

## ***Interactive comment on “Wet-dry cycles impact DOM retention in subsurface soils” by Yaniv Olshansky et al.***

**Anonymous Referee #2**

Received and published: 30 September 2017

**General comments** The paper is well written and structured and provides new insights in the retention of dissolved organic matter in soil under varying moisture conditions, which is highly interesting especially for the scientific community dedicated to elucidate organic carbon dynamics along the soil profile. The chosen methodological tool set is innovative and suitable to reach the proposed research aims. I have some comments that should be addressed before the paper is potentially acceptable for publication.

**Specific comments** The entire paper needs to be checked for a proper introduction of the used abbreviation and their constant usage throughout the paper. Lines 15-19: split into two sentences Line 24: "considered" not "consider" Line 27: introduce the abbreviation for carbon and then use this abbreviation constantly throughout the paper Lines 68-71: give some explanations why do you expect an increase in surface complexation

C1

of carboxyl groups with metal (oxy)hydroxide surfaces or hydroxylated edge surfaces of aluminosilicate clays and promoted associations of hydrophobic fractions with pre-adsorbed and desiccated DOM components especially in the light of the relatively high pH value of the JRB soil and the statement in lines 348-349 that it is important to note that the studied soils are predominantly composed of silicate and aluminosilicate minerals and are relatively depleted in crystalline and short range order metal oxides Lines 86-89: explain the sampling design in more detail, at the moment the reader is not able to understand if true field replicates, pseudo field replicates or just laboratory replicates of one composite sample were analysed, this is important in relation to the statistical tests that were performed, which require at least pseudo field replicates from, for example, three different soil pits per site, also I did not find in the text any information about the number of replicates Lines 109-111: the 28 ml that have been filtered, is this the decanted aliquot or did this volume was removed by pipetting? please explain in more detail Lines 133-134: why did you use different preparation techniques for the two soils, and how this affects the comparability of the spectra? Line 250: for better understanding, please explain "relative", did you compute ratios or normalized the spectra to the o-alkyl peak? Lines 303-305: this statement has to be backed up with references and including a discussion of information from the literature because in this study no direct measurements of microbial parameter were carried out Lines 310-311: any recommendation how to address this? how should an experiment be designed to get further information here 354-356: this criticism is stressed specifically in the Abstract and in the Conclusions, if the authors wish to make a point here, they should discuss this in more detail including some recommendation for future experimental work

---

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-262, 2017.

C2