Response to Comments – Referee #1

I think that the paper is of good general quality, with clear and well-structured writing, and a valuable scientific contribution overall. After having read (and quite enjoyed) the manuscript, I only have a few comments, many related to clarity and rigor of equations and symbols. However, I would also like the authors to discuss with care the representativity of their results based on the choices made regarding configuration parameters (see comment on Line 191). All comments below:

We would like to thank Dr. Sole-Mari for his time reviewing our manuscript and the feedback offered to help us improve the rigor and clarity of our manuscript. We are glad you enjoyed reading our work! In general, we will address the problems with the notation in the equations and we will add clarifications to the model constraints and the derived analyses. Below, we address each comment in detail and outline specific revisions.

Equation 1 and elsewhere: I think it is formally wrong to define K(h) and K(n) with the same exact symbol, which would seem to imply that they are the same exact function, and that here you just evaluate it for either h or n. You should use two different symbols for these two different factors.

The symbols **K(h)** and **K(n)** are replaced by $\mathbf{K_u(h)}$ and $\mathbf{K_c(n)}$, respectively, to indicate that they are different functions. The former refers to the change of hydraulic conductivity due to unsaturated conditions and the former refers to the change due to clogging.

Equation 2b: I think you should point out at some point that the capillary head h is always negative, even if that might seem obvious to the authors, I think it is worth clarifying in order to ensure good interpretation of the equations with the minus signs in front of alpha h.

The following sentence with this clarification will be added to the manuscript.

"Under unsaturated conditions, $\mathbf{h < 0}$, whereas under saturated conditions, a positive pressure head replaces the matric head and $\mathbf{s_e = 1}$"

Equation 5: There seems to be a typo, the advection term should indicate the divergence of qCi (mind the dot as well as the parentheses).

The typo is corrected.

Equations 5 and 6: Each species Ci should have a different reaction rate Ri, which is possibly a function of many other different "Cj". I believe you are wrongly using the parentheses (which should stand for "function of") as if they were part of the function's symbol. Brief: Ri, not R(Ci), and same for equations 6.

The notation to represent the reaction rates has been revised as suggested, and now reads $\mathbf{R_i}$, where the subindex **i** refers to either organic carbon or dissolved oxygen.

Line 100: X X

The typo has been corrected. This was meant to read $\mathbf{X_j}$ which is the mass of biomass per unit volume. The subindex **j** refers to the different types of biomass considered in the model.

Equations 9 and 10: Same comment as eq 5 and 6.

The notation has been replaced here as well. They now read $\mathbf{R_j}$, where the subindex **j** refers to the biomass fractions.

Lines 182-185: So you imposed a fixed gradient boundary condition at the inlet which changes over time in order to always get the 1mL/min inflow (or 0mL/min in dry conditions) ? So you kind of imposed a fixed flow at the inlet *boundary, or how is it different? Also, this reads "as shown in Figure 3", but Figure 3 doesn't really show much about how the boundary conditions are implemented. Maybe Figure 3 could indeed be improved to include more information.*

> The fixed flow of 1mL/min is imposed until the system is clogged and ponding occurs. Since the hydraulic conductivity changes over time, the head gradient that needs to be imposed to maintain the fixed influx condition is recalculated at every timestep. From this calculation, the head value at the boundary is calculated. When the capillary head at the boundary becomes positive, it means that ponding is occurring. The fixed flow boundary condition is applied only until ponding occurs, after which the boundary condition becomes a constant head corresponding to the ponding in the experiment. We will add this explanation to the Methods section, and Figure 3 will be updated accordingly to better illustrate how the boundary conditions are set.

Line 191: Why that choice of 450min? (which to me would seem like quite a short wetting time for operational realism). Later (section 3.5) you do seem to confirm this suspicion by finding quite a longer optimal cycle time, but at that point you have already fixed the dry-wet time ratio at 4.5. This leaves me wondering, for instance, if there isn't a more optimal strategy that uses rather long drying periods and also a lower dry-wet time ratio maximize hydraulic loading (unless I am missing something). I guess you could say that this is a first attempt and a framework which can be used for further investigation and optimization, but I think that some discussion around this possible limitation of the study is missing. In other words, you do discuss as of now that there are these two configuration parameters (wet-dry time ratio and dry time), but it should be made clear that there is probably a complex interplay between them and that for instance, for each different dry time, one may find different results regarding the role of the dry-wet time ratio.

> We chose that specific case just to demonstrate the effect of varying the $\mathbf{t_{dry}/t_{wet}}$ while keeping $\mathbf{t_{wet}}$ constant (Figure 6), and the effect of varying $\mathbf{t_{dry}}$ while keeping the $\mathbf{t_{dry}/t_{wet}}$ ratio constant (Figure 8). We determined that this would be an interesting case to show the relation between biomass spatial distribution and hydraulic controls, as these trends were common among simulations. We ran the model for a wider range of $\mathbf{t_{wet}}$ and $\mathbf{t_{dry}}$ combinations and will add a Supplemental Figure showing the hydraulic efficiency results for these combinations. However, we did not intend to find a global optimum that is directly applicable to full-scale SAT applications. As you correctly point out, wet and dry periods are longer in full scale SAT systems. For the current SAT operation at Shafdan, wet periods are typically 1-2 days and dry periods are 2-4 days (Sharma & Kennedy, 2017; Idelovitch & Michail, 1984). We will add discussion to the manuscript on the need for the model to be scaled up to optimize pilot and full-scale SAT systems.

Line 295: I would say it as "Therefore, our results would suggest that neither…". Mostly because like I said earlier, you have not really simultaneously explored different ratios for different drying times.

> We ran the model for a wider range of $\mathbf{t_{wet}}$ and $\mathbf{t_{wet}}$ combinations and these findings were common among simulations. But we recognize that we make this statement based on our simulation, so it will be modified following your recommendation.

Line 325: ,,

> The typo has been fixed.

Sincerely,
Guillem Sole-Mari

References

Idelovitch, E. and Michail, M.: Soil-Aquifer Treatment: A New Approach to an Old Method of Wastewater Reuse. Journal (Water Pollution Control Federation) , 56, No. 8, Conference Preview Issue (Aug., 1984), pp. 936-943, 1984.

Sharma, S. K. and Kennedy, M. D.: Soil aquifer treatment for wastewater treatment and reuse, International Biodeterioration & Biodegradation, 119, 671–677, https://doi.org/10.1016/j.ibiod.2016.09.013, 2017.