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***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 #1

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General comments

This manuscript tackles the interesting problem of reconstructing past TOC levels (and the correlated pCO₂) in three lakes using NIRS on the sediment cores and a previously published calibration function. Although the subject is very interesting, I felt the overall interpretative approach was weak. A good example of this is at the beginning of the Results section regarding L. Makkassjon, where an unexpected result (high NIRS-inferred TOC values in the early Holocene) is explained away using a rather weakly supported claim. All throughout the manuscript, the results are couched in a series of

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hand-waving speculative explanations for just about everything. In the end, I was rather unconvinced by the strength of evidence regarding the influence of fire frequency, humidity, mire formation, vegetation development on lake TOC levels.

A second difficulty arises because little attention was given to the statistical analysis of the purported changes in lake TOC. Simple back-of-the-envelope calculations suggest that with the levels of prediction uncertainty generated by the various calibration models that the minimum difference that can be judged as significantly different for pH, TOC, and color are 1.1 pH unit, 4.4 mg/L and 97 mg Pt/L, respectively. In some instances, these cover the entire range of inferred values.

Detailed comments

Page 3, line 46. Land use change would be more appropriate.

Line 47. Add Allochthonous before TOC.

Page 4, lines 76-77. It is unclear to me how the use of multiple proxies will help disentangling the natural correlation between pH and TOC. Explain.

Lines 82-83. All these factors are likely related to one another. How will their respective influence be separated ?

Page 5. Lines 96 and 99. Liming occurred in 1990 but October 1990 is one year after liming ?? Also, what happened to the phosphorus concentration after superphosphate addition in Makkassjon ?

Page 8, lines 168-179. This whole section is copy-pasted from a previous paper. Page 8, lines 171-175. This sentence is unclear. Do the coefficients refer to the correlations between NIRS-inferred lake TOC and sediment LOI, or between NIRS and lake TOC ?

Page 10, line 218. I am uncomfortable with the idea of explaining away an unexpected result with very little evidence for it. Page 10, line 222. Higher pH does not favour high primary production. It is the other way around. High primary production will drive

pH up because of CO₂ consumption.

Page 12, line 257. The implicit assumption in the statement that increased allochthonous TOC (which is >90% DOC) will be reflected in the sediment C :N ratios is that sediments are ultimately composed of flocculated DOC. This may be so but that assumption has to be stated somewhere.

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