Referee #1

We thank the referee for his supportive review and for the useful comments. Below are our answers, including proposed action, *following each of the comments.*

1. *Line 18, abstract: Please add the length of the columns in the column description (e.g. after six long column…)*

   Column length was 2 m, as described in the text. This information was added to the abstract (P.1. line 18).

2. *Line 24: "doubled the infiltration time" might be misleading as wording. Suggest changing to "double the time during which infiltration was possible"*

   Accepted and was rephrased in the revised manuscript (P.1, line 25).

3. *Line 27: What is SLPM?*

   SLPM stands for *standard liter per minute* (i.e., at given temperature and pressure). We changed the wording and now use l/min instead and clarify this in the revised manuscript (P. 1, line 28 and throughout the manuscript).

4. *Lines 59-62: Could you please state threshold values or value ranges for oxygen for some of the processes listed here? What do you consider aerobic conditions? Under what oxygen conditions does the ammonification rate decrease substantially? Is there a threshold that can be defined as clear tipping point? Does nitrification assume the same oxygen conditions (same number ranges)?*

   This is an interesting and important question. While in this study we did not use precise threshold values, the literature contains several different definitions (e.g., Sohasalam et al., 2008 who used the ranges of above 300 mV, 100-300 mV, and below 100 mV, respectively). Other classifications (e.g., Zhang and Furman, 2021 who followed several other definitions) classified Redox differently (oxidizing, weakly reducing, moderately reducing, and strongly reducing conditions (>400, 200 to 400, -100 to 200 and <-100 mV, respectively). The following text was added to the revised manuscript (P. 3, line 69-70). *"[Note, we follow here the definitions of Sohasalam et al., 2008 who used the ranges of above 300 mV, 100-300 mV, and below 100 mV, respectively]*."

5. *Line 72: What is the mechanisms behind the higher removal of CECs during longer drying periods? Is it just the aerobic conditions?*

   We are not sure if the reviewer refers here to CECs or to removal of major carbon and nitrogen compounds. The specific mechanism of CEC degradation was not part of this research but is discussed in detail starting at P.17, line 506. The literature suggests (as discussed in the later parts of the results and discussion section) that in some cases it is related to adsorption and in some to oxidation. Our results support the oxidation approach. However, it would be premature to suggest so with such a minimal database.

6. *Line 82: The column description is a bit confusing. Was the column only 200 cm long with the top 30 and the bottom 30 cm kept empty to provide room for flooding and drainage. That would suggest only 140 cm were effectively filled with soil? Also what are the 20x10x10 modules? How were they connected? How did you pack the column and avoided capillary boundaries between the layers/modules?*

   Thanks for highlighting the lack of clarity. The column itself is 200 cm long, of which 160 cm was filled with soil. The upper 30 cm was operational, and the lower 10 cm was used for drainage. This was clarified in the revised manuscript (P.4, lines 92-93).

   The column is modular with perfect sealing between modules (both rubber ring, shape fit, and overall pressing of the column with external long screws from top to bottom). This information was also added at P.4 lines 94-95.

7. *Line 108: By "surface pressure head" you mean the hydrostatic pressure of the ponded water?*

   Exactly. This is now clarified in the revised manuscript (P.5, line 114).

8. *Line 158: SLPM still not defined …but finally given in Table caption for Table 1.*

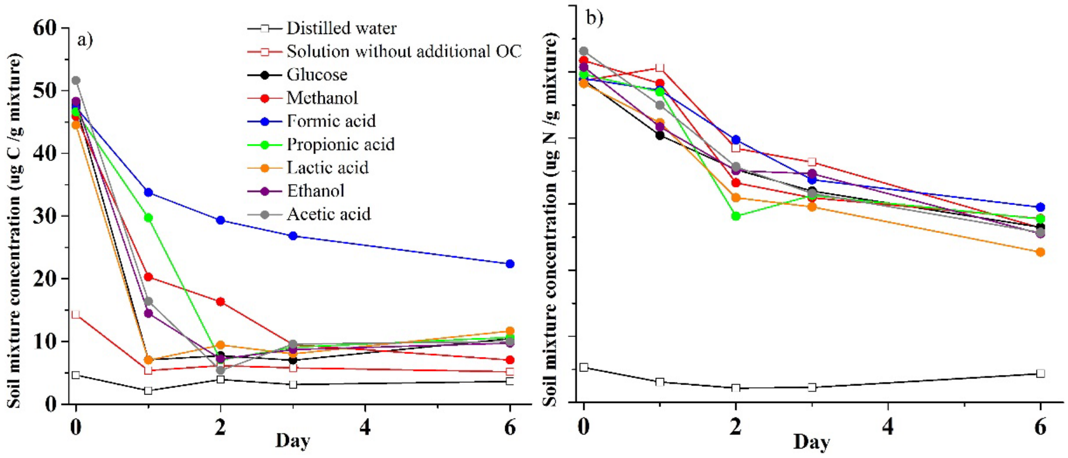
   See answer to comment #3. We have changed to l/min throughout the manuscript.

