Biogeosciences Discuss., 9, C153–C156, 2012
www.biogeosciences-discuss.net/9/C153/2012/

Biogeosciences
Discussions

Interactive comment on "High resolution record of carbon accumulation rates during boreal peatland initiation" *by* I. Florin Pendea and G. L. Chmura

Anonymous Referee #2

Received and published: 23 February 2012

In this manuscript the authors present a new paleoecological record for vegetation change and C accumulation rates during the last century from a wetland on recently-emerged shoreline due to continued isostatic rebound in James Bays, Quebec. The authors then use this record and in particular high apparent C accumulation rates over the last several decades to argue that the isostatic rebound and tidal marsh-to-fen succession in the past could partly contribute to the increased biosphere C storage and decrease in atmospheric CO2 concentration at 10,000 to 7000 year BP.

This is an interesting idea, in particular by focusing on change in peatland C accumulation at decadal scales and on the use of modern analogues in addressing paleo questions. However, there are some major problems with the interpretations of the results and conclusions.

A fundamental process that should be considered in peat accumulation studies is that peat decomposes "rapidly" in the acrotelm (the surface, oxic layer above water table) and only a small proportion (10-20% or so) of biomass produced by photosynthesis would transfer to and accumulate in the deep catotelm (the anoxic layer, permanently waterlogged). So the high apparent observed C rates in recent peat as observed cannot be directly compared with the long-term C accumulation rates of 20-30 gC/m2/yr as cited and emphasized throughout the manuscript. Also, many records even in long Holocene-scale studies show rapid (up to 100s gC/m2/yr) during recent decades, so it is very common to observe apparently high C accumulation rates in the recent decades from peatlands of 10,000 years in age. However, the high amount of C in the acrotelm would oftentimes not sequester in deep peat that would affect CO2 concentration over centuries or millennia.

Also, extrapolation from a single record to a global scale question related to CO2 concentration change would require quantification of the area emerged from isostatic rebound in the early Holocene and how that compares with the area submerged due to sea-level rise at the same time. Is that potential area for wetlands large enough to have noticeable impact on atmospheric CO2 concentration?

I'd suggest that the author focus on presenting a new peatland record from an understudied region, perhaps by focusing on peat accumulation differences between tidal marshes and freshwater fens at decadal time scales. They have a high quality data set. In any case, the acrotelm and decomposition process should be discussed in the manuscript to put the observed C accumulation rates in proper context.

Specific comments (following the page and line # in the BGD manuscript):

Page 1116, line 16: "six times higher": this is not comparable, considering that acrotelm peat hasn't experienced decomposition as the deep catotelm peat used for the calculation of global averages (see comments above).

p. 1116, l.17-18: the conclusion is not supported by the results, as the C accumu-

lation rates are not necessarily high from these newly formed fens (see above) and no quantification is provided about the relative magnitude of these new available land area.

p. 1116, l.23: change to “boreal and subarctic peatlands”

p.1117, l.28: “autocompaction”: probably biological/microbial decomposition is an important (more important) process, than mechanical compaction in fens or bogs. Tidal marshes may be different.

p.1119, l.14: Pendea and Chuma (2012) is not in the references cited.

p.1119, l.19 and l.22-25: why is macrofossil analysis not practical? I think just indicate that it was not done. If you use subsamples for different types of analysis (assuming you have large enough peat samples) as often done, you would not have this sample problem. In any case, the description about not doing macrofossil analysis is no necessary. Indeed, macrofossil data would be very useful for delineating different types of wetlands, in addition to pollen data.

p.1120, l.16: from the formula, I read peat OM (LOI) contains 51.2% C (seem below for discussion of 32% C used).

p.1121, l.3: “atmospheric testing”

p.1121, l.10-18: in this paragraph, the depths for zones are inconsistent with Fig. 2. For example, 25-21 cm for low marsh should be 29-22 cm. Check. Also, as you have age models already, would it be more useful to use ages/dates, rather than depths in discussion/description?

Ages/dates used in the next paragraph are inconsistent with Fig. 2.

p.1122, l.17: Your study actually shows fen peat mass contains 51.2% C (see p. 1120), so the calculations are incorrect here and the cited fen should have high apparent C accumulation rate than indicated.

Figure 2: zone boundaries have different depths and ages/dates as in the text. Check. Ages (yr) should be “Age (yr before 2006)”.

Figure 3: It would be useful to use age/date scale for this diagram. Also, the panels should be plotted side-by-side in one row (for easy comparison), without depth/age axis for each panel (to save space). Is this also form AMC core? If so, indicate so on figure or in the caption.

Interactive comment on Biogeosciences Discuss., 9, 1115, 2012.