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Climate of the Past Discussions

Interactive comment on "Reconstructing past atmospheric circulation changes using oxygen isotopes in lake sediments from Sweden" *by* C. E. Jonsson et al.

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General comments

The paper presents a synthesis of calcite-, diatom- and cellulose-based oxygen isotope records from five lakes across Sweden in order to track regional Holocene climate change across Scandinavia. The manuscript has an excellent discussion on the controls of these isotope-based proxies, which puts them in a modern hydrological context to aid down-core interpretation. The authors put forward two models of climate control on the oxygen isotope records depending on the isotope hydrology of the lake: (1) changes in δ18Op related to atmospheric processes that are recorded in non-

evaporative short residence time lakes and (2) changes in the balance of evaporation over inputs related to effective moisture in lakes that undergo seasonal evaporative enrichment. All data presented are previously published, but this paper represents the first synthesis of these to provide a coherent regional interpretation. They conclude, based on the long-term trend of these oxygen isotope records, atmospheric circulation changed from a zonal to a more meridional airflow over the Holocene i.e. more progressive influence of the Polar air mass. Except, even though the overall decreasing trend in isotope values of Lake Igelsjön is similar to Lake 850 and Tibetanus, it does show opposite variations during the early Holocene, which I feel needs to be discussed further. Short-term variations in the isotope records are thought to be related to changes in the NAO, suggesting the climate over Sweden is strongly controlled by both atmospheric and oceanic circulation. However, the discussion is rather cursory and I'm not convinced by the comparison with the IRD data and suggest the integration of more regional proxy records may bolster the northern European perspective, rather than focusing solely on the North Atlantic.

The paper is extremely well written, although there are some awkward sentences and the diagrams are of a very high standard. I can recommend the paper is accepted for publication, but do have a few comments for the authors to consider.

Technical corrections and specific comments

1611, line 12: change determine to determining

1613, lines 23-27: This short paragraph seems awkward and feels like it has been tacked on to the end of the section. I suggest you integrate it into the main body of the text.

1615, line 2-4: You introduce Swedish examples at the beginning of the paragraph and then for the lakes where carbonate is absent you reference studies that are not located in Sweden at all e.g. Schiff et al., 2009 = Alaska; Jones et al., 2004 = Kola Peninsula; Edwards et al., 1996, 2004 = Canada; Wolfe et al., 2000, 2007 = Russia.

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This is confusing so I suggest you remove these references and only refer to specific Swedish ones.

1615, line 23-25: Secondary isotope exchange – you do not note whether the 2.5 is positive or negative nor do you give the units.

1615, line 25-27: Why would Holzmaar be any different from the lakes in Sweden? Just because there is currently no evidence for it, doesn't suggest its not there. To what extent did Schiff et al., 2009 and Jonsson et al., 2009b investigate this? Was it as thorough as the sediment trap work by Moschen et al., 2006?

1616, line 16: Do you have any evidence for the effects of vegetation succession etc. affecting lake productivity and the subsequent precipitation of minerals? Ref is needed.

1617, line 1: Remove 'that'

1618, line 10: Remove 'as'

1619, line 7: Change (Schiff et al., 2007, 2009) to (Schiff, 2007; Schiff et al., 2009)

1619, line 19: Remove 'the'

1619, line 20: My understanding of lake nutrient dynamics is that there is overturn in the spring when you have diatom blooms and then a second bloom when overturn occurs in the autumn. Conceivably the diatom isotopes could be capturing both the spring and autumn signals.

1621, line 1: How do you know there is an absence of any major changes in δ18Op? Also, I would expect the E/I imprint to be greater than the changes in δ18Op, so how can you tell when which is controlling the δ18O record?

1624, line 1: What about the diminishing Laurentide Ice Sheet – what effect would that have on the δ18Op of the region during the early Holocene?

1643, Fig. 4: I would separate the speleothem record from the lake isotope records,

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looks confusing otherwise.

1644, Fig. 5: Again, I would separate the IRD (if you continue to use it) from the lake isotope records. I think more regional records are required e.g. from pollen, diatom, chironomids to support your isotope case first before broadening the discussion.

Interactive comment on Clim. Past Discuss., 5, 1609, 2009.