9. *Line 196: Why calculate a mean ORP for the profile if there is clearly a gradient with depth?*

   Mean ORP is calculated only for the purpose of the discussion regarding overall system performance. In other places we use specific ORP values. This was explained in the revised manuscript: "*To clarify, mean ORP is calculated only for the purpose of discussion of overall system functionality*" (P.7, lines 189-190).

10. *Line 199: Why did you choose to use glucose, which is a very digestible form of carbon? What forms of carbon are typically found in the wastewater?*

    The reviewer is touching here one of the greatest technical challenges of this research. First, wastewater contains a cocktail of various forms of carbon molecules, as noted in the manuscript with reference for a specific relevant source. We have attempted first to use several "well-accepted wastewater recipes" that contain other forms of carbon (see below, experiment conducted by Dr. Zengyu Zhang, in our lab), and found that they, in many cases, degrade too fast (even before reaching the soil). These simple experiments show that glucose is reasonable choice in terms of degradation time. We agree that this is not ideal, but as can be seen in the figure, reasonable. To clarify that point, we have added (P.7, lines 194-195) the following text: "*Although not ideal, the use of glucose is common in wastewater degradation studies (e.g., Liu & Logan, 2004; Liao et al., 2001)*". Our plan for the coming year is to work with treated wastewater from a treatment plan at larger setup. This is impossible in laboratory experiments for various practical and technical reasons.



11. *Line 283: The fast developing clogging effect is really striking! Have you thought about adding soil microbial analysis to investigate whether the abundance of bacteria is increasing supporting the idea of biofilm formation?*
    Thanks for the suggestion. During the experiments described in this manuscript it was not possible to take soil samples. Nevertheless, future experiments will include vast microbiology analyses and quantification, including the correlation between the development of biofilm and clogging.

12. *Line 385: These results suggest that the air perhaps should be injected even deeper (>1m) in the profile? Air buoyancy will ensure that air rises to the top of the profile but perhaps the deeper injection can address some of the low oxygen concentrations in the deeper profile.*
    This is true, but not 100% precise. Deeper injection also means that the air pressure will have to be higher than the hydrostatic pressure at that depth, which may require techno-economic optimization. Moreover, practically, inserting air tubes in depth of more than 1 m will be expensive and more complicated technically (i.e., different types of machinery). At the same time, greater depths probably mean larger space between air-injection tubes, which may lower the costs. These points will be examined in our future field-scale research.

13. *Line 465: It seems maintaining aerobic conditions in the upper profile provides suitable conditions for mineralization and nitrification and having low oxygen and reduced conditions in the deeper profile creates the right environment for denitrification. Often natural soils*

show a huge decrease in microbial abundance from the top 10 cm of the soil profile to 1 m. What is the microbial abundance on denitrifiers in the deeper soil profile that can actually reduce large amounts of nitrate?

Thanks for this interesting question. This specific question was not tested in our experiments, and therefore we cannot answer directly. As mentioned in our answer to comment 11, we do intend to test in detail the microbiological aspects in a series of field experiments that we hope to operate in the coming year.

Sohsalam, P., & Sirianuntapiboon, S. (2008). Feasibility of using constructed wetland treatment for molasses wastewater treatment. Bioresource Technology, 99(13), 5610-5616.

Zhang, Z., & Furman, A. (2021). Redox dynamics at a dynamic capillary fringe for nitrogen cycling in a sandy column. *Journal of Hydrology*, *603*, 126899.

Liao, B. Q., Allen, D. G., Droppo, I. G., Leppard, G. G., & Liss, S. N. (2001). Surface properties of sludge and their role in bioflocculation and settleability. Water research, 35(2), 339-350.

Liu, H., & Logan, B. E. (2004). Electricity generation using an air-cathode single chamber microbial fuel cell in the presence and absence of a proton exchange membrane. Environmental science & technology, 38(14), 4040-4046.