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Biogeosciences Discussions

Interactive comment on "A Holocene record of mercury accumulation in a pristine lake in Southernmost South America (53 S) – climatic and environmental drivers" *by* Y.-M. Hermanns and H. Biester

Y.-M. Hermanns and H. Biester

y-m.hermanns@tu-bs.de

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We thank referee 3 for his comments and answer as follows: RC: referee comment, AR: author response

RC 1: In the materials and methods section, I didn't get how you ended up with a 1414cm core. Could you provide more precision on sediment recovery?

AR 1: The total length of the core stated in the manuscript was incorrect, it should be 1,393cm. The sediment was recovered in three single drives using a 5m long piston

corer ensuring an overlap of about 80 cm at each transition. The overlapping core sections were correlated afterwards using the geochemical records. We will include a more precise description on core recovery in the manuscript.

RC 2: You also mention in the materials and methods that plankton, terrestrial plants, rocks and soils were sampled but it lack a lot of information on how and where this samples were collected. The revised manuscript should precise this information as terrestrial plant for example could be very heterogeneous in C and Hg concentration. Is the sampling occurred within the watershed at several sampling stations or very close to the lake ? What kind of terrestrial plant has been collected, what part? Precise also what do you mean by soil samples (Organic and inorganic horizons ? Humus ?...)

AR 2: Unfortunately a mistake of ours has caused a misunderstanding. Contrary to a previous intention we did not show any element concentrations in plants (neither plankton nor terrestrial plants, see Tab. 3). We simply forgot to delete this in the description of the methods, but we will do that in the revised version of the manuscript. The soils in the catchments were identified as Cambisols and each soil horizon was sampled separately (Ah (including O), Bv and C). We will add this in the method section as well as in Tab. 3.

RC 3: What do you mean by carbonate-free samples.

AR 3: It should mean calcite free samples, will be corrected in the revised version of the manuscript.

RC 4: PCA is generally used to explore a large set of variables or observations to highlight correlation between them and to highlight the most important factors explaining the variance in the data. Your paragraph on this analysis is unclear and should be rewritten in order to explain how you "extracted" geochemical signatures" from this analysis.

AR 4: We will refine this paragraph according to the referee's suggestion. The interpre-

tation of the derived principle components is based on knowledge from the literature about the geochemical behavior of major and trace elements in lake systems.

RC 5: In the results and discussion section, the C/N ratio has been used as a proxy to trace back the source of sedimentary OM. However, when used in linear mixing equations the C/N ratio yields the fraction of terrestrially derived organic N. The N/C ratio should be used to indicate the fraction of terrestrially derived organic carbon when compared with CAR. The authors need to revise the manuscript even if the results and their interpretation maybe quite similar. (see Perdue 2007 Est, Coast. Shelf Sci. 73: 65-72).

AR 5: The referee is right and we will replace C/N by N/C ratios in the revised version of the manuscript.

RC 6: In this section you do not consider diagenesis to explain C/N change along the core.

AR 6: It is known that especially early diagenetic alterations after sediment burial increase (decrease) the C/N (N/C) ratios of organic matter, due to a preferential degradation of N-rich aquatic organic material (e.g. Gälmann et al. 2008, Meyers and Teranes 2011), whereas terrestrial organic matter degradation mostly results in decreasing/increasing C/N (N/C) ratios (Meyers et al. 1995). However, those changes are commonly not large enough to erase original source differences (see literature review by Meyers and Terranes 2001). We will comment on that in the revised manuscript.

RC 7: p.6561 : line23-25. How can you conclude that sediments composition is almost exclusively OM? Please provide a reference on the link between OM content and %C. In this sentence, are you referring at section I (line 24) or section III ?

AR 7: This was just a rough estimation based on the fact that organic material usually consists of 45-55% C and median C concentrations of 37% in the Lake Hambre sediments. However, since several depths show C concentrations down to 12% (which

we attribute to erosion of siliclastic material) we would like to withdraw this statement, since it is not generally valid. In line 24 we refer to section III, but wrote section I by mistake. We will change that.

