the supplements do not contain the references to the originally published proxy records used for the tuning exercise.

15. Is the amount and quality of supplementary material appropriate?

As said already, the supplements could potentially contain also tables with the full proxy records, but at least the stratigraphic tie points and the intervals of the eliminated mass flow/turbidite sequences in the record could be listed.

Interactive comment on Clim. Past Discuss., 9, 3061, 2013.

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