Dear Dr. Gerrit H. de Rooij,

We greatly appreciate the helpful and constructive comments from the two reviewers and the editor of our manuscript entitled "**Continuous Monitoring of a Soil Aquifer Treatment System's Physico-Chemical Conditions to Optimize Operational Performance"** by Turkeltaub et al. (Paper hess-2021-455). In the following response we address all the specific comments made by the reviewers. The specific comments received are highlighted in *blue italics* and our responses are in black. The line numbers refer to the numbers in the revised manuscript. We believe the comments helped us strengthen the manuscript and that it is now ready for publication at HESS.

Editor:

Comment 1: *Please check the guidelines for authors about the notation of variables (e.g., the use of italics) and units. You only have to explain variables once, on first occurrence.*

Reply to Comment 1: We have revised the manuscript according to the guidelines including the replacing the word 'Figure' with the abbreviation 'Fig.' (lines: 129, 130, 140, 146, 150, 153, 156, 161, 177, 185, 186, 190, 191, 196, 197, 222, 224-227, 229, 235, 239, 240, 243, 244, 247-249, 253, 255, 258, 259, 263, 264, 266, 267, 303, 306, 310-312, 319-321, 329, 331, 333, 336, 339, 342, 345, 354, 375, 377, 380, 382-385, 408, 411, 414-416, 422, 428, 430, 431, 435, 451, 452, 454, 482, 483, 486, 490-496, 498, 499, 501, 503, 509, 511, 524, 554; caption of Fig. 2, caption of Fig. 3, Table 2).

Comment 2: *Please replace Eh and SAT by terms that can be understood without referring to the text.*

Reply to Comment 2: The text has been revised (lines 15-16).

Comment 3: *Line 87: 'arability' The only meaning I can find for this word is 'suitability for agriculture', which does not really apply here.*

Reply to Comment 3: Thanks for catching this. The word 'arability' was mistakenly used, we meant to say 'availability'. The text has been revised (line 88).

Comment 4: *Line 109: 'water table' The water table is neither an environment, nor can it be measured by a redox-potential sensor.*

Reply to Comment 4: We replaced 'water table' with 'Groundwater' (line 110).

Comment 5: *Line 128: 'Km' lower case*

Reply to Comment 5: The text has been revised (line 129).

Comment 6: *Line 140: replace 'various depths' in 25, 50, and 100 cm depth (l. 148?)*

Reply to Comment 6: The text has been revised (line 141).

Comment 7: *Line 146: You declared theta as the symbol for the volumetric water content above. Why do you need 'VWC'?*

Reply to Comment 7: We removed VWC from the text and used only the θ symbol to refer to the volumetric water content. We have revised the manuscript accordingly (lines 150, 153, 222, 224, 229, 258-259, 263, 266, 524, 554; caption of Fig. 2, caption of Fig. 3).

Comment 8: *Line 156: "Inbar" regulations, What is this, and does the readership needs to know?*

Reply to Comment 8: We provided a reference for the 'Inbar' regulations (lines 157, 662-664).

Comment 9: *Figure 1: HESS is apolitical and the guidelines stipulate that maps should adhere to UN naming conventions and avoid mapping borders. Contested borders may be labelled as such by the editors.*

Reply to Comment 9: Figure 1 has been revised accordingly.

Comment 10: *Line 173-174: L is time-dependent then, because it is equal to the depth of the wetting front. In that case, this would be the Green-Ampt model slightly modified for non-zero ponding depth and water-entry value.*

Reply to Comment 10: We agree with the editor's comment, this is a Green-and-Ampt equation for infiltration into a flooded soil. The text has been revised to indicate this (line 174).

Comment 11: *Line 175: This is unit-gradient flow. If I am correct you assume the water content throughout the unsaturated zone to be uniform over its entire depth and vary with time only. Perhaps it is good to mention this.*

Reply to Comment 11: The water content is assumed to be uniform for the simulated depth and changes only with time. We have provided an explanation and the text has been revised accordingly (line 179-181).

Comment 12: *Line 179-180: '(also effective porosity)' What does this mean?*

Reply to Comment 12: We meant to emphasize that the saturated water content represents the portion of the porosity that contributes to the water flow. However, it is out of context in the current study and the text has been removed.

Comment 13: *Line 189: I believe the subscript i is missing in Eq. (4)*

Reply to Comment 13: Equation 4 has been revised accordingly (line 195).

