

## ***Interactive comment on “On the role of operational dynamics in biogeochemical efficiency of a soil aquifer treatment system” by Shany Ben Moshe et al.***

**Anonymous Referee #2**

Received and published: 10 October 2019

### 1) General comments

The study carried out by the authors is of deep interest in the context of water scarcity and re-use in arid and semi-arid regions of the globe. The increased use of soil aquifer treatment (SAT) definitely urges soil and water scientists to acquire a better understanding of such complex systems. The content of the presented manuscript is therefore worthy of publication in a journal such as HESS. Studying SAT is not trivial as it deals both with soil hydrology and the science of water treatment. The experts in the aforementioned fields will however have an hard time reading this manuscript. As an hydrologist, one will find himself frustated because of a poor description of the flow

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conditions in this simple 1D-column. The basic notions of soil hydrodynamics are overlooked. Experimental variables such as hydraulic loading rate and saturated hydraulic conductivity of the soil are not mentionned which makes any comparisons with other studies complicated and makes it hard for the reader to understand initial and boundary conditions. In addition, the use of vague terms and notions such as flow rates, timing water content (WC) peaks or time to replenish oxygen concentration (instead of expressing mean water velocity or reoxygenation rate) is not acceptable. As an expert in water treatment technologies, one will find himself exasperated by the absence of a proper description of the biogeochemical parameters (e.g. characteristics of the wastewater such as chemical and biochemical oxygen demand, total suspended solids per liter of water, number of colony forming units per liter of water...) and by the improper use of units (see specific comments section). Such information should be mentionned and properly summarised in the main body of the article (not in the supplementary material) considering that they are the most important experimental variables affecting the results. The experimental design of this study is quite impressive and definitely attracted my attention. However, it is disappointing that the take-home message of the study is quite trivial (i.e. longer drying periods allow for higher ORP values but mean less volume of water infiltrated per unit of time). The other conclusions are somehow weak and not put in a straightforward manner. In addition, the train of thoughts of the authors is most of the time unstructured which makes this manuscript hard to read. The efforts made to carry out this study definitely should result in a greater contribution to the topic of management and operation of SAT. Considering the above and the comments listed below in the appropriate sections, I recommend major revisions of this manuscript.

### 2) Specific comments

1. (line 95) - What is the link between choosing glucose as the main source of carbon and the fact that enables the study of the behaviour of the system in field SAT ? Why is it not tradional ? Information is missing or this sentence should be restructured.

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1. (line 102) - What was the frequency of data acquisition by the sensors ? As a subsequent question, was there any data manipulation/processing (e.g. outlier removal, filtering and/or curve smoothing techniques) of the time series presented in the paper ? If yes, they should be described or at least mentioned. I am really impressed by the quality of the data. At first glance, the time series looked like modelling results to me.

3. (line 115 to 119) - The authors mention the presence of pressure head sensors and soil solution sampling devices. Yet, no data regarding those sensors are shown. Why ? If the authors do not intend to show results, there is no need to mention their presence in my opinion unless it impacted the obtained results (e.g. disturbance of the flow regime at specific location, air intrusion,...).

4. (table 2) - Many space wasted and not many information contained in this table. If a proper (and scaled) schematic of the column was presented in figure 1, this table could be discarded.

4. (line 126) - Comments valid for the whole "Results and discussion part". Since ORP values and oxygen transfer are investigated, it would make sense in my opinion to express WC in terms of relative saturation of water (WC divided by WC at saturation). By doing that, the reader can directly have an idea of which fraction of the pore space is either air-filled or water-filled. Same can be said regarding oxygen concentration which could be expressed as DO/DO<sub>sat</sub> if the temperature is known at any time of measurement.

6. (line 203) - The following holds true for the entire manuscript. The authors should pay extra attention to the use of units, specifically the ones for nitrogen species. What is expressed here ? milligrams of ammonium per liter of water OR milligrams of nitrogen in the form of ammonium per liter of water ? I suspect the latter but this should be clearly stated (especially in figure 4 where having a common y-axis for all subfigures is simply wrong!). If it is the latter, the notation should be NH<sub>4</sub>-N (mgN/l) for ammonium

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and NO<sub>3</sub>-N (mgN/l) for nitrate.

1. (line 220) (3.1 Comparison with field observations). The Israeli SHAFDAN SAT site is very poorly (if at all) described in the method section which makes comparisons difficult to interpret. Where is it exactly ? What is the mean annual temperature there ? Under which conditions is it operated ? How is it comparable to the lab experiment conducted in Saxony ? If the point is to make a reliable comparison between the lab and field experiments, extra information should be added and this should be stated clearly as one of the purpose of the study in the introduction part.

3) Technical comments

see in text of the attached pdf file.

Please also note the supplement to this comment:

<https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-371/hess-2019-371-RC2-supplement.pdf>

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2019-371>, 2019.

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