

Interactive comment on “Spatial gradients in soil-carbon character of a coastal forested floodplain are associated with abiotic features, but not microbial communities” by Aditi Sengupta et al.

Anonymous Referee #1

Received and published: 20 June 2019

This paper attempts to identify associations between soil carbon chemistry (molecular composition of SOC fractions revealed by FT-ICR MS analysis) and microbial communities (analyzed by 16S rRNA) at the coastal terrestrial-aquatic interfaces (TAIs) influenced by salinity gradients along a small first order stream in the Washington Coast. These two high-resolution techniques generate tons of information on organic matter chemistry and microbial community composition, which allows detailed examination of their linkages. The introduction part nicely lays out the rationale and hypothesis of this study and the paper is overall well written. However, there are a few issues that need

C1

to be addressed.

First of all, the extracted fractions and analyzed molecules are only a small part of the SOC, which may (very likely) not reflect the overall chemistry of total soil organic matter. In this regard, the title and related descriptions should be clarified—it is “chemical characteristics of soil carbon fractions” instead of “soil-carbon character”. It should also be mentioned in the Methods how much SOC was extracted by the employed method. Given the lability of WSOC, it is hence more likely to be influenced by microbial decomposition compared to bulk SOC, but it is also strongly influenced by direct inputs of low-molecular compounds from root exudates, etc.—this brings my second point.

Despite the nicely formulated hypotheses for this paper, the authors seem to largely ignore (or underestimate) the influence of input processes on the molecular composition of extractable OC. Water- and solvent-extractable OC may derive from direct plant and algal inputs other than depolymerization of soil macromolecules by microbial-mediated enzyme attack. How would root exudates contribute to the thermodynamically less favorable C, for instance? Do you have an estimate of NPP (hence soil inputs) along the study gradient? The observed changes in C chemistry may well be a combined result of decomposition and input processes. Similarly, how would photo-oxidation affect the signal?

Regarding the analysis and interpretation of the FT-ICR MS data, I am not convinced that the number of common/unique formulas is the best parameter to describe changes in OC chemistry. The relative abundance of these formulas should be considered. How representative are the unique formulas in the overall abundance of total MS peaks, for instance? How does the relative abundance of common formulas change with salinity gradient? Hemingway et al. 2017 GCA give a good example for such kind of analysis.

Specific comments: Line 219: Why these two depths? Line 395: Relationship with what?