Comment 14: *Line 387: Are you sure?*

Reply to Comment 14: The day should not be with -1. We indicate that it takes about 1.25 days for the solution to be de-oxygenated. The text has been revised accordingly (lines 425-426).

Comment 15: *Line 390: Really?*

Reply to Comment 15: Please see reply to comment 14 above (lines 425-426).

Comment 16: *Lines 396-399: If I understand you correctly, low microbial activity, not the availabilty of oxygen, may limit the purification of infiltrated effluent when the temperature is low. This would imply that the even a combination of redox-potential sensors and oxygen sensors may not adequately signal reduced breakdown, and therefore overestimate the permissable wetting stage. But you leave out the oxygen sensors and only mention the redox potential sensors.*

Reply to Comment 16: The combination between E_h and gaseous O_2 measurements can provide a good estimation for the optimal wetting length during summer. During winter, the decrease of E_h is moderate compared to the decrease during summer. This can be partly explained by the longer presence of gaseous O_2 in the SAT vadose zone (see new Fig. 8). Nevertheless, following the depletion of gaseous O_2, no further substantial decrease in E_h conditions is observed (especially compared with summer). This is attributed to lower microbial activity under lower temperatures. Therefore, the adequate definition of the wetting cycle length during winter should be supported with other observations. From a practical perspective, we suggest following the summer definition of the cycle wetting length. We modified the 'The length of the wetting stage according to E_h measurements' section where Figure 7 now includes the redox measurements from all depths. Furthermore, we provided a new figure, Figure 8, which describes the average changes in gaseous O_2 from the start of the wetting cycle (lines 401-461).

Comment 17: *Line 396: replace 'intensive' with 'high'.*

Reply to Comment 17: The text has been modified.

Comment 18: *lines 480-481: The oxygen data should also provide useful information, I believe.*

Reply to Comment 18: Please see reply to comment 16 above.

Reviewer: 1

General comment 1: *Obviously, prevailing of unsaturated conditions immediately below the shallow saturated layer would impact dramatically the oxidation conditions as well as the analysis of the percolation conditions. The impact of the gas phase in the unsaturated layer very close to the surface, is well reflected through the O2 and Eh sensors at 50-100 cm, as can be observed in figures 4 and 6. Obviously these are the most important parameteer for efficient water treatment. However in the is manuscript, most of the analysis that refers to the treatment efficiency refers mainly to 25 cm which is in the very shallow saturated part (e.g. information presented in figure 7 8 and 9), ignoring the hydraulic and oxidizing conditions under that layer. Accordingly, I believe that the authored should elaborate on the potential impact of the unsaturated conditions that prevail vary close to the shallow saturated part, on both the infiltration and treatment conditions.*

Reply to general comment 1: Our main motivation to focus on processes that occur in the topsoil (25 cm depth) was driven by previous studies, which indicated that most of the removal processes occur at topsoil and further contribution of deeper parts of the vadose zone are negligible (Lin et al., 2008; Grinshpan et al., 2022; Fox et al., 2005; Quanrud et al., 1996, 2003; Miller et al., 2006; Essandoh et al., 2013; Sopilniak et al., 2018; Goren et al., 2014; Sopilniak et al., 2017). To demonstrate that the previously stated outcomes in the first version of the manuscript do not change substantially, we included further analysis of the E_h observations from 50, 75 and 100 and O_2 observations from 50, 75 and 150 cm depth of the SAT vadose zone.

Figure 7 now includes E_h observations that were obtained at 25, 50, 75 and 100 cm depth of the vadose zone. An additional figure is provided, i.e., new Fig. 8, which describes the gaseous O_2 concentrations in the SAT vadose zone at 25, 50, 75 and 150 cm depth during the recorded wetting cycles. Fig. 9 (previously Fig. 8) describes the E_h conditions and the gaseous O_2 concentrations during the drying stage for winter and summer. Further discussion was provided to describe the differences between the different depths and the possible impact of the unsaturated conditions on the SAT efficiency (lines 413-461).

Specific comments:

Comment 1: *One more aspect which is rather technical refers to the structure of figure 9. It is a very strange presentation where O2 concentration is presented Vs depth in multiple times, while the depth is constant and the time is variable. The data should be presented as O2 concentration Vs time in single or multiple depths.*

Reply to comment 1: Figure 9 has been revised accordingly. Note that Fig. 9 includes the E_h measurements during the drying stage as well as the gaseous O_2 observations.

