

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 #1

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This paper presents palaeolimnological data from two lakes in southern Sweden, reconstructing lake TOC concentration, pH, catchment land use and erosion over the past 800 years from sediment proxies. The authors found that TOC concentrations varied during the past centuries in response to land use, and, most interestingly to me, that there was a historical minimum in lake TOC during the period 1940-1980. This puts, for the first time that I am aware of, the recent increase in surface water TOC concentrations (sometimes referred to as “browning”) in a historical context: the recent increase seems to be related mainly to a recovery from acidification, and is historically

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speaking a rebound to more “natural”, previous conditions, instead of a new problem. While recovery from acidification has been put forward as an important explanator of increased TOC concentrations, this is the first time that this factor is shown to be important on longer timescales. The authors however also show that land use change and recent climate changes may contribute to the observed historical patterns.

This topic is very “hot” and intensely debated, as the rising TOC levels in many northern regions have important implications for drinking water production and carbon cycling in inland waters. The present paper will be a very valuable and important addition to our current knowledge.

I found this a very nice and well-written paper: a decent and comprehensive dataset, sound methodology, and the evidence is presented very clearly and discussed appropriately. I have very few comments. I fully recommend publication in Biogeosciences.

Detailed comments I think the main conclusion of the paper could be stated more clearly in the title and abstract. Particularly the title is quite long and sounds very descriptive. Why not something like “Historical TOC concentration minima during peak sulphur deposition in two Swedish lakes”?

Introduction – The VNIRS methods reconstructs past TOC levels, but a lot of the text on page 19972 is about DOC, while only POC will build up sediment. It should be stated briefly how DOC is linked to POC, such as allochthonous import of TOC includes POC, or that allochthonous DOC can flocculate to POC.

Methods

P19974 L24. Which model was used for analysing the Pb-210 data?

P19975 L22. What does ZRAX mean?

P19977 L6. Why was another lake included for the LRA analyses?

Discussion

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P19980 L21-22. This is a bit unclear. What is “increased openness”, how did that affect biomass production of what land use element, and how did that affect TOC levels? Should be rephrased.

P19981 L7. Older work on links between wetlands and TOC are Rasmussen et al. (1989) The humic content of lake water and its relationship to watershed and lake morphometry. Limnol. Oceanogr. 34(7):1336-1343, and Kortelainen P (1993) Content of total organic carbon in Finnish lakes and its relationship to catchment characteristics. Can. J. Fish. Aquat. Sci. 50:1477-1483

P19983 L23. It would be better to state something like “a study of TOC trends in rivers” than “monitoring study” when referring to Erlandsson et al 2010.

P19984 L10. Use “negative correlation” instead of “anti-correlation”. Also, it would be great to actually see the correlation between inferred lake TOC and sulphate deposition over the past 100 years, i.e. to make a scatterplot between TOC and SO₄ based on the data in Figure 3. Such a new Figure would convey the main message of the paper (at least as I see it) very clearly.

P19985 L14-24. Given that TOC mobilization from soils after clear-cutting very likely is a transient, short-term effect, this part could be shortened.

P19987 Heading 5.3. “Brownification” sounds awkward to me, I would prefer “browning”.

P19987 L26. P/B is a new abbreviation, please explain.

P19987 L24-P19988 L19. I found this part of the text quite speculative. For example, how would DOC of small molecular size end up in the sediment record? In order to settle to the sediment, molecules must be large and form particles. Also, are the P/B determinations precise enough to allow for identification of relationships with the small changes in inferred TOC and pH (which themselves have uncertainties)?

P19989 L5-6. This is the key finding of the paper, which has important implications. It

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should appear in the Abstract.

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