



EGUsphere, referee comment RC3  
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## **Comment on egusphere-2022-469**

Anonymous Referee #3

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Referee comment on "Wetting and drying cycles, organic amendments, and gypsum play a key role in structure formation and stability of sodic Vertisols" by Sara Niaz et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-469-RC3>, 2022

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Review for article "wetting and drying cycles, organic matter and gypsum play a key role in structure formation and stability of sodic Vertisols", by Niaz et al.

Dear Authors,

first of all, I think you did a nice job. The article topic fits with Soil aim and scope, and furthering the understanding of aggregate formations is paramount if we want to protect soil health and increase soil organic carbon stock. The manuscript is written in good English and it is well structured.

I have some concerns, however, about the way the study is presented: the study tries to link physical structure of the soil (aggregates) with many different factors (chemical and biological) all together. This is not wrong in itself: the soil is a complex system, and complex system cannot be fully comprehended using reductionism. However, when studying the correlations between macro-variables of complex systems, it is important to understand that we are only finding correlations and not causal links. As such, I think that re-shaping the manuscript focusing on the correlations analysed (aggregate size - WD cycle; aggregated size - amendment; Aggregates stability indices compared with respect to WD; WD - respiration) and the discussion should focus more on putting the correlations found "in context" with respect to other vertisols in real conditions.

What I suggest the most is to be laser focused in the manuscript: write down clearly all your hypotheses in the "objective" paragraph of the Introduction (at the moment there are only WD-respiration; WD - aggregate stability; number of WD cycles to improve aggregate stability). Then get back in the introduction and help us understand exactly the relationship between soil respiration and soil aggregate formation, size and stability as found in literature (which is something it is missing right now). By the way, I really felt that the manuscript was missing the link between respiration and aggregates formation/stability.

In the Materials and Methods please be a bit more rigorous: define clearly what you mean by "dispersive" (line 114); explain why the two soils, albeit very similar, have different physical behaviour (line 113); clearly define the terms in equation 1 in the text (at the moment, they are a bit confusing); clearly write down the meaning and value of 'n' for equation 2 (number of fractions = 4, but it should be written down).

Please add error bars in the figures, or at least put some error indication in supplementary materials.

The discussion should be more focused on what we see in the results; at the moment

there is much speculation about possible causes - which is ok, but should not be the whole discussion. First, link the results, focus our interest on the relevant data, explain what is in the figures further, discuss limitations. Then you can go with speculation. By the way, I did not understand section 4.1: the mechanisms of microbial action are not explained in detail and are not related to existing studies, and I think the discussion says the opposite of what we see in the data! E.g. line 394 "macroaggregates break more easily during WD cycles compared to microagregates" - figure 1 shows a significant increase in macroaggregates for all treatments, all soils, between WD1 and WD4 (or am I wrong?). line 402 "the proportion of small macroaggregates did not increase after the second WD cycle" - again, this is the opposite of what I see in figure 1. The problem is that these sentences are the core of section 4.1! Please help me understand.

Conclusion should clearly answer the scientific questions posed in the Introduction: if you have 3 objective, give 3 clear conclusions.

Finally, and very importantly, the literature cited is "old": the most recent article is from 2018, and the number of post-2015 articles is only 3. This is a major issue, since our conception of soil mechanisms has definitely changes as marked by Lehman 2015 "the contentious nature of soil organic matter" - and organic matter is related with microbial activity and aggregates formation and stabilization. I also suggest the reading of articles that analyse soil microbial respiration with WD cycles (Brangari 2020-2021, the latest works by Lindsay Todman, etc...). Finally, if you base some of your discussion on unpublished data, please put some of them in the supplementary materials, at least as "summary data", or, even better, include them in the article, if possible.

Small corrections:

line 41 "wind, condensation, (Utomo and Dexter 1982) and evaporation"

line 49 "indirect effects on plants ecology and soil microbial..."

line 55 "soil microbial activity, since the latter influences..."

line 82 "organic matter affects aggregate stability"

line 245 "significant change in"

line 255 "after each WD cycle."

Figure 2: insert legend