

Interactive comment on “On the role of soil water retention characteristic on aerobic microbial respiration” by Teamrat A. Ghezzehei et al.

A. Ebrahimi (Referee)

ali.ebrahimi@usys.ethz.ch

Received and published: 16 July 2018

The manuscript proposes a new modeling framework that integrates the important role of soil water potential on regulating the rate of soil respiration. The model is built on assuming a single pool soil organic matter (SOM) where a first-order kinetics for the rate of SOM decomposition is considered. Authors have expanded the decay rate of SOM (k parameter) to incorporate for the role of biophysical factors, mainly matric potential. This step is performed by a simple and testable exponential relationship between the decay rate and matric potential. The model is then expanded to include variations in oxygen and substrate diffusion as a function of matric potential and soil depth. The simple nature of proposed mathematical framework allows its application for large-scale carbon cycle and climate models while preserving the effects of some

[Printer-friendly version](#)

[Discussion paper](#)



of the key biophysical factors. This step is performed nicely in this model by reducing the number of calibration parameters and limiting them to some measurable quantities. The model is ultimately tested against a good amount of datasets.

Overall, the technical quality of the manuscript is high and the proposed model has potential to be used in other biogeochemical gas flux models to account for the role of water content and potential, individually. I have some minor comments and recommendations that I believe could help the manuscript to be stronger and accessible for broader audiences.

My main suggestion is to better discuss uncertainties and limitations associated with the previously developed models that the current model aims to address those limitations. At the moment, it is not completely clear how incorporating matric potential into the model improves the model predictions compared to the models without this feature.

While the idea of using SWC is nice, the implementation and formulation is rather confusing and hard to follow. The main problem might be the inadequate description of the parameters and the links of parameters through the equations.

I also found that the manuscript is a bit bulky in the introduction and method descriptions. I suggest shortening the introduction and methods. While some of the discussions and examples in the introduction and method are informative, I think it might be distracting. For instance examples and discussions on nitrification process could be misleading, since the main story is about respiration and the connection between respiration and nitrification processes is not immediately clear even though both could be aerobic processes. If this part is necessary, I would suggest to provide a discussion on its need.

My other suggestion is to better explain the difference between water content and matric potential, maybe in a schematic. For instance the independent relationship of water potential from water content and its effects on osmotic potential that is discussed in the manuscript is not so clear. This is an important motivation of the paper and could be illus-

[Printer-friendly version](#)[Discussion paper](#)

trated a little bit more. Meanwhile, the effects of osmotic potential are discussed in the introduction, but its incorporation in the model is not so clear, even though it has been assumed that Eq. 6 could also account for osmotic potential.

Minor comments: Page 1, Line 21: “are strongly correlated heterotrophic respiration rates” grammar error? References are not consistent. Some author names are capital and some are not. Page 2, line 3: “films is dependent” grammar? “Moisture sensitivity curve” is probably not accurate terminology. I suggest to define moisture sensitivity term. In page 4 line 18, “biophysical rates”. Here it is not clear what authors mean. In Eq. 6 and 7 different k parameters are used. I would suggest to better define these parameters. The current version is a bit confusing. Section 2.2 “SOM dynamics modeling” is very long that makes it hard to read and follow the method. I suggest breaking down this section into subsections with detailed subheadings. In line 6, page 8, I think the difference between gas and liquid diffusion coefficients of oxygen is about 4 orders of magnitude, I suggest checking the number, once more. Figure A1 is unclear. At the moment, it is unclear what dashed lines mean. PWP and FC could be defined in the caption of the figure. “Bioavailable SOC” should be defined. The term has not been defined and discussed in the rest of the manuscript.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2018-265>, 2018.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

