

***Interactive comment on* “Links between Baltic Sea submarine terraces and groundwater sapping” by Martin Jakobsson et al.**

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General comments

This manuscript presents a study of seafloor morphological features in Stockholm Archipelago and Blekinge Archipelago in Sweden, and an archipelago area in the southern Finnish coast, focusing on submarine terraces that are interpreted to be formed by groundwater sapping. The study is based on high-resolution multibeam echosounder bathymetry, supplemented by seafloor photography and filming by ROVs, sedimentological and geophysical analysis of sediment cores, and C and O isotope analysis of a carbonate concretion found in one of the cores. The dataset is strong and has a good regional coverage, the quality of multibeam data is excellent, and

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the supporting information is adequate. Overall, the manuscript is well written and illustrated, and the topic is both relevant to the wider audience and suitable for Earth Surface Dynamics. However, parts of interpretation of results and Discussion need to be developed before the manuscript is acceptable for publication. My main concern is that while the authors convincingly explain groundwater sapping as the mechanism of the formation of submarine terraces, they do not discuss other mechanisms that can produce similar structures. The submarine terraces are observed in glacial varved sediments, where liquefaction of the coarser couplet layers is interpreted to have resulted in the detachment and sliding of sediment blocks, leaving behind the observed terrace structures. While groundwater flow can cause liquefaction of the coarser couplet layers, also seismic shaking and reduced hydrostatic pressure as a consequence of rapid water level fall can result in sediment liquefaction and produce similar terrace structures. Paleoseismic activity is well known in the area, and sliding, slumping and debris flow deposition of glacial varved sediments has been previously associated with seismically triggered liquefaction (e.g. Hutri & Kotilainen, 2007, *Marine Geology*; Virtasalo et al., 2007, *Sedimentology*). Also drops in water level are known to have taken place previously in the area (e.g. Sauramo 1923, *Studies of the Quaternary Varve Sediments of Southern Finland*, *Bull. Comm. Geol. Finl.* 60). The authors thus need to consider other potential mechanisms before they can associate the observed submarine terraces with groundwater discharge as strongly as it is currently written in the manuscript.

My other concern is that the submarine terraces are observed on seafloor areas with relatively low topographic gradients, whereas significant groundwater flow requires a strong hydraulic gradient. How can the required hydraulic gradient be developed in such flat areas and at some distance to the shoreline? Furthermore, in Fig. 5a and 5b, terraces are interpreted on the slopes and tops of local elevations. What would be the mechanism leading to a sufficiently strong groundwater discharge to these local elevations to produce the terrace structures?

The manuscript is lacking hard evidence of groundwater influence such as radon measurements that could potentially help strengthening the interpretation of the formation mechanism of submarine terraces. Isotope analysis of the carbonate concretion is a good attempt in this direction, but the results unfortunately are inconclusive. In case it is not possible to exclude the other possible mechanisms, the tone of the manuscript should be changed from “submarine terraces are produced by groundwater sapping” to “submarine terraces can be produced by several processes, but we interpret our observed structures to be produced by groundwater sapping”.

The manuscript also reports seeps from the seafloor, but their connection to terraces, groundwater and the topic of the manuscript in general is not clearly explained.

Specific comments

Page 2, line 25. Add “varved” between “glacial” and “clay”. It would help the reader in case the varved clay structure was shortly explained already in this paragraph.

Page 10, the first paragraph about glacial varved clays is very much dominated by Swedish publications. Please consider adding classical works by e.g. Sauramo in the discussion.

Page 10, line 31. There is no compelling evidence for brackish Yoldia Sea northward and eastward from the south-central Sweden (Schoning 2001, Boreas). Yoldia Sea is not necessarily relevant to the topic of the manuscript and it could be excluded from the discussion.

Page 11, line 4, “irrefutably”.

Page 11, lines 5-12. Perhaps the concretions formed already during glaciolacustrine or post-glacial lacustrine environments, and comparisons to present brackish-water Baltic Sea are not relevant?

Page 12, lines 1-5. How about the O isotope composition of the post-glacial lacustrine phase? Perhaps it was similar to large lakes in Sweden and Finland today?

That ends my referee comments.

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