

We would like, first, to thank the two anonymous reviewers for the time they spent on the reading of our manuscript and for the comments they posted. These comments will be useful for improving a new version of the manuscript.

We apologize for the poor quality (editorial and grammatical forms) of the manuscript, that is partly due to the lack of time in order to respect the dead line of this special issue on « Climate, weather and hydrology of East African Highlands ». **English will be improved through help of an English native speaker. We guess that this poor quality has led to a misunderstanding of the paper objectives. We will then modify its structure.**

The two main objectives are :

- **to present a first set of data relative to trace element chemistry of lake Malawi. As underlined by reviewer 1, there is a lack of published data on this topic. These data do not allow to discuss physico-chemical process but their presentation is relevant in particular to prepare future fieldwork. We agree with the reviewer that Cu, Zn, Pb and Cr data are suspect. We realised a datacrossing with new data that invalidate those presented in this paper.**
- **To compute and discuss a hydrochemical (major elements) budget even if it's done with heterogeneous data (from spatial and temporal point of view). In such environment (far away from a fixed lab, transboundary lake, ...) it is quite impossible to collect complete (spatial and temporal) data (river, water-column). This work shows that even with available heterogeneous data (our dataset completed with bibliographical data) it is possible to build a budget that is not robust (no statistical strength) but that is relevant to characterize such a hydrosystem at a global scale. It has been previously shown that water column is affected by numerous local specificity (Halfman, 1993; Branchu et al., 2005) and by a temporally evolution (Branchu & Bergonzini, 2004; Branchu et al., 2010). As asked by Reviewer 1 we will however discuss budget incertitude due to the heterogeneous used dataset. Such discussion will be performed using Hecky's river data (Hecky et al., 2003), Bootsma's Si cycle (Bootsma et al., 2003), more recent data on Mbaka and Kiwira rivers (Delalande, 2008), ... One of the main argument for being confident about this budget is the crossing of R concentrations (computed from measured river concentrations and flows) and CAT (computed from epilimnetic concentration data).**

You will find below answers to Reviewers specific comments.

« There seems to have been a problem with the analysis of the anions. I would assume that the variability of Cl and probably also of SO₄ in the water column is an artifact. Especially Cl should behave like Na or K with very homogeneous concentrations.»

variability in the water column is linked to the analytical method it is not an artifact. We are confident in the data, the ionic balance is almost equilibrated .

The residence times given for the epilimnion make no sense from a system-analytical point of view. They are dominated by the exchange between the metalimnion and the epilimnion, but this is largely an exchange of ions by identical ions, and an average ion will pass through the epilimnion many times before it leaves the lake. I think the residence times for the epilimnion should be calculated as the total inputs (but only considering the net input from the metalimnion, not the gross exchange as it is done now) divided by the content. Epilimnion residence times would then be on the order of 100 to 200 years rather than 3 to 4 years.

Could you precise your point of view ?

Two rows should be added to table 8, one specifying the net flux of the elements (concentration difference multiplied by exchange coefficient) from the meta- to the epilimnion, and one showing the total imbalance (total outputs minus total inputs). This would allow discussing the accuracy of the budgets and potential missing sinks.

We agree to add these data

« The calculated residence times are five times (or more) higher for all ions than for water (100 years). The authors state that this illustrates the weak exchange between the deep and the surface waters. I don't agree. Conductivity and main ion concentrations in Lake Malawi vary only by a few percent with depth. The difference in the calculated residence times is mainly due to the fact that 80% of the water leaves the lake by evaporation »

It's right

« In the same line, the calculation of CAT for the trace metals seems not to be correct (Page 4389, lines 10 ff). Since ions are concentrated in the lake by evaporation, inflow concentrations must be lower than lake concentrations for these elements, if no other processes are considered. The text says inflow concentrations are three times higher than lake concentrations. Where are the excess ions going in this simple model? »
This is linked to element reactivity in the water column.

« Averages and standard deviations (Tables 2 and 3) don't make much sense for irregularly spaced vertical profile data (except if you assume the variation is due to measurement errors). Volume-weighted averages (and standard deviations) should be presented separately for the epilimnion and the metalimnion. »

OK

« Table 6: Maybe add the recently published data on Lake Kivu (Tassi et al., G3, 2009, doi:10.1029/2008GC002191). »

We agree

« Figure 2: If you want to discuss differences between different profiles (P1 to P3, section 3.6), all three profiles should be shown in these figures

T°C and Conductivity profiles are representative for the Northern area

« Figure 4: I think the axis labels are exchanged. R should be much larger for Si than for CAT, not the other way round. Also the numbers for R do not agree (by about a factor of 2) with those given in Table 8. I suggest to remove Figure 8, to add the CAT values to Table 8, and to check whether the values in the table or in the figure are correct. »

We agree.