

Interactive comment on “Statistical distribution of series of 12 monthly concentration samples for environmental classification of rivers” by J. Eliasson and T. Thordarson

Anonymous Referee #3

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General comments

The paper is concerned with the important issue of water quality classification of rivers based on a limited amount of samples under economical constraints. To accomplish this task the authors investigate the distribution of monthly concentration values of different hydrochemical constituents of 14 rivers sampled over the period of one year. They compare it to the normal and lognormal distributions and propose a new cumulative distribution function (DoC) to allow water quality classification according to Icelandic regulations. While the paper tries to tackle this interesting question, it has in

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my opinion some severe limitations:

1. I disagree with the author's assumption that 12-14 samples of different chemical parameters measured over a period of only one year are representative and would allow a sufficient classification and choice of the correct statistical distribution. This is especially the case when analysing the long tails of the distribution function as discussed in this paper. For a sufficient analysis and choice of the statistical distribution a much larger sample of concentration variables needs to be taken into account.

2. A further limitation is that the author's statistical analysis is based on the assumption that the concentration variables are independent from each other and identically distributed. This is the fundamental precondition of the central limit theorem where the authors refer to in the interpretation of Figure 5. But when dealing with the different chemical constituents of this study (e.g. pH, E.coli, TOC, IC, T-N etc.) one would expect correlations of some parameters that would violate this assumption. For instance the authors state that there is no strong seasonal correlation, but they ignore that some constituents are certainly correlated to the discharge. Neglecting the discharge conditions in the statistical analysis is a major constraint that needs to be addressed in this study.

3. The structure and language of the paper make it difficult for the reader to follow the line of thought. The objective and reasoning throughout the paper appear to be vague. For instance it does not become entirely clear how the major objective, the stream classification according to water quality, can be improved by the applied statistical methods. The paper would benefit in my view from a more precise objective and procedure in combination with a clear structure and revised language.

Specific comments

1. The title of this paper may be misleading since 12 monthly concentration samples may imply 12 samples per month and not per year.

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2. P2562 L23 - P2564 L23: This column deals with the description of the study area and should be moved to section '2 Study area and sampling sites'. Moreover a short introduction about the state of the art of water quality classifications based on a limited amount of samples and relations to other studies would be beneficial.
3. P2566 Section 3.3: This section is named 'non-parametric approach' although it deals with a parametric approach.
4. P2564 section 2: This section deals with the description of the sampling sites and sampling conditions. However the section lacks information about the general sampling scheme and sampling conditions, e.g. base flow sampling?
5. P2565 L15: The authors claim that the constituents do not have a strong seasonal correlation. Based on which statistical test was this conclusion drawn? Please specify 'strong'.
6. P2566 L4 onwards: The authors state that statistical tests do not reject the normal distribution for most of the series, although systematic derivation can be observed. Please specify which statistical tests have been made (e.g. Ks?) and for which series the test was rejected.
7. P2572 L7 onwards: The authors argue that the pooling presumes that the constituents do belong to a common two parameter distribution, a presumption already in use in most environmental monitoring practice. As in this case, the authors frequently refer to the 'general practice' without supporting their argumentation with references.
8. The amount of references is quite limited and contains a lot of grey literature. Moreover two references of the annex are not mentioned in the document (ISO, State of New York Department of Health)
9. It is not entirely clear what the correlation coefficient in Table 1 stands for.

Technical corrections

1. The display and layout of the Figures could be improved. In almost all Figures the differences between the constituents are hard to differentiate, a description of axis is lacking in Figure 6 and Figure 7 and axis deviations are sometimes overlaid by the graph like in Figure 7.

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