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Interactive comment on “The complementary power of pH and lake water organic carbon reconstructions for discerning the influences on surface waters across decadal to millennial time scales” by P. Rosén et al.

J. Smol (Referee)

SmolJ@QueensU.ca

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The complementary power of pH and lake water organic carbon reconstructions for discerning the influences on surface waters across decadal to millennial time scales

P. Rosén, R. Bindler, T. Korsman, T. Mighall, and K. Bishop

This paper is well organized and clearly written. I found that it contains some of the clearest explanations of cultural alkalization (understandably not a term common in North American paleo studies). I believe that this is important work that has been well

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conducted.

My comments are minor.

P. 2400. First line of Abstract, give location of the lake (i.e. southwest Sweden)

P. 2400. The second line of the Abstract, I found the wording a bit awkward. “over which is” – do you really mean “both of which are”. In any event, the sentence needs some editing.

P. 2400. Line 14 of Abstract, encapsulated is not the right work, in my view. Maybe “distinguished”

P. 2400. Opening lines of introduction. It is true DOC has increased in areas, but also decreased with climate warming, such as droughts, etc. Maybe needs an extra sentence there.

p. 2411. References are fine, but a few are a bit out of date. For example, why not refer to Battarbee et al.’s new review on using diatoms to track pH, instead of a reference from almost 2 decades ago?

See: Battarbee, R.W., Charles, D.F., Bigler, C., Cumming, B.F., and Renberg, I. 2010. Diatoms as indicators of lake-water acidity. In: Smol, J.P. and Stoermer, E. F. (Eds.). The Diatoms: Applications for the Environmental and Earth Sciences, 2nd edition. Cambridge University Press. pp. 98-121.

Line 18 on page 2442, I don’t think ANC has yet been defined in the paper.

p. 2444. Line 9. I find this a bit confusing...so did you use also apply a Di-pH model on the newer core for the intervals spanning 1986 - 2003?

p. 2446, line 4. How were these periods identified? By visual inspection? You could delineate diatom zones with CONISS etc, but I don’t think that’s absolutely necessary. But perhaps at least state how the zones were picked out (e.g. visually based on large congruent changes in the different proxies).

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p. 2447, line 19, should this be 35 cm and not 30 cm?

P. 2557, line 8. What is the error associated with the DI-pH model? What is the error with the tri-protic ANC model? Are the values of DI-pH at 5.9 and the lakewater pH of 6.2 (assuming a constant ANC value of $116 \mu\text{eq L}^{-1}$) actually within range of model errors?

p. 2448, lines 7-8, the wording “of species taking advantage of human activities” seems odd to me. Maybe “species more competitive following human disturbances”

p. 2448, line 22, PC2 and PC3 show some of the largest changes here and in the following period (whereas PC1 follows the very early stages of lake development).

p. 2449, title of section 3.4. Looking at the changes in the figures, I would add another zone line lower than 10cm ...maybe around 15-20 cm?- this will match up with changes in Di-pH, PC1, PC2, PC3, ANC, as well as a modest change in NIRS TOC etc.

If this is possible, it would be nice to enlarge (in the vertical direction) the top $\sim 20 - 25$ cm of the profiles as it is difficult to see the most recent and important changes in such a small figure.

p. 2449, line 21. Particularly for this part of the core, it would be better to have a higher resolution scaling of the y-axis here so that the reader could more readily identify where 7 cm (even to 10 cm scaling rather than 25 cm?).

p. 2449, line 25, Again, because of the coarse scaling on both y-axes, it is difficult to identify where this is on the figures.

This peak in Di-pH is a natural spot to delineate a zone of change as suggested in comment above.

p. 2450, line 4 (heading). Understandably, this small zone is difficult to see - particularly in Figs. 2 and 3.

Line 6. This increasing trend in Di-pH is difficult to see even in the larger figure 4.

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p. 2451, line 17. Both autochthonous and allochthonous are mis-spelled. (missing an H in both)

At the bottom of p. 2452 and the top of p. 2453: This is interesting but seems to be a bit of a leap at times in terms of interpreting the inferred biogenic silica increase (based on Si:Ti). Greater diatom abundances from original counted slides – indicating higher benthic diatom production as a response to clearer waters and improved light conditions. I understand that a decrease in TOC could result in improved light conditions etc. but how would a more open forest with an increase in the delivery of base cations (stated in previous paragraph) at this time lead to improved water clarity?

P 2452, line 19. Not clear to me as to how a more actively worked catchment (small scale cutting and agriculture) would result in reduced TOC transport from catchment to lake. Would not these activities increase TOC transport to the lake?

P. 2453, Line 3. An increase in pH - through increased primary production: Do you mean through in-lake alkalinity generation including from increased biological productivity and sulfate reduction processes – e.g. Psenner and Schmidt (1992) and Schindler et al. (1996) etc.

p. 2453, Line 13. Would not one expect that a change to a more coniferous forest might result in increases in DOC and declines in pH?

p. 2453, Line 14, “During” on my copy is missing a “g” – my copy has it spelled as Durin. But in lines 14-23 – not sure about the interpretation here?

p. 2453, Line 21, not totally clear. So land use was more intense prior to 300 years ago? Then post-1900 it increases substantially?

P 2454, line 10. Guaranteed not guarantied

I like the last section on implications – but it should also perhaps include some text that it becomes more complex with recent warming trends.

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References: Some references have only DOI numbers. Authors should probably add the vol and page numbers.

Figures:

To better enable the reader to locate the changes that the authors refer to in the paper (particularly in the more recent sediments), I would suggest increasing the scale of the y-axes. For example, the depth scale is set to every 25 years – even increasing this to every 10 years will greatly aid the reader to better pinpoint the changes discussed in text.

In terms of the zones/periods identified in the stratigraphic profiles: how were these zones realized? Was it through visual inspection? I find that zone 3 is a bit odd in where the line of change was placed. I think that a more natural placement of a zone of change here would be at the peak of ANC, Di-pH etc. ca. 15-20 cm (again it is hard to pinpoint where this occurs given the coarseness of the y-axis scaling. However, I understand that putting a zone here may not correspond to the timing of interest (ca. last 200 years). Perhaps adding this line in addition to the delineation of zone 4?

Although this may be difficult to resolve, it is difficult to see the changes discussed over the last ca. 200 years given the very small scale of the figures (e.g. Figs. 2 and 3). Even the larger Fig. 4 it is very difficult to see the increase in Di-pH in the last period of the record as suggested on in section 3.5 of the results. If there is a way to expand the y-axis for the last ~200 years or so, that might help.

Fig. 4. Need to include in your figure caption “diatom-inferred pH”.

Fig 2. You tell readers that filled circles represent biogenic silica, but not what open circles represent.

Interesting paper.

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