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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.

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First of all, we thank the referee for the constructive reviews (Biogeosciences Discuss., 7, C1806-C1809, 2010: www.biogeosciences-discuss.net/7/C1806/2010/). We sincerely appreciate the comment which helped us to improve this manuscript.

Please find our responses to the general and specific comments below.

Specific comments:

(1) 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.

[Reply] Suggested experiments can only be conducted as part of another study because of current sample limitation. However, in L. Oyako, the profiles of C and N also jumped in 80 cm depth as well as L. Skallen's profile. Moreover, the nitrogen isotopic composition of lacustrine stage (< +0.3 ‰ vs. Air) also drastically changed after marine stage sediment (<+6.7‰ vs. Air).

(2) 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.

[Reply] We also determined 14C date after high S stage (presumably, meromictic stage) to model the age profile. Our age profile of Sk5S is consistent with the data set from Matsumoto et al. (2010). We agree with the suggestion of error bars for the uplift rate. However, we are currently submitting a proposal for a linear approximation

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model showing the plausible range of uplift rate (Figure 9).

Matsumoto-Inoue, G. et al., (2010) Holocene paleolimnological changes in Lake Skallen Oike in the Syowa Station area of Antarctica inferred from organic components in a sediment core (Sk4C-02). Journal of Paleolimnology, 44, 677-693. (Accepted: 7 June 2010)

(3) 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?

[Reply] We inserted in P.4349, L27 “Al2O3/TiO2 and SiO2/TiO2 are informative with respect to source lock element including a hydro-geological surge event timing.”

(4) 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.

[Reply] We stated the name of raised beach “Kizahashi” in P.4345, L14 and Figure 1. We newly add the information of “Kizahashi (see Figure 1)” in Figure 9 caption.

(5) 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.

[Reply] We added the sentence in P.4345, L.20 “The corer equipped with core-catcher was penetrated to the bed-rock of both lakes, i.e. the bottom of the sediment. Matsumoto et al. (2010) also cored to the sediment bottom at a different location within L. Skallen.”

(6) 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

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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.

[Reply] We changed heterotroph bacteria to “other types of bacteria”. We also added the sentence in the introduction, “To identify the ancient community members, we occasionally conducted molecular analysis by DGGE with 16S rRNA.”

(7) 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).

[Reply] Our apologies, we used Achnanthidium brevipes incorrectly, and this is now modified to “Achnanthes brevipes” (i.e. the brackish taxon). With regards terminology, our intention was primarily to distinguish between marine and non-marine diatoms, and thus we see how the use of ’freshwater’ or ’esturine’ is misleading, and this has been revised. With regards Navicula directa we state in the following sentence, “Such taxa are typically brackish...” and it was not our intention to suggest that freshwater conditions had been reached. The text has been modified to make such distinctions clearer.

(8) 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...

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[Reply] Tables 3-5 were moved to supplementary materials (as Supplementary Table 1-3). We need Figure 8 to elucidate the process between age determination and crustal uplifting rate constants including age-record of L. elliptica (Miura et al., 1998c).

(9) p. 4347: Were the samples heated up to 70 6C?

[Reply] We changed to "70°C".

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

[Reply] Suggested experiments can only be conducted as part of another study because of current sample limitation. We are primarily describing a marine-lacustrine transition event during the crustal uplifting.

(11) 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.

[Reply] We revised as "brackish/freshwater taxa", not estuarine. According to literatures, Hirano (1983) reported these taxa (Navicula arcuata, Navicula molesta, Stauroneis anceps, Diploneis subcincta, Navicula gregaria, Achnanthes hankiana, Amphora ovalis, Amphora oligotrophenta) from several lakes near Showa station, East Antarctica. Spaulding et al. (2008) also reported Amphora oligotrophenta.

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

[Reply] We deleted the paragraph.

(13) p.4354: the calculation of the uplift rate is straightforward but described in an

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overcomplicated way; there is no need to give the formula here. Also Fig. 8 can be deleted.

[Reply] We have to define uplifting rate k as the formula. We conclude that this description (p.4354) is simplified scheme as well as Figure 8. Otherwise, readers will confuse the process between age determination and crustal uplifting rate constants including age-record of L. elliptica (Miura et al., 1998).

(14) 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).

[Reply] We modified "The relative sea-level curve of the Vestfold Hills was estimated from marine lacustrine transition including Ace Lake and Anderson Lake (Roberts and McMinn, 1999; Zwartz et al., 1998). Subsequently, the 10 relative sea-level curve of the Larsemann Hills was also estimated (Verleyen et al., 2005). We introduced the literatures (Huybrechts, 2002; Ivins and James, 2005) instead of local comparison of other East Antarctic regions.

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

[Reply] We showed 2-sigma errors of the reservoir effect in Table 2.

(16) 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.

[Reply] We used 1300 years for the calibration of L. elliptica (shell portion) in accordance with the latest literature of Miura and co-workers studies (Miura et al. 2002), whereas the sediments cores was also after literatures based on living marine organism (e.g., Yoshida and Moriwaki, 1979, Hayashi and Yoshida, 1994: Ingolfsson, 1998).

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The information was inserted to Figure 9 caption.

Miura, H., Maemoku, H., Moriwaki, K. : Holocene raised beach stratigraphy and sea-level history at Kizahashi Beach, Skarvsnes, Lutzow-Holm Bay, Antarctica. Royal Society of New Zealand Bulletin, 35, 391-396, 2002.

(17) Table 2: heading of first column should be 'lake'

[Reply] We modified the first column.

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