

Interactive comment on “Hydrochemistry (major and trace elements) of Lake Malawi (Nyasa), Tanzanian Northern Basin: local versus global considerations” by P. Branchu et al.

Anonymous Referee #1

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This paper presents and analyzes the major and trace element chemistry from three vertical profiles and five tributaries of Northern Lake Malawi. Lake Malawi is one of the largest lakes in the world and the lake with the highest fish diversity. Given its importance, it is amazing that so little is known about the chemistry of this lake. The content of this paper is therefore without doubt an important contribution to filling this gap.

However, the quality of the paper needs to be considerably improved before it can be published. To be honest, I am surprised how the paper could have passed the

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internal review by seven co-authors, and I almost decided to reject it without further comments.

First of all, the manuscript contains a large number of spelling and vocabulary errors, incorrect translations from French, and even some incomplete sentences. Many sentences are not formulated with the required accuracy. Furthermore, the presentation of the results does not follow a clear thread. Just a few examples: the section on river data is placed between sections on the lake data; in the middle of section 3.4, there is a paragraph on the redox gradients that belongs to section 3.3; it does not really make sense to first discuss the chemical budgets of the lake and then calculate the mean tributary concentrations which are required for the budgets, etc. All this makes the manuscript very tedious to read.

The data needs to be checked again. 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. Some of the river data looks suspect: the probability is very low that concentrations in five different rivers are so similar as presented here for Cr, Cu, Zn and Pb. The variability of the other elements is much more typical for what would be expected. It should definitely be checked with the original data whether all the measurements, calculations and data manipulations were performed correctly. If everything is correct, are there any reasonable explanations for these homogeneous concentrations in the catchment? Also the very strong correlations between some of the trace metals in the lake (especially the pairs Cu/Cr and Pb/Ni) despite the fact that they scatter strongly if plotted versus depth, in my opinion rather indicates some artifact in either the measurements or the data manipulation than a true correlation. This needs to be checked.

The methods used for the budget calculations need to be specified more clearly. There should be a table specifying the numbers of samples (both from this publication and from other publications), average concentrations for external sources and rain as well

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as average epilimnion, metalimnion and hypolimnion concentrations. Methods for averaging the inhomogeneous and non-representative datasets should be specified. 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. 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. For the ions that behave more like passive tracers, the outflow should be much more precise than the inflow, because the epilimnion concentrations are rather constant, whereas river concentrations can vary considerably and average inflow concentrations are based only on a limited number of samples. Furthermore, there must be an error in the budget calculations presented in Table 8. It is not possible that the two major anions (Cl and Alk) have residence times of 966 and 733 years when all the major cations (Ca, Na, Mg and K) have residence times on the order of 500 years. The ionic balances have to agree in the budget.

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.

In the same line, the calculation of CAT for the trace metals seems not to be correct

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(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?

In section 6, it should be considered that this whole discussion is only based on five samples (one for each river) taken during the dry season from rivers in the Northern Basin. For some compounds, concentrations in rivers can vary strongly with time, and they could be very different during the dry and the wet season. Five samples are clearly insufficient to estimate the average inflow concentrations with an error of less than a factor 2. Even with monthly samples over a year significant uncertainties of at least 20 to 50% usually remain.

Finally, a few more detailed comments to the tables and the figures:

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.

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

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.

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.

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