

## ***Interactive comment on “A mathematical representation of microalgae distribution in aridisol and water scarcity” by Abdolmajid Lababpour***

### **Anonymous Referee #1**

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In the manuscript entitled “A mathematical representation of microalgae distribution in aridisol and water scarcity”, Lababpour presents a mathematical model of microalgae development on surfaces. The presented model attempts to describe the interaction between cyanobacteria and soil water with the possible inclusion of climatic variables. Although the approach of using a reaction diffusion equation of biomass and water is interesting, the manuscript does not provide in-depth tests to grasp the applicability and predictability of the model. Furthermore, the presented equations raise some questions regarding the model capability that author discusses. I enjoyed reading about the work, but have several major issues with the model and its presentation for the publication.

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Firstly, the author describes the growth of cyanobacteria to be a function of light intensity (which should be time dependent, day and night) and water. However, the growth term is not well described in the manuscript and the solution does not reflect such behaviour.

Secondly, unlike the title highlights that the model is for aridisol, the mathematical description does not include any of the properties of such soils. Using water as one of main variables and introducing the multiplication of the porosity  $n$  might imply such application to dry soils. However, the coupled PDE only replaces the limiting nutrient to water in the conventional reaction diffusion equation for microbial growth. Especially, without including the input and output of water in the equation, the role of soil physical property (porosity in this work) cancels out, thus no soils in the equation. To be constructive, using water content instead of water concentration (it is quite difficult to understand what this term means) can be considerable to include properties of soils. The Richards equation with the saturation based soil water diffusivity can provide the proper water dynamics in dry environments.

Thirdly, the example solution of the model in this manuscript is too simple, even trivial. Simulating without any heterogeneity in the domain and uniform distributions of both variables as initial conditions should result in no differences during the dynamics (as the figures show and the author has mentioned in the result section). Furthermore, I cannot find any physical reasoning for the used boundary condition, equation (11).

Finally, the units provided in Table 3, do not seem to be correct. All units and dimensions need to be checked again in the model.

Unfortunately, the model has major issues and needs further development. The attempt, however, to seek for a simplified representation of such a complex system is gratefully acknowledged.

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