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Biogeosciences Discussions

Interactive comment on "Temporal and spatial trends for trace metals in streams and rivers across Sweden (1996–2009)" *by* B. J. Huser et al.

Anonymous Referee #2

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GENERAL COMMENTS:

Herein, Huser et al. present and examine a long-term data set (1996-2009) of trace metal measurements taken in streams and rivers throughout Sweden. The authors examine positive and negative trends in the concentration of several trace metals over time using standardized methods, and attempt to determine any dependence between trends in trace metals and potential in-stream drivers (TOC, Fe, sulphate, pH). The authors also contrast trends in trace metal concentrations between the northern and southern regions of Sweden.

The paper is very clear and well written. The authors did a good job of explaining how data were examined for outliers and data points/sites that heavily skewed results, and satisfactorily justified the removal of some data before examining trends.

The data set presented in this paper is very comprehensive. Data sets of this length and breadth (covering an entire country) are not often published in the aquatic sciences; this breadth of data is even more rare for trace metals. The data set is also interesting because, in contrast to most papers examining trace metal loading in streams and rivers, any sites directly influenced by point sources were excluded. This reduces the factors influencing trends in trace metal concentrations to deposition from diffuse sources. In light of the continuing strong decline in trace metal deposition throughout Sweden, this will be an interesting and important data set to maintain into the future, as it will undoubtedly shed light on the release of trace metal contaminants from soils over time. Also, there are obvious parallels between Sweden and many regions in the boreal forests of Russia and Canada.

I agree with anonymous referee #1 – it would be interesting to examine changes in biological processes resulting from climate change, and how these may potentially influence trace metal mobility/transport. It would also be interesting to examine the discharge and precipitation records for the streams and rivers that were sampled in this study to examine the potential effects of snowmelt and overland flow on trace metal transport. The seasonal Kendall test effectively removed any strong seasonal variations from the data sets, but this variation may shed some light on what is driving the dependence (or lack of dependence) of trace metal transport on drivers. Also, perhaps there would be some interesting dependencies between trace metals and drivers if one of the other classification categories given in Table 2 (e.g. land use) were used instead of, or in addition to, the ecotone boundary. The authors have indicated (in the final paragraph of the discussion) that they intend to examine the effects of additional variables, including climate, discharge and land use, on trends in trace metal concentrations in future papers. It may be even more effective to layer these classifications and consider them in combination (e.g. multiple linear regression analysis).

In light of the uniqueness of the data set presented, and the authors' intent to expand on the analyses presented within, this article is certainly worthy of publication. I have

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only two specific comments for the authors' consideration, and perhaps the second comment below is beyond the scope of possible revisions for this paper.

SPECIFIC COMMENTS: At the end of section 2.1 (general description), the authors state that Sweden was divided into "two regions based on the limes norrlandicus ecotone". The criteria for the limes norrlandicus ecotone boundary are not stated – this would be useful information as it would more clearly demonstrate the contrast between the two regions. A description of the difference between the two should definitely be included in the published version of this paper.

For the authors' consideration - it looks to me as though there is a similarity in the pattern of strong relationships (as determined by Kendall's tau coefficients) between in-stream trace metal concentrations and two of the drivers, Fe and TOC (Table 5). Could this be some form of co-variance? Possibly the TOC is binding both the Fe and the trace metal? Was this examined, or tested for in the lab? Perhaps this should be discussed briefly?

TECHNICAL CORRECTIONS: Page 805 lines 20 – 21: The sentence beginning "The number of less than values. . ." is unclear. Do you mean less than detection values? Page 814 line 19: Lettenmaier et al. Page 816 line 7: "affect in-stream processes and metal dynamics. . ." Page 816 line 8: "that can drive changes in in-stream chemistry. . ." Figure 3 spans 2 pages – the maps on the first page of the figure (for Ni, As, V, Pb, Cu, Zn) are small. Please enlarge the figure so that the symbols are more clearly visible. References were fine.

Interactive comment on Biogeosciences Discuss., 8, 801, 2011.