

Dear Julie Tolu and co-authors,

First I would like to thank the two reviewers for their extensive review of your manuscript and of course you for your thorough reply. Without going into all the details of the reviews there are a few things I would like to emphasize here a bit more.

In reply to some of the comments of referee one you stated that a more extensive discussion on the Fe, Mn, and Al distributions in the lake falls outside the scope of this study. The scope of this study more or less defined as determining the spatial heterogeneity in the lake indicating that taking a single sample and scaling up to the entire surface area of the lake is not the way to go. I do agree that an extensive discussion is not warranted here, but you do suggest some mechanisms that might play a role and you specify these mechanisms. This makes the argument of the scope of the study invalid, I think, if something falls outside the scope of the work you could argue it should not be mentioned at all. It is not a very good excuse. Therefore I would like to ask you to either make the effort of writing a more extensive discussion on different possible mechanisms or you make it very clear that the mechanisms mentioned are just of few possibilities and that there might be others such as ... (see review and reply).

The other thing I noticed is that the "whole-lake" concept creates some response from both reviewers in different ways. As for the title I agree with the suggested changes. Throughout the manuscript, just realize that you did not analyze the whole lake. Rather than just the typical one sediment sample you analyzed more samples from different sites to get an idea of the heterogeneity. This is very interesting, and provides a much better picture than analyzing just one sample, but you did not analyze the whole lake or even the all the lake sediments. One question I had is what about micro-heterogeneity did you analyze multiple "replicates" from the same sample to see how large the variation is within one sample? 200µg is a relatively small amount.

You suggest that you looked at the whole matrix rather than only specific compounds or classes. And although I agree that pyrolysis might be one of the better tools to do this, the method still has biases and issues like all other analytical methods. If it doesn't fit in your analytical window you don't see it. The discussion about the pyrolysis temperature makes that quite clear, different temperatures show (slightly) different results, so please be careful in how you phrase what your pyrolysis results reflect. That brings me to a comment from referee two about other studies on OM composition studies. Other groups have investigated complex organic matter from lake and other sediments through a whole range of different techniques including pyrolysis (with and without TMAH), lipid extractions, GC and LC mass spectrometry etc etc. I think your approach of analyzing every samples the same way works fine for this manuscript, but I think it is good to put your own work in a larger, historical perspective by shortly mentioning other studies. Pyrolysis has been around for quite some time and it is good that this would be reflected in your reference list.

As referee two mentions, be very careful with steroids and hopanoids, they come very different sources. Looks can be deceiving. Be very careful with assigning different classes and binning compounds together and using only or mainly the NIST database for compound identifications. Mistakes are easily made.

Based on the reviews and responses I think this manuscript can be published in Biogeosciences if the authors make the changes they indicated they would in their response with special attention to the items I mentioned above.

Best regards,

Marcel