

We want to thank the anonymous reviewer #4 for the constructive review. The comments are a great help to improve the manuscript. Following you can find detailed responses (in red) to the comments (black):

The paper by Schindler Wildhaber et al. uses stable isotopes and other measures of organic components to determine sources of suspended sediment in a small watershed. The questions are of interest, the background laid out and the literature well represented and integrated. The use of the un-mixing model of Phillips and Gregg is not new for characterizing stream suspended sediment, but is a useful way of differentiating sources. The paper goes on to attempt to link the observed changes with the variations in flow regimes which is of interest hydrologically.

However, there are a number of problems with the use of statistics in this paper and while I think there may be many significant findings in the data set presented I think the rationale and justification for the use of many of the statistical tests, including the validation of the tests assumptions have to be cleared up.

In table 1 means and standard deviations are presented for a range of variables and the three sites. While a student t-test was indicated as the method of differentiating these samples there are more robust methods that could be used with this amount of data. The overlap of the standard deviations in many of these variables ($^{13}\text{C}_{\text{tot}}$ for example) between sites makes it appear that there are not significant differences. This table should also have some indication using superscripts or some notation method to show which sites are different and similar to each other for easy interpretation.

Author reply: Yes, you are totally right. We will use ANOVA tests to compare the three sites. We will indicate the differences and similarities in the table 1.

The use of regression analysis is also problematic. The assumptions of data distribution have not been addressed, or at least written about, and the assumption of independence among variables has been violated. While correlations of variables could suffice in some cases, again the assumptions of distribution would need to be addressed. For example on pg 464 line 6-9 the authors state they found a negative correlation between C/N ratios and ^{15}N of SS. First they are using regression but stating it is a correlation. Even so as these two variables are not independent of each other it violates the assumptions of regression.

Author reply: We changed the sentence on page 464, l.6-9 to: „Regression analysis indicate a significant negative correlation between mean C/Na and mean $\delta^{15}\text{N}$ of SS (Spearman rank correlation = -0.8). This inverse relationship can also be seen in Fig. 2c.”

Problems also exist with the multivariate statistics. Given these problems it is hard to determine what significant patterns exist. But the un-mixing model does not rely on the lack of pattern or the pattern between source materials but rather the composition of the end-members and their apparent separation.

Author reply: Yes, we agree. We will discuss this point in the discussion and conclusion. We will particularly refer to Phillips and Gregg (2001): Uncertainty in source portioning using stable isotopes.

I think this paper should focus on model more and include a better description of it. I am not clear as to why the mixing model excluded the autochthonous end-member (algae). The lack of a linear fit between C/N ratios with the two forms of sediment does not seem a suitable rationale. Why not run the test with the algal member in the data set to show it is excluded by being a very low contributor?

Author reply: We excluded the algae samples not only due to the small C/Na ratio but also due to the low $\delta^{13}\text{C}_{\text{org}}$ value between -41 and -31‰. Those small C/Na and the low $\delta^{13}\text{C}_{\text{org}}$ values of the algae samples compared to the SS samples indicate none or minimal autochthonous C_{org} contributions. You are right we could have run the tests with the algal member. But we decided not to do so because mixing analyses get more complex and are connected with a higher uncertainty with an increasing number of end members, especially if there is little isotopic differentiation among sources (Phillips and Gregg, 2003). $\delta^{13}\text{C}_{\text{tot}}$ and $\delta^{15}\text{N}$ values of algae are not significantly different from the other three/four sources. Consequently, results would be connected to a high uncertainty.

We will discuss this approach in the manuscript.

The sampling of suspended sediment using the Phillips traps is not necessarily representative of both organic and inorganic components of the suspended sediment. As the authors state that organic sediment is under-represented in the infiltrated sediment due to its low specific gravity, this would also affect its capture by the SS trap which has a small aperture and is usually mounted above the bed (the paper should state the height above the bed that they positioned). For a review of some problems with the Phillips traps see (MacDonald DM, Lamoureux SF, Warburton J. 2010. Assessment of a time-integrate fluvial suspended sediment sampler in a high arctic setting. *Geografiska Annaler Series A-Physical Geography* 92A : 225-235. DOI: 10.1111/j.1468-0459.2010.00391.x.). Did the authors have any SS taken from filtered water to compare to the traps for OM percentage as this would allow some estimation of the omission by the traps?

Author reply: The SS traps were mounted just above the river bed. The aperture height was therefore at about 60 – 70 mm above the river bed. We will add this information to the manuscript.

We did not measure organic matter in the water samples. You are right, smallest particles of the SS do get underestimated to some extent with the SS samplers (Phillips et al. 2000). But Phillips et al. (2000) also showed a close correspondence between weighted mean d_{50} of sediment collected by the SS sampler and of point samples. They concluded that SS samplers collect statistically representative samples. They proposed an increase of the ratio between the diameter of the inlet tube and that of the main cylinder to enhance the trapping efficiency of smallest particles. McDonald et al. (2010) increased this ratio somewhat, but they shortened the sampler from 1000 mm to 228 mm. Consequently, there is less time for particle to settle while flowing through the sample. This has probably decreased the efficiency in capturing smallest suspended sediment particles. Due to this modification, we do not believe that the results of McDonald et al. are representative for our study. Nevertheless, we will discuss the potential bias on capturing smallest particles and organic matter in the SS samples in the manuscript.

The paper suffers somewhat from trying to address a large range of variables in several different ways rather than focusing on one approach. For example the details regarding the water quality evaluation of the river and piezo samples is an aside from the main aims of the paper. Interesting but not all that relevant in this context.

Author reply: The main objective of the manuscript is the organic matter dynamics during the brown trout spawning season and the tracing of the sources of suspended sediment. We believe that water quality of the river and in the piezometers is within the aim of the manuscript. The water samples of the piezometers are necessary as they represent the water quality at the eggs. We will discuss this in more detail in the introduction and in the conclusion.

If the statistics were properly used and described the paper and the model presented in more detail this paper could be strengthened.

Author reply: Yes, we will work on the statistic (detailed answers see remarks above)