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Hydrology and Earth System Sciences
Discussions

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

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We would like to thank Anonymous Referee 2 for his/her constructive comments. Most of the suggestions and comments were accepted and implemented in the revised version of the paper, as we report in the following point–by–point reply.

However, before we start we would like to put this research in the right perspective, from our point of view. SAT research combines earth sciences (hydrology, soil physics) with biochemical processes associated with wastewater treatment (i.e. processes like nitrification, de-nitrification, mineralization, etc).

The terminology used in each of the disciplines may sound lacking to people from the other. Our perspective is closer to earth/geo sciences, looking at SAT processes without comparison to classic wastewater treatment, rather as processes that may be controlled and manipulated by the system's operational dynamics. We believe that some of the comments provided by the reviewer are due to this difference in perspective. But perhaps more importantly, our perspective in this study is to test the ability to conceptually change (and improve) SAT operation. While we do qualitatively compare our results to the SHAFDAN facility in Israel, the specific details of the site are less important than the concept that SAT sites (both in field and laboratory scale) should not be seen as a passive component of the wastewater treatment process but as a 'pseudo reactor' that may (and should) be controlled by hydraulic operation manipulations.

General comments (GC)

GC 1 - *... The basic notions of soil hydrodynamics are overlooked. Experimental variables such as hydraulic loading rate and saturated hydraulic conductivity of the soil are not mentioned which makes any comparisons with other studies complicated and makes it hard for the reader to understand initial and boundary conditions*
Autors' response: We fully agree - hydraulics were so trivial (to us) that we forgot to include it. Average flux (that is of the same order of hydraulic conductivity in our gravity driven system) is now included in the 'Materials and Methods' section.

GC 2 - *...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.
Autors' response: We agree in part with this comment. Where possible, terms were clarified. However, we do not see some of the terms suggested by the reviewer, adopted from the classic environmental engineering terminology, as being proper to SAT. Therefore we choose to keep some terms and avoid using terms that may be misleading (such as 'reoxygenation rate'), as we later elaborate in our response to the Technical comments (specifically -the technical comment referring to line 177).

GC 3 - *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 mentioned and properly summarised in the main body of the article (not in the supplementary material) ...*
Autors' response: We accept that wastewater chemical analysis data should be in the main text rather than the supplementary material. Therefore, we included this information for both the synthetic and real wastewater as Table 2 in the revised manuscript. However, we see this work as a conceptual attempt to discuss SAT operation and its effect on the biogeochemical state of the soil profile. Hence, the very specifics of the wastewater and soil, while important for the sake of completeness, are not the focus of this manuscript and could add unnecessary complexity to this type of paper.

GC 4 - *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).
Autors' response: This comment helped us understand that the main conclusions of this study were not highlighted well enough. It is true that qualitatively increasing DP will result in better oxygenation of the subsurface. However, the classic way SAT is being looked at is of a system where most of the oxidizing conditions (and hence removal of most of the ammonium and organic matter) happen in the very shallow subsurface. What we show here, we believe for the first time, is that longer DP also means extending the volume of the aerated subsurface, or increasing the volume of the 'pseudo-reactor', in our terminology. In other words, we extend the aerobically-active part of the system. We highlighted this conclusion in the revised 'Summary and Conclusions' section. We expect to further support our conclusions in a follow-up paper that includes the development and calibration of a full numerical flow and reactive transport model.

GC 5 - *The other conclusions are somehow weak and not put in a straigthforward 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...*
Autors' response: We thank the reviewer for this comment. The entire manuscript was revised and we believe it reads much better now. Moreover, in addition to the main points described above that are shown here for the first time in the context of SAT, the work described here assisted to develop a numerical model that will help to improve SAT operation under various conditions. Therefore, there will be a significant overall contribution both scientifically and practically (to SAT operation).

Specific comments (SC)

SC 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 traditional ? Information is missing or this sentence should be restructured*
Autors' response: We accept that the word 'Untraditionally' is not clear and even confusing. Hence, we omitted it. Our original intention was to refer to the fact that glucose is usually not the only carbon source in treated wastewater. Nevertheless, since glucose is easily degradable by bacteria (compared to more complex carbohydrates or humic acids that might be present in wastewater) and is common in wastewater treatment and SAT research, its use as the main carbon source allowed us to sustain the short wetting and drying cycles implemented in our experiments and also work in the desired ORP ranges. We included this explanation in the revised manuscript.

