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Interactive comment on “Sediment source attribution from multiple land use systems with CSIA” *by* C. Alewell et al.

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Reviewer 1 mainly addresses three points: 1) neglect of literature, 2) unnecessary reanalysis of samples of Schindler et al., 2012 and rationale of the introduction in general, and 3) use of terminology. We would like to address these three main points here while we will answer to the more specific comments in the revision of the manuscript. Regarding the first point of critique, reviewer 1 feels we neglected relevant literature. Thanks for the effort to provide us with a list of studies on CSIA on soils, organic matter and sediments. We carefully evaluated all suggested studies but we think many are actually not relevant for this manuscript or redundant to the studies we already quoted. Our aim here is not to write a general review on the use of CSIA in soil science or on the use of CSIA on organic matter transport from terrestrial sources to marine sediments. Our focus was on studies that used sediment source fingerprinting with CSIA in fluvial systems. Glaser et al., 2005, Jandl et al., 2000, Madan et al., 2002, Naafs et al., 2004a, Naafs et al., 2004b, Nierop et al., 2001 and 2005 are studies on CSIA generally in soil science or on CSIA of SOM and do not contribute to the topic of our manuscript. Colombo et al. 1997, Jeng and Huh, 2004; Sanchez-Garcia et al., 2008 investigate terrestrial versus petrogenic and/or aquatic origin of organic matter in marine sediments. Colombo et al., 1997 discusses possible degradation of FAs, but we evaluate the isotopic signature of FAs and not their content. As such, the question is whether or not this signature is altered if there is degradation and not if we have a change in the absolute content. The latter is addressed by some of the other studies the reviewer mentioned, generally supporting the idea that there is no fractionation of compound-specific stable isotope (CSSI) of FAs during degradation and/or that FAs are remarkable stable (Drenzek et al., 2007; Marseille et al., 1999; Wiesenberg et al. 2004). While Drenzek et al., 2007 and Marseille et al., 1999 find FAs to be more stable than alkanes, Wiesenberg et al. 2004 finds alkanes to have longer turnover times than FAs. There is more literature to support our approach of using long chain FAs as tracers of terrestrial sediments and some of them point out that the short chain FAs are of bacterial and/or aquatic origin (Eglinton and Eglinton, 2008; Ficken et al., 2000, Huang et al., 1996; Lichtfouse et al., 2004, van Dongen et al. 2000). Of course we could add some of these to the already quoted studies in our manuscript. The same applies to studies investigating the change from C3 to C4 plants: Ficken et al., 2002; Quenea, 2006.

We are sorry that referee 1 thinks our introduction is very confuse. We will certainly give it a close look and consider the specific comments below during the revision. This overall confusion seems to emerge partly from a misunderstanding regarding the aims of this study, as reviewer 1 thinks it is not useful to analyse CSSI if we already answered the questions regarding the sediments sources in this specific catchment “Enziwigger” with the study of Schindler et al., 2012. Schindler et al. (2012) tracked sediments with bulk analysis of δ13C which was only possible because nearly all forest cover in this catchment is stocking on calcareous bedrock while all arable land and grasslands are on siliceous material. This is a very rare situation and as soon as we will move on to another catchment this approach will not be applicable any more. As such we used the setting to test whether or not the CSSIs of fatty acids might be suitable tracers which are also suitable for catchments with no change in geology between the different land cover/land uses. In contrast to the reviewer we don't consider the analysis of Schindler et al. (2012) and our analysis redundant since the results found by Schindler et al. (2012) can be used to verify the suitability of CSSIs of fatty acids as sediment fingerprints in C3 plant dominated catchments. When revising the manuscript we will try to make that more clear. In addition we tested if we would find a difference in the CSSI signatures of FAs from grassland and arable soils which of course is not possible with isotope bulk analysis.

We are criticized to mix up the terms CSSI and CSIA. For example, the reviewer thinks we misused the terms in L50-56: "A new technique, using the compound-specific stable isotope (CSSI) signatures of inherent soil organic biomarkers, can discriminate and apportion the source soil contribution from different land-uses in order to reinforce the effectiveness of soil conservation measures (Gibbs, 2008; Blake et al., 2012; Guzman et al., 2013; Hancock and Revill, 2013; Ponton et al., 53 2014). The compound-specific stable isotope analysis (CSIA) measures the δ13C or δ2H isotope signature of specific organic compounds associated with the organic matter bound to the soil/sediment." We cannot see any fault in the use of the terms here. The first sentence addresses the isotope signatures, the second the technique. Maybe the reviewer 1 is "overcritical" here. We admit we accidentally added a stable when introducing the technique. As mentioned by reviewer 2 the more logical abbreviation would then be CSSIA, but since previous studies introduced the term CSIA and it is a commonly used term by now, we would rather stick with this abbreviation.

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