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Interactive comment on “Landscape control of uranium and thorium in boreal streams – spatiotemporal variability and the role of wetlands” by F. Lidman et al.

Anonymous Referee #2

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The paper presents not novel but geochemically-valuable topic, concentrations and migration of uranium and thorium in wetlands. This topic is certainly interesting for geochemists but I am not sure about the pertinence of this work to Biogeosciences journal (unless there is a special issue). The amount of discussion of organic matter / biota and related mechanisms are minimal.

The motivation and objectives of this work are unclear. Is there any hypothesis to be tested? Without clearly presenting this issue, the paper may appear “of local interest only”. The novelty of main results and main conclusions are unclear. The amount of primary material presented in this work is insufficient. Some data useful for the

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discussion are in preparation; they should be certainly shown.

Introduction: The authors should formulate the objectives and scientific questions of this work. What is new in this paper compared to previous study of Anderson et al., 1995, 1998? Only the choice of the catchment? Then this study is of local interest only. Secondly, the authors achieve the same conclusion on the correlation between element concentration in soils and in streams as that of Astrom et al., 2009. The question of novelty and originality is, again, becomes important.

The last sentence of the Abstract is rather trivial unless the solid and dissolved fractions (bulk and filtered samples) are provided.

The sampling and analysis are insufficiently described and as such the quality of experimental approach can not be judged. Describe briefly the methods: sampling, filtration, storage, analysis. What are the uncertainties, detection limits? How good were the analyses of certified samples such as SLRS-4? If the samples were not filtered (what are the bulked volume weighted monthly samples?) then the pertinence of results to biogeochemistry becomes almost nil.

Careful measurements of discharge are certainly appreciated.

p. 2830, line 21: The reference to the Abstract is certainly useless here.

Discussion on p. 2832 on relative mobility of U and Th. Much lower mobility of Th compared to U is fairly well known for long time. One would not question, for example, lower mobility of Al compared to Mg, although their concentrations in rock-forming minerals may be similar. The export of U from different watersheds should be placed in the context of dominant lithological substrates (silicate rocks and their weathering products; carbonates. . .)

Section 3.4. Airborne gamma spectrometry. How well comparable these data with the bulk soil/rock analysis by wet chemical methods? At least some comparison should be given here.

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Section 3.6. Migration of Th and U in the wetlands. The limiting factor of element migration may be not the source but the carrier availability (Fe colloids, organic colloids, organo-ferric colloids). However there is almost no discussion on these issues, neither on U and Th speciation in streams and their main carriers (Fe, OC). The data on U and Th accumulation in the peat should be shown. Why do the authors fractionate the results? (Line 6, p. 2839 states that the data on Th and U are in preparation(!)) How these data are related to the present study? It is impossible to understand the mechanisms without seeing these data.

p. 2839, last line: Why the link between U concentration and alkalinity mentioned in this part of the text is not at all investigated? U-carbonate complexes may indeed compete with Fe-C colloids and this should be certainly addressed.

One of the main conclusions of this work is that the wetlands are sinks for uranium and thorium. However, this result is not new and the authors cited the relevant papers.

Figure 3 presents the most important part of collected data but it should be better visualized and probably re-drawn. First, a plot of concentration versus discharge for different streams is needed. Comparison of metal flux with DOC flux is certainly needed.

Figure 4. It remains unclear why the silt was excluded. Just for mathematical convenience? Please provide a sound geochemical explanation.

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