SC 2 - *(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.*
Autors' response: Data acquisition was every 1 minute. This information was added to the 'Materials ans Methods' section of the revised manuscript. The raw data was not manipulated or smoothened. The only processing step that was performed is correction of negative values recorded by the surface head sensor - when soil surface was completely dry the sensor would occasionally read small negative values. These values were set to 0. This is now clarified in the 'Materials and Methods' section of the revised manuscript.

SC 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,...).*
Autors' response: We fully accept the comment. The tensiometers and suction cups that were mentioned in the text were indeed used for qualitative verification of the flow and transport processes. However, since their results are not presented in this manuscript, we specifically stated it in the revised manuscript: "Tensio 150 (UGT GmbH) tensiometers for pressure head and ECO Tech Bonn (1.5 cm diameter) ceramics were installed along the column as well. While their data is not shown here, it fully supports our presented findings".

SC 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.*
Autors' response: We accept the comment. In light of the changes we made in SC 3, this table seems to be of minor value to the reader. It was omitted from the revised manuscript.

SC 5 - *(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/DOsat if the temperature is known at any time of measurement.
Autors' response: This indeed is a point that we had hard time deciding on. On one hand, as the reviewer states, normalized values may be more beneficial as they provide immediate and direct notation of aeration. On the other hand, most readers, so we feel, are more comfortable with actual water content values . Therefore we choose to leave values as are.

SC 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 sub figures is simply wrong!). If it is the latter, the notation should be NH4-N (mgN/l) for ammonium and NO3-N (mgN/l) for nitrate.*
Autors' response: DOC and TKN analyses results are reported in our work in mg/L (of C and N respectively). For NO_3^- - and NH_4^+ we initially chose to use mg/L units (mg of the species per liter). However, we accept that consistent use of units is preferable and hence we now use $NO_3^- - N$ and $NH_4^+ - N$ in mg/L as was suggested .

SC 7 - *(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.
Autors' response: The SHAFDAN sites infiltration ponds' operation regime, location and and characteristics were described in multiple publications before. We, therefore, referred to some of them in the introduction and in the 'comparison with field observations' section (e.g. -Icekson et al., 2011, Goren et al.,2014). Section 3.1 of the manuscript shows qualitative agreement between the field and the columns experiments' results. Since the field and laboratory SAT systems are very different in many ways, and especially scale and dimensionality, this agreement is exceptionally interesting and points to the fact that regardless of the major scale differences, some of our findings (i.e. deep aeration and extension of the aerobically-active zone) are relevant to full scale field SAT systems. In that sense, the SHAFDAN site was merely the inspiration to this chapter and not the focus of it. Nevertheless, to allow the reader easy access to the full information, we included a short description of the SHAFDAN site in the beginning of section 3.1. In addition, a comprehensive description was added to the revised 'supplementary material' document.

Technical comments

Referee 2's technical comments are summarized in the following PDF file: https://www.hydrol-earth-syst-sci-discuss.net/hess-2019-371/hess-2019-371-RC2-supplement.pdf
Autors' response: A full revision of the manuscript was performed. Minor comments (e.g. typos, word selection suggestions etc.) were corrected according to referee's suggestions. More general comments are addressed below:

Figures - Referee suggested multiple adjustments to the figures.

Autors' response: We carefully considered each of the specific comments and we believe that all figures were improved thanks to Referee 2's constructive comments. Specific changes we made according to the comments are hereby reported:
Figure 1: The labels denoting the modules of the column were omitted and the port positions labels were adjusted to a bigger font.
Figure 2: According to Referee 2's suggestion, we added the depths next to each of the a-e sub-plots.
Figure 3: We accepted Referee 2's suggestion to separate the different stages of the experiment by a dashed line and added a clear label denoting 'stage 1' and 'stage 2'. We accept that a presentation of the x-axis in 'days' might be easier to read for long time-series. However, for a system that operates at cycles of hours with no meaning to day/night (sunlight), we feel that this will not help, rather it will make the presentation cumbersome. For example, our FP will be 1/24 days). Therefore, we would rather keep time units in minutes.
Figure 4: The legend of the figure was corrected according to the comment. However, we disagree with the idea of connecting measurements with a straight line. A line connecting two data points implies that a linear trend is assumed. We do not assume that and thus, we believe that singular data pints are more suitable for this figure.
Figure 5: According to Referee 2's suggestion, we changes the y-axes of both sub-plots (Figure 5 a and b) to have the same range of values. The depth of the field measurements was added to the caption of the figure (note that it is also mentioned in Line 223). However, we disagree with the notion of connecting ORP measurements with a straight line. In addition to the above, in the case of the field data, each point represent an independent infiltration campaign. Hence, connecting the dots would not describe accurately the presented data.