Reviewer: 2

General comment 1: *My main questions about the study are, why was DO not measured in the vadose zone of the infiltration basin? The authors could have collected pore water samples and analyzed the pore water for DO in situ. It seems the study is trying to use redox potential as a proxy to capture the decline in DO in the SAT system, however, no DO data was collected or presented?*

Reply to general comment 1: We agree with the reviewer's comment that dissolved oxygen (DO) measurements would provide further information regarding the removal rates and redox conditions in the SAT subsurface. DO sensors were installed at the site, but due to technical problems and limitations, no reliable dissolved oxygen data could be obtained. Our understanding and personal experience is that this low reliability of DO sensors in variably saturated and dynamic field conditions is common.

We claim that the E_h is a useful practical tool to define the optimal duration, from a geochemical perspective, of a wetting and a drying cycle in SAT. Note that the good correlation between DO and E_h measurements deviates once the DO is depleted, since the E_h conditions continue to decrease as other oxidized species presence in the pore-water solution (e.g., nitrate). Brettar et al. (2002) indicated that the end of the denitrification process at topsoil occurs at the value of 0 mV and for deeper soils at a value of 100 mV. These conclusions were explained by the higher carbon availability at topsoil compared to deeper parts of the soil. Ultimately, the E_h observations at the SAT vadose zone enable the prevention of establishment of anoxic conditions (100mV >), where no O_2 or NO_3 are available. This discussion is now provided in the manuscript (lines 413-423).

General comment 2: *I find the analysis and interpretation of the drying cycle data a bit short/insufficient. The authors conclude that a 36-hr drying period is optimal for both the summer and winter season, irrespective of the starting conditions in redox potential at the beginning of the drying cycle. Yet the analysis does not fully explore or explain why Eh is recovering so much faster in the summer than in winter.*

Reply to general comment 2: We suggest that the E_h recovery is dominated by the rates of gaseous O_2 intrusion to the soil. During winter, the gaseous O_2 concentrations show moderate increase with time, while during summer all the observed O_2 curves show a steep recovery that ceases after about 20 hours. The recovery of the E_h conditions reflect comparable trends. During summer the E_h conditions increase rapidly and during winter the E_h recovery shows moderate increase with time. The revised Fig. 9 displays the differences between oxygen intrusion during winter and summer and an additional discussion was provided (lines 489-511).

Specific comments:

Comment 1: *Line 114: Suggest rephrasing to "…was to examine the temporal variability in redox potential…"*

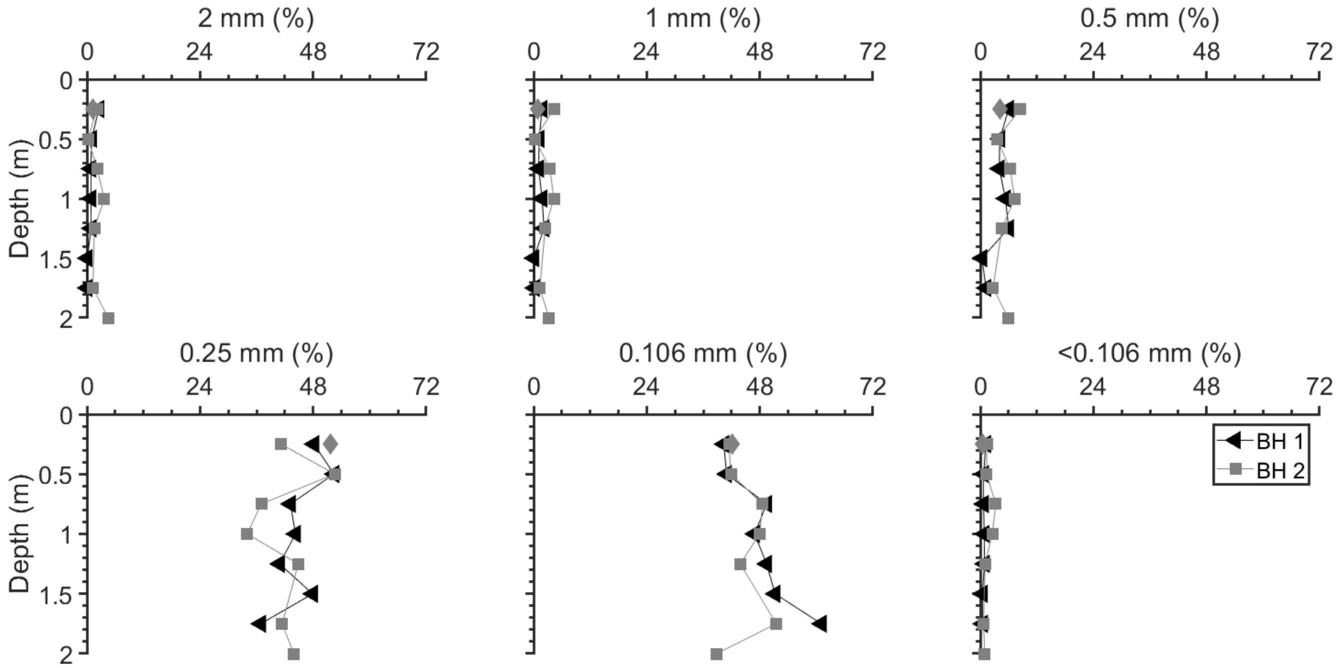
Reply to Comment 1: The text has been revised (lines 115-116).

