

Interactive comment on “Linking dissolved organic matter composition to metal bioavailability in agricultural soils: effect of anionic surfactants” by M. C. Hernandez-Soriano and J. C. Jimenez-Lopez

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Firstly, the authors would like to sincerely appreciate the time and effort of the anonymous referee. Responses to the comments and questions are provided below.

This paper presents a controlled experiment designed to assess the effects of two commercially available surfactants added to three different soils under two different moisture regimes. Fundamentally, the experimental design is flawed in that there are not enough replicates ($N=2$) of each treatment \times soil \times moisture combination. The

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results have therefore been over-analysed and it is not surprising that few statistically significant effects were detected either between treatments or between moisture regimes. The conclusions are therefore not sustainable. Response: The authors consider that it is arguable whether increasing the numbers of analytical replicates would strength the validity of the statistical analysis. Indeed, and according to well established methodology of experimental research theory and concepts (for instance: <http://documents.software.dell.com/Statistics/Textbook/Experimental-Design>), exploring the experimental domain defined by the relevant variables should include enough combinations of the variables studied to avoid confounding effects. Thus, the combinations treatment x soil x moisture used in the study are sufficient to discriminate the effect of the 3 variables. Secondly, the analytical methods used in this study are reproducible and robust and increasing the number of replicates to examine the validity of the methods was not relevant to the study.

There is reference to bioavailability in the aims and results, but as far as I can tell no assays for bioavailability were performed and the term is never really defined in the context of this work. Response: The authors clearly state as second objective (Introduction section): examine the chemical speciation of metals as predictor for metal bioavailability. It was not an objective of the study to examine bioavailability but chemical speciation, which has been thoroughly discussed and accepted in the literature as potential predictor of bioavailability (Chapman et al. 2012, Nolan et al., 2009 cited in this work and references therein). The authors considered that the term bioavailability is well established and that references provided are sufficient for the reader to explore the term. Nevertheless, the authors would be glad to include a suitable definition of the concept in the introduction.

There is some value in the analyses conducted using WHAM VI and fluorescence. However the various ways that solutions have been characterized (UV-VIS absorbance, fluorescence, speciation using WHAM VI) have received rather superficial attention to detail. Response: The authors will be glad to extend the discussion on the results

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yielded from the mentioned analysis as requested. We find the suggestion rather un-specific, but we are most willing to further interpret those results in alignment with the main objectives of the work.

For example, WHAM requires many inputs and there is no information on how the model was parameterized. Response: The authors have provided references that extensively describe the WHAM input approach used in this study (e.g. in section 2.5 Speciation and statistical analysis: ‘General specifications for data input are described elsewhere, (Van Laer et al., 2006)’). The authors would be glad to describe any specific approach that might be considered relevant to be included in the current manuscript. However, we would rather avoid repetition of information provided in previous work.

Considering the flaws in experimental design I think these results would be better placed as a much more focussed technical note in a discipline-specific journal. Response: Following previous comments, it is debatable that the experimental design is flawed: the statistical strength is derived from the different combinations of variables explored, which is more informative than increasing the number of analytical replicates. We consider that the work presented is well within the scope of this journal and we would be glad to extend discussion of the data presented if considered relevant. However, it is rather unclear from the referee comments which aspects should be specifically further discussed.

There are shortfalls in the way that the literature has been reviewed particularly with respect to what is known about metal-organic matter speciation and bioavailability. Response: The authors have extensively explored the literature and provided representative, up to date references to the best of their knowledge. If specific suggestions are provided we would be glad to include/discuss further references.

Data in tables is rather confusingly presented. It would be much better to have separate tables for soil characterisation and experimental results (solution characterisation). Response: The authors find this comment certainly useful. We will be glad to reorganize

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the data according to the suggestion provided.

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