Line 26 - DOC, NH_4^+ and organic nitrogen concentrations of secondary effluent at the SHAFDAN site are presented in μM. Referee suggested to convert to mg/L

Autors' response: We accept Referee's suggestion. Units were converted to mg/L.

Line 86 - Referee commented that the terms Flooding periods (FP) and Drying periods (DP) were defined before.
Autors' response: The terms FP and DP were indeed defined before. However, this sentence was specifically phrased to clarify authors' interpretation of the terms as it was used throughout the manuscript. Thus, in this case we believe the current wording is appropriate.

Line 135 - Authors included timing of the water front. Referee commented that this information is not informative
Autors' response: We accept the comment. This line was omitted.

Line 136 - Authors mentioned 'classic infiltration theory'. Referee suggested to refer to a specific model
Autors' response: We accept the comment. By 'classic infiltration theory' we intended to refer to simple sharp-front models such as the Green and Ampt infiltration model. We added this information in the revised manuscript.

Line 147 stated "As observed in multiple studies in laboratory and field work, close to the surface, DO concentrations are expected to increase in response to the soil aeration during the DP since regardless of the oxygen movement mechanism (diffusion, advection or convection), the short distance ensures fast response of the

system". Referee commented that this is not new information
Autors' response: This is indeed known information that was previously shown by others. We included this line to emphasize the difference between the expected oxygen recovery behaviour in the shallow parts of the profile compares to the deeper parts (that are discussed in the next paragraph)

Line 162-163 - Referee commented that the sentence is missing the subject and thus is not meaningful
Autors' response: We thank Referee 2 for the attention. The sentence was corrected.

Line 177 - Referee suggested to calculate re-oxygenation rate instead of the use of the term 'oxygen recovery'.
Autors' response: We thank Referee 2 for the suggestion. We acknowledge that re-oxygenation rates may be valuable information for the understanding of some reactors or filters that are well-mixed or of fixed volume. In this case, however, the increase in DP in response to the longer DP varied between the different depth of the column. For example - while the deepest parts of the column were able to sustain DO concentrations of $\sim$3 mg/L (during the longer DP experiments), the term 're-oxygenation' does not accurately describe the system's behavior. Further, one of our main findings is the relation between DP and the 'oxidizing volume'. After careful consideration, we believe that the use of the term 'DO recovery' is more suitable for the purpose of the sentence.

Line 186 - Authors stated: "Considering the fact that sustaining the shorter DP of stage 1 (of experiment 2) would result in total DO depletion $\sim$175 cm depth (supplementary

material), these are very important observations. Referee commented that the importance of the sentence is not clear to him/her.
Autors' response: This line expresses one of the important points of our work. Studies have shown before that long DP are beneficial for the upper $\sim$ 1 meters of a SAT profile in terms of DO concentrations and oxidation rates. While this is correct, we demonstrated here that deeper areas (in this specific sentence $\sim$175 cm depth) displayed a significant DO increase in response to the longer DPs. This means that longer DPs lead to extension of the aerobic volume of the SAT 'pseudo reactor'. The referral to the fact that sustaining the shorter DPs would lead to complete oxygen depletion in this depth is important for comparison reasons, but we believe that displaying the figure in the main text does not add additional value to the purpose of the claim.

Line 205 - Authors reported α value for the statistical t-test performed. Referee suggested to display p_value instead.
Autors' response: In the text, we use phrases such as 'significantly higher concentrations' to denote the statistically significant difference in outflow concentrations between experiments 3 and 4. To provide the reader with the information on the significance level we chose for the tests, we report the α value that was the same for all the concentration pairs examined in the t-tests (i.e. DOC, TKN and NH_4^+).

Line 254 - Authors stated that inflow DOC, TKN and NH_4^+ content was matched between the synthetic and the real wastewater. Referee pointed this information should be stated in the 'methods' section.
Autors' response: Although the review provided was very detailed, this was probably missed. This information is stated in the 'Materials and Methods' section (Line 113).

Lines 262-272 - Referee pointed that this paragraph is too vague and hard to follow.
Autors' response: This section was completely revised. The revised paragraph includes a comparison of our findings to a paper by Ak et al.,2013, that compared organic matter removal in a series of column experiments with synthetic and real WW. We discuss the similarities between their results and our findings and also the differences and the possible reasons for them. We believe the revised paragraph is much clearer and better reflects the concept it addresses.

Summary and conclusions - Referee pointed that there is a change in tense between the first and second paragraphs.
Autors' response: We thank referee 2 for the attention. The 'Summary and Conclusions' section was fully revised and all comments were addressed

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