Comment 2: *Line 116: Seasonal changes in climate such as rainfall and temperature are likely to also influence the wetting/drying stages and not just operation of the SAT system. I would suggest mentioning climate or season in line 116.*

Reply to Comment 2: The text has been revised accordingly (lines 117).

Comment 3: *Line 135: You state that the surface of the spreading basins is plowed on a regular basis to prevent clogging. In the past 10-15 years operators of infiltration basins have moved away from this practice because they observed that the plowing allowed fine particles to move deeper into the vadose zone (e.g. 1-3 m), where they would accumulate and form a flow impeding layer. Have you run any geophysical scans of the vadose zone underneath the infiltration basins whether percolation has been impacted by the plowing?*

Reply to Comment 3: During the establishment of the monitoring stations, two boreholes were drilled to 2 m depth and soil samples were collected at 25 cm intervals (8 × 2 soil samples in total). The soil samples were analyzed for particle size distribution (PSD). According to the PSD analysis in the figure below, there is no indication for accumulation of fine particles or establishment of a layer with low permeability. The figure below is provided in supporting information (Fig. S3).



Comment 4: *Line 145: were the suction cups installed at the same depth as they sensors?*

Reply to Comment 4: The suction cups were installed at similar depths to the other sensors. The text has been revised to clarify this point (lines 146).

Comment 5: *Line 195: Could you please clarify if the data on the long and short cycles are averages over the stated periods (e.g. Nov-April) or what is the time frame for these? If so, please also add a column stating the number of event (N). In addition, please clarify if the duration of these stages was set by the operator of the basin or whether there was a systematic operation scheme that was tested in this study. It is not quite clear who defined the long and short cycles.*

Reply to Comment 5: We agree with the reviewer's comment that there might be a confusion concerning the different data sources. The data presented in Table 1 was provided by the Shafdan operators. Note that the number of recorded cycles is included in Table 1. Our monitoring systems collected data every 20 minutes as mentioned in the Method section (lines 146 - 154). The text has been revised accordingly (lines 204-216).

Comment 6: *Line 217: Please clarify what you mean by "high saturation values" – field capacity, 80% pore space filled?*

Reply to Comment 6: We removed the word high and stated that the soil remained at similar level of saturation throughout each wetting stage according to θ observations (line 235).

Comment 7: *Line 225: Add "parameters" after Ks and β.*

Reply to Comment 7: The text has been revised accordingly (lines 243).

Comment 8: *Line 234: How is the soil drainage process defined in the operation? Is there a minimum water content or redox value that needs to be achieved? If so, please state it?*

Reply to Comment 8: The water levels at the soil surface of the Shafdan ponds are monitored with water level sensors. As can be observed in Figure 2, the drying process starts when no water is present at the soil surface (black line). Nevertheless, the water level is measured only at a single point (close to the pond inlet). Therefore, some variations in water heads, at the order of a few cm, may exist due to microtopography and the distance between the inlet and the far parts of the pond. This may lead to some delays in water arrival or recession from the stations. We added the definitions to the text (lines 208 - 212).

Comment 9: *Line 246: How do you explain the higher VWC value during the winter?*

Reply to Comment 9: We claim, by using a hydrological modeling approach, that the soil physical properties alter during winter under long wetting stages (lines 261 - 279). The decrease in biodegradation of organic matter (OM) during winter causes accumulation at the topsoil of the

SAT. Increases of OM in the soil is reflected in the increase of the VWC. This is in addition to changes in viscosity and density that further change the retention and conductivity of the soil.

