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Interactive comment on “Organic matter dynamics and stable isotopes for tracing sources of suspended sediment” by Y. Schindler Wildhaber et al.

Anonymous Referee #4

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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 assump-

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tions 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 (13C_{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. 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 15N 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. 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. 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? 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

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

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