

***Interactive comment on* “Effects of climate, fire and vegetation development on Holocene changes in total organic carbon concentration in three boreal forest lakes in northern Sweden” by P. Rosén and D. Hammarlund**

Anonymous Referee #2

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This paper of Rosén and Hammarlund set out to assess the role of different environmental factors (vegetation succession, fires, climate) in controlling the variability of total organic carbon concentrations in three Swedish lakes during Holocene. The paper is principally well written, the data analysis techniques are state-of-the-art, and the conclusions are interesting. Also, the topic of the paper is very important and the need for this type of study is very well legitimized in the paper introduction.

However, I cannot recommend the publication of this paper for the following major reasons:

1) The dating is very uncertain in the studied sequences, for which reason comparisons between the three independent TOC records are very difficult. Clearly, more radiocarbon measurements should be made in order to allow for these comparisons. With very few datings it is also quite hazardous to assume that some of them are ³anomalic² (p. 8/143). In practice, two of the total five radiocarbong datings for Lundsjön are assumed to be wrong!

2) NIRS inferred TOC is determined from all three lakes, but the predictor variables have been analysed for only one of them. With inadequate dating scheme it is practically very challenging, if not impossible, to evaluate the role of different forcing factors in controlling the DOC dynamics if the potential external drivers are not measured from all sites. In addition, it seems that the lake (Makkasjön) that contain both DOC and other explanatory variables (charcoal, vegetation, Di-pH, C/N etc.) is exhibiting a somewhat different story of DOC variability than the rest of the two lakes. It is therefore bitty that predictors have been analysed from this lake only.

3) The environmental setting of the lakes should be described in more detail in order to help the reader to evaluate the results. How much forest, mire etc. is covered by the catchment of each site? Does there exist any independent knowledge (i.e. not inferred from the TOC records of the present study that leads to circular reasoning) when the mires started to develop in their catchments?

4) The paper doe not discuss the role of internal factors (acidification, alkalization - in-lake removal) in impacting the levels of TOC concentration in the studied lakes (e.g. Schindler et al. 2004, Biogeochemistry)

5) Ideally, variance decomposition between different predictor variables or groups of variables (climate-related, vegetation, water quality) using partial canonical ordinations should have been done to assess the controls of various drivers in more quantitative way. This would have naturally presumed all proxies to be determined from all sites.