Comment 10: *Line 247: Do you mean "explore" instead of "elaborate"?*

Reply to Comment 10: The text has been revised accordingly (lines 263).

Comment 11: *Line 254: Please elaborate on how the winter cycles affect infiltration capabilities.*

Reply to Comment 11: Due to the lower biodegradation during winter, there is an accumulation of organic matter in the vadose zone of the SAT. The low specific surface area of sandy soils (~0.0077 g m^{-2}) compared to clay (~900 g m^{-2}) (Doerr et al., 2000) implies that a limited amount of organic matter is required in order to coat the sand grains and to develop soil water repellency. Arye et al. (2011) showed that soil hydrophobicity is attributed to the reduction of the liquid surface tension and increasing of the contact angle. These changes in soil properties are related to the reduction of the soil permeability. This explanation is now provided in the text (lines 269 - 277).

Comment 12: *Line 294: What is the Ksat at the site? It is a bit surprising that Eh is recovering so quickly. It is hard to determine based on Figure 4, hence I would recommend stating average recovery times in days or hours for Eh to return to positive values. Is this typical for these infiltration basins? What is the retention time of the wastewater in the unsaturated zone?*

Reply to Comment 12: The subsection 'The length of the drying stage using E_h and gaseous O_2 measurements' (lines 478 - 511) elaborate the issues regarding the E_h recovery time. The estimated Ks for the site is presented in Table 2. In a recent study by our group we found similar Ks values for a different infiltration basin of the Shafdan (Grinshpan et al., 2022). According to our estimations, the average pore-water velocity ($q/\theta s$) is between 2.8 and 2.2 cm/h. For a vadose zone thickness of 35 m, the retention time should be between 52 and 70 days. These estimations are in accordance to previous residence times suggested by Elkayam et al. (2015).

Comment 13: *Figure 6 is indicating that the infiltration basin has an inverted water table below the basin bottom which is maybe 50-80 cm thick. Below this inverted water table oxygen content and Eh seem to be higher potentially indicating unsaturated conditions. Most denitrification is therefore occurring withing the saturated zone (or inverted water table) below the basin bottom, which varies in thickness depending on texture. Gorski et al. 2019 (ES&T) recently summarized some of these dynamics in a nice conceptual way, which could be helpful for this study. Have you checked whether the saturated thickness is changing with the duration of the wetting and drying cycles as well as season?*

Reply to Comment 13: According to the gaseous O_2 measurements, unsaturated conditions prevail at 150 cm during most of the monitoring period. It takes a longer time for the gaseous O_2 to disappear at 25, 50 and 75 cm depths during winter (see the new Fig. 8). However, we are not sure if the absence of gaseous O_2 indicate saturated conditions. As can be seen in the Reply to Comment 3, above, there is no evidence for substantial soil texture variability. Therefore, we do not anticipate considerable changes in the saturated thickness. The conceptual model by Gorski et al. (2019) is indeed interesting and relevant and was discussed earlier in the manuscript (lines 316-325). In addition, to accurately measure the saturated thickness both the volumetric water content (θ) and soil matric pressure (ψ) should be measured simultaneously at multiple depths. Unfortunately, since it was not in the scope of the current study, we did not install pressure sensors.

Comment 14: *Line 388: Changes in Eh are usually lagging behind changes in oxygen content as highlight in Figure 4. Unfortunately this study does not show DO data for the pore water at 25, 50 and 100 cm depth, therefore it is difficult to estimate whether Eh is indeed a useful proxy for predicting DO depletion.*

Reply to Comment 14: Please see reply to general comment 1 above.

Comment 15: *Figure 8: It would have been helpful to overlay the recovery of the volumetric water content since start of the drying cycle on top of the Eh recovery. The steep increase in Eh around 18 hrs since start of the drying cycle could be supported by the higher ET during* *summer. I am also wondering if the timing of the operation plays a role (e.g. operator stops flooding at the end of a workday (e.g. evening), hence ET is highest the next day around noon or 18 hours later)?*

Reply to Comment 15: We plotted O_2 data together with the E_h observations during the drying cycle in the revised Fig. 9. The timing of operation does not play a role since the effluent supply to the infiltration basin is according to requirement and it is not affected by the operator's availability.

References

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