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Interactive comment on "Quantifying uncertainty on sediment loads using bootstrap confidence intervals" *by* J. I. F. Slaets et al.

Anonymous Referee #2

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This paper is a very good contribution to the literature about load estimation and the uncertainty of load estimates. The consideration of the relative role of discharge uncertainty and concentration versus discharge uncertainty is a valuable contribution.

I do have concerns about the quality of the regression relationships that were used in the analysis. See my comments on figures 3 and 4. Also, they need to be clear about how they view year-to-year variability. Do they consider each year to be a separate population or are they each a different sample from the same population. Are estimates from the two years done separately? There are two schools of thought about how concentration prediction models should be built: using just data from the year of interest or using data from many years, with some consideration for the possibility that there may be a temporal trend in that relationship. They should be explicit about this issue.

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line 222-229. It is not clear why the base-flow samples should be considered to be independent. My own experience is that they are not (I am thinking here about the residuals from a concentration versus discharge relationship). They used a first order autoregressive process but it seems to me that the process may be more complex than that, with memory that lasts for many days duration.

line 302. It appears to me that the method of removing transformation bias is similar to the approach called the “smearing estimate” proposed by Duan. I realize that Duan’s smearing estimator is mentioned later in the paper (line 472) but I think it needs to be mentioned here as well. Furthermore, the question of whether the residuals are homoscedastic needs to be considered. If it is not, then this approach becomes problematic.

Figure 3. A sample size of 15 is very small for constructing a rating curve. It is disturbing that for the lowest two and highest three predicted values the residuals are all negative and fairly substantially so. My reaction to this plot is that there is some significant lack of fit to the proposed rating curve model. Even with this small number of observations, perhaps a higher order or non-linear model should have been considered.

Figure 4 is even more disturbing in terms of fit. The observations should be on the y-axis so that the residuals can be visualized as the vertical distance from the 1:1 line. For virtually every observed value greater than about 600 mg/L the residuals were all positive or only very slightly negative. Conversely, for the vast majority of the observed values below about 100 mg/L the residuals were almost all negative and in some cases the predictions were as much as 10 times greater than the observed. This is a very flawed model to be used as the basis for this experiment. A study of errors needs to start with a fitted model that does not have such a high degree of bias.

485-489. The issue is not whether concentrations or loads are log-normally distributed. The issue is the normality of the residuals from the fitted model. This is a common error

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in analysis of such data sets. The adequacy of the estimation method should be based on the distribution properties of the residuals.

500-518. These points about overly complex models are very good. This is an important concern and I'm glad the authors emphasize it here.

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