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Interactive comment on "Forcing mechanisms behind variations in total organic carbon (TOC) concentration of lake waters during the past eight centuries – palaeolimnological evidence from southern Sweden" *by* P. Bragée et al.

Anonymous Referee #2

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This study purports to assess the forcing factors underpinning changes in in-lake TOC over the last ca. 800 years in two southern Swedish lakes. The data are interesting but it should be noted that some of them are re-plotted from an early paper and some of the data that are used in that JoPL paper are not re-cycled here, but should have been (some of the XRF geochemistry data). The main additions to the present paper are diatom analyses (and diatom inferred pH), documentary data and some interesting pollen-based land-use modelling. Taken together the data are interesting but their interpretation and treatment are very subjective (flawed in places). The data and their

analysis need to be re-visited in an objective fashion (with accompanying statistical treatment) and the conclusions reconsidered.

As it stands now there is little to recommend publication in the present journal.

I will not offer detailed critical analysis of the manuscript (as it requires very extensive revision) but merely offer some broad, general criticisms of the methods, the data interpretation and the lack of any statistical treatment. I suggest some possible approaches.

First. Data treatment. The authors seem to be unaware of "recent" developments in paleolimnology with regard to stratigraphic data analysis. As a multi-proxy dataset the present data are ideally suited to a critical analysis using a variety of regression approaches. Arguably the simplest is to use redundancy analysis to identify relationships among varaibles (using a response – predictor approach) and then determine their statistical significance. You could also then do some partial analyses (with time as a co-variable) to remove co-varying effects, for example between sulphur deposition and land-use, which is critical to your study. I suspect that you are aware of this approach but in case you are not, some illustrative examples are Hall et al 1999 (L&O), Anderson et al 2006 (Ecosystems), Leavitt et al on Lough Neagh (also in L&O) and more recently McGowan et al in FWB. If you feeling more adventurous, you could also look at recent work by Gavin Simpson on the use of linear modeling in paleolimnology.

Your study is a nice example of the role of multiple stressors on lake ecosystems (land-use/land-cover change, nutrient export, atmospheric deposition, climate) and the analysis/treatment needs to reflect this. It is far too selective and subjective at present.

Second. Errors in paleolimnological data and models. For the limitations of paleo inference models I refer you to Juggins recent (2013) summary. While much of this relates to diatom/microfossil data (see below for a critique of your di-pH) NIR also suffers from the same problem – the models are essentially black boxes and you are not quite sure what you are reconstructing. To take the inferred values (TOC, di-pH) at their face value is ecologically and statistically naive.

Furthermore, the quoted errors on the NIR values are large yet they are not presented nor discussed in relation to the temporal changes presented here. You try to interpret changes in concentration that are not significant. The RDA would help in this regard too.

It could be argued that there only two significant changes in the TOC profiles. Lindhult lake is essentially constant apart from the reduction in the mid-20th century. The changes in Abodasjon are also muted if considered in terms of the possible errors. I don't think you can call the increase from 1800 substantial!

Your lack of understanding of the constraints on inference models in paleolimnology is highlighted by your use of the EDDI data to infer pH, largely without justification. You do not present sufficient data/results for a reader to critically evaluate the model used. As we are not shown the diatom data (I at least could not find it in supplementary information) it is not possible to evaluate the validity of inferring pH from the diatom assemblages in the two study lakes. I suspect that there is a combined strong nutrient/DOC signal in the diatom data which means that the pH inferences are spurious. Read the Juggins paper if you do not understand this. You mentioned Aul. tenella so there is clearly a DOC signal and Aul. ambigua is probably best thought of as responding to nutrients – look at the work of Simola in Finnish lakes disrupted by early land-use change.

It would be good to see the diatom data or are you holding them back for another paper? Whatever, you need to present the PCA or DCA axis sample scores as an indicator of overall assemblages change. Your interpretation of the P/B ratio and the diatom accumulation rates are also simplistic (line 2 on page 19981 – you state diatom concentration peaked, leaving aside that you have present accumulation rates, are these changes significant?) and do not allow for spatial variability within a lake.

You seem unaware of the complex effects of land use change on nutrient (as well DOC) export from watersheds. You could try to use the geochemical P data in the

RDA analysis. A flawed proxy possibly (if sediment redox conditions change) but it is independent of the diatoms and other data.

You should present the bulk dry mass accumulation rate data, as well as the organic Carbon accumulation rate and the minerogenic fraction. If you don't want to do this then why are you presenting diatom accumulation rates? Also, some of the changes in sediment TC are not fully explained in terms of land-use changes and associated nutrient export to the lakes (which would affect the diatoms).

Your use of the TOC export literature is interesting but its use to justify the interpretation is largely subjective given the constraints on the paleolimnological data outlined above.

If you believe in your interpretation of the changes presented in Figure 3 then you need to provide supporting statistical analyses. Your interpretation of changes in sulphur deposition and its effect on lakes does not reflect fully the NIR data presented, in my view. Interestingly, the Pb profiles presented in the JoPL profile are also different in terms of the timing of the increase from the mi-19th century (or not). As the lakes are so close together, they should be similar.

Interactive comment on Biogeosciences Discuss., 10, 19969, 2013.