Report on the paper "A mathematical representation of microalgae distribution in aridisol and water scarcity" by A. Lababpour

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The author presents a model consisting of two diffusion-reaction equations for biomass and water development. The resulting model equations are solved using a Matlab toolbox. The author thoroughly discusses extensions of the modeling approach and outline directions of further research. He particularly discusses influencing factors for the three dimensional development of the soil biocrust. Although this part is quite well explained and the topic itself is of interest, I do not recommend the manuscript in its current stage for publishing in Biogeosciences. For details see the following comments below.

1. The model is very simplified although possible reasonable extensions are discussed. The predictive power is therefore questionable. A quantitative discussion e.g. in comparison with with data should be added.

2. The model under consideration and its precise mathematical formulation remains unclear. The author should clearly differentiate what could be done and what is done within the current research. The model that is implemented it not clearly stated in its whole at any point. Exactly this model should be summarized once in the paper including the domain of definition, the used variables and parameters.

   (a) The geometric setting is unclear. First the three dimensional domain $[0, L_x] \times [0, L_y] \times [0, L_z]$ is discussed. This is thereafter changed to the half space (which actually is not the half space but the first quadrant). Thereafter a square $[0, 4]^2$ is considered.

   (b) It is unclear how the porosity enters the investigations. The equation (4) is reasonable only in case that the porosity is independent of space. Otherwise I would expect that the diffusion terms reads

$\nabla \cdot (nVD_B \nabla B)$. However, it seems that there is no modulation of the porosity throughout the simulation scenario, e.g. in the initial conditions.

(c) The inclusion of the illumination rate into the growth term of equation (4) is unclear. How is I related to μ and how is it determined/defined in the simulation scenario? Along the same lines d is undefined in (8). Does the logistic model enter the proposed model in terms the growth rate μ in (4) and (8) and if so how does it?

(d) The statement of boundary condition in (11) makes no sense. First the boundary conditions must be prescribed on the boundary of the domain rather that in the domain itself. Second the variables are independent quantities. I assume the author means $B = 0.02$ and $w = 0.2$ are prescribed on the boundary.

3. The outcome is of the simulations are not convincing:

(a) The scaling in Figure 2 and 3 is unclear. Variations are seen in order 10^{-4} or even below. This seems not to be relevant compared to the initial/boundary values or even the reference value of 0.7921 and 0.5198 in Figure 2 and 3. The smallness of the variations could even be due to numerical or rounding errors. It is remarkable that the values for B and w are within the same order of magnitude directly although starting with one order of magnitude difference initially.

(b) The initial conditions are not matched for $t = 0$ in Figure 2 and 3. This does not make the results reliable.

(c) It is unclear for which time the spatial distribution is plotted. The values are very small (order of 10^{-4}) compared to the chosen initial values. Is zero maybe a steady state solution?

(d) Explicit comparison to data is missing. This could maybe shed some light into the results and make the discussion more quantitatively.

4. The language should be improved throughout the manuscript.