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Biogeosciences Discussions

Interactive comment on "Crustal uplifting rate associated with late-Holocene glacial-isostatic rebound at Skallen and Skarvsnes, Lützow-Holm Bay, East Antarctica: evidence of a synchrony in sedimentary and biological facies on geological setting" *by* Y. Takano et al.

Anonymous Referee #1

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This paper describes the Holocene Relative sea (RSL) changes in the Lützow Holm Bay region and its effect on microbial communities. Although the development of RSL curves around the Antarctic margin is crucial to test ice sheet models, the data presented here add little additional information to the existing records from the region and should be published in a more specialised journal (e.g. Antarctic Science, Polar Science).

Specific comments

Measurements of Holocene uplift rates should be accurate and particularly when they are compared with other regions and curves derived using other methods (raised beach data). Diatom based identifications of the marine, freshwater and transition zones in isolation basins have been proven to be robust. In L. Skallen the authors indeed used diatoms, however in L. Oyako only sedimentological and geochemical methods were used. I am willing to believe that the transition occurs at c. 80 cm depth, but there are no data presented in the manuscript to confirm this. Moreover, the relatively high S concentration after transition indeed might suggest that the lake was meromictic (and saline/brackish) after isolation and hence that part of the water column was not in equilibrium with atmospheric CO2. If this is the case, then the 14C dates of this part of the sediment cores should be corrected for the marine reservoir effect as well. Whether or not to correct for the marine reservoir effect has important implications for the calculation of the uplift rate and its comparison with other regions and previous records based upon raised beaches. I therefore strongly recommend that the authors give error bars for the uplift rate, taking into account these unknowns, and analyse the diatoms at the end of the marine zone and in the transition zone.

The geochemical proxies used and their appropriateness to delineate marine from lacustrine sediments should be discussed in more detail, i.e. what is the relevance of changes in the TiO2 and Al2O3 concentration?

The ecological characteristics of L. eleptica are informative with respect to their use in the development of relative sea level curves. However, I missed a direct comparison with the uplift rate derived from isolation basins and that derived from raised beaches. Apparently, uplift rates are more or less similar between both approaches? Apart from the transition from marine to lacustrine conditions, the bottom ages of lake sediment cores in Antarctica are also informative as they allow scientists to estimate the time of deglaciation of the region. Please elaborate whether the bottom sediments were hit during coring or whether coring was stopped before reaching glacial sediments.

p.4353: the description of the DGGE analyses is too vague and should contain at least the dominant taxa present in the lake sediment core. A table with the identity and % of similarity from a BLAST search should be given as well. Were cyanobacteria dominant in the lacustrine sediments? The title of this paragraph suggests they are (phototrophs), but at the bottom of the page it is written that heterotrophic bacteria are dominant (?). Moreover the reason why fossil DNA is used in the present study should be given in the introduction.

p. 4351: Although it is difficult to check the identification of the different diatom species without pictures, I believe some taxa were misidentified. I strongly advice that the taxa are identified using Antarctic diatom floras analysed using up-to-date taxonomic insights (e.g. Sabbe et al. 2003, Cremer et al. 2003, Ohtsuka et al. 2006). Achnanthidium brevipes should be Achnanthes brevipes. Navicula directa is not a freshwater species. In fact, in the Vestfold Hills it occurs in the most saline lakes (Roberts & McMinn 1998).

Technical comments

Table 3-5 are redundant as the data are already included in the figures. Fig.8 can be deleted as well. p. 4346: ...from diatoms, namely siliceous primary... should be ... from diatoms which are important primary producers in Antarctica...

p. 4347: Were the samples heated up to 70 6°C?

p. 4347: state that DNA and diatoms were only analysed in the L. Skallen sediment core

p.4352: there are no estuarine taxa present in the core. Maybe this should be changed into taxa favouring brackish conditions? Moreover, the statement 'All of these taxa can be found in contemporary lake environments in the Skarvsnes area (Hirano, 1983) and in freshwater creeks in the McMurdo Dry Valleys region of Antarctica (Spaulding et al., 2008)' is not correct. Some of these taxa were not reported from East Antarctica

before, suggesting that the diatoms from the sediment cores were incorrectly identified.

p.4352, L. 14-24 is the same paragraph as on the bottom of p. 4351

p.4354: the calculation of the uplift rate is straightforward but described in an overcomplicated way; there is no need to give the formula here. Also Fig. 8 can be deleted.

p.4355: no need to list all the lakes; a comparison with the other East Antarctic regions is enough. However, I suggest the authors also compare their data with uplift rates from other East Antarctic regions (e.g. the Windmill Islands, Southern Victoria Land).

p.4355: The reservoir effect is also introducing errors when dating marine (bulk) sediments.

p.4356: Please elaborate why the published estimate for the reservoir effects (1300 years) was used for the raised beaches, whereas in the sediments cores the regional estimate (1200 years) was used.

Table 2: heading of first column should be 'lake'

Interactive comment on Biogeosciences Discuss., 7, 4341, 2010.