RC 8: p6563. Line 13. It is unclear why you refer to mean value and then median value.

AR 8: We will replace mean by median.

RC 9:Line 15. How about algae productivity increase, with increasing temperature and nutrient transfer from the watershed?

AR 9: At this point of the manuscript we try to explain the rise in C/N ratios and C concentrations by changing environmental conditions probably caused by a changing climate. Although a response of productivity towards a changing climate is likely, our results do not enable us to evaluate changes in productivity, since we did not measure a proxy reflecting solely aquatic productivity. The rise of the C/N ratios, however, reveals that the rise in the terrestrial OM fluxes clearly surpasses any potential changes in aquatic productivity.

RC 10: p.6565. Lines 17-23: I'm not convinced by your explanation. How you can conclude on C/N, %C variation during 5000yr, only by your observations during a single sampling mission? Can you provide more details (maybe adding literature) on what is happening above Mt. Burney tephra?

AR 10: We agree with the referee that this is just a single "snapshot" and that it is probably not reasonably to use it for an interpretation of 5000 years of sedimentation. We just wanted to confirm what is already shown by the C/N ratios. However, since this it is not necessary we will delete this paragraph. Volcanic eruptions can have severe effects on ecosystems, such as mechanical destructions, soil erosion and acidification and subsequent changes in element fluxes. Many studies reveal that those impacts influence ecosystems usually only tens to hundreds of years, whereas using the exam-

ple of the same Mt Burney tephra which is evident in our record it has been shown that those impacts may last up to 2000 years (Kilian et al. 2006). However, regarding Lake Hambre and its catchment there is no evidence for a severe, long lasting impact of the fallout in the sedimentary record (e.g. no indications of increased soil erosion, leaching of nutrients due to acidification etc.), which suggests that the catchment recovered rapidly. We therefore decided not to comment on that any further in the manuscript. The tephra itself is excluded from any calculations.

RC 11: p.6566. I wonder how you can compare the 3-5 enrichment factor calculated in very recent years (hundreds of years) with a record of several thousand years?

AR 11: We are not sure if we understand the reviewer right, because it is obvious that the calculated changes in Hg accumulation rates are based on sections covering only a few hundred years or less. So the resolution of the record allows comparison to the past 200 years. During the past tens of years great efforts have been made to deduce enrichment factors of Hg in environmental archives in order to get information about anthropogenically induced changes in atmospheric fluxes. Calculation of those enrichment factors is usually based on background values derived from older sediment sections. Those “pre-pollution” Hg accumulation rates are often assumed to be constant through time. However, our study clearly shows that natural Hg accumulation in lake sediments can be highly variable, which should be considered in calculations of enrichment factors.

RC 12: Paragraph 3.4.3. I wonder why you didn’t start this paragraph with the PCA analysis as done in the two above section? This structuration change is a bit confusing.

AR 12: We agree with the referee. The structure will be changed.

RC 13: A graphic representation of the PCA could be nicer. . .

AR 13: Results from PCA analysis are often depicted as quadrant plots showing the distribution of element loadings in the different components. However, those plots usu-

ally just allow to compare two ore three principle components (two- or three- dimensional plots). Since we analyzed 3 sections separately and extracted 3 to 5 principle components each a graphic representation will probably be more confusing. As the PCA data is quite distinct we believe that the information derived from an additional graph will not go beyond of what could be seen in the table and will therefore omit additional graphs.

RC 14: - p6564. Line 26. “Weather” or “whether”?

AR 14: “whether”! Will be changed.

RC 15: I think references should be cited chronologically in the text e.g. (Lamy et al., 2010; Markgraf et al., 2003; Waldmann et al., 2010; Moy et al., 2011)

AR 15: We will change that.

AR 16: - P6567. Line 10-11. “must have caused the Hg the pronounced Hg enrichments”

AR 16: Will be changed in the revised version of the manuscript.

References

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