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

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Major Comments

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

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Discussion paper

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 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 again 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.

Comment 1: 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.

> Several changes that were in response to the other reviewers' comments will also address this issue. We have clarified what the scope and limitation of the proposed model are (see in particular Comment 7 of Reviewer #1 and Comments 1 and 2 of Reviewer #2). We added a new paragraph and figure added in the end to illustrate how moisture sensitivity curves vary by soil textural class (SWC parameters for the textural class averages were derived from ROSETTA pedotransfer function).

Comment 2: 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.

> There were some typographic errors in the main moisture sensitivity equa-

tion (Eq 12) that may have contributed to this lack of clarity. SWC contributes to moisture sensitivity in three ways: effect of water potential, effect of oxygen concentration, and effect of aqueous diffusion. The corrected Eq 12 now clearly shows this combined effect as a product of the three contributions (Eqs 7, 9, and 10, respectively) as explained by Eq. 6.

Comment 3: 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 destructing. 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.

We agree that the introduction and methods are longer than typical. The current version of the manuscript evolved in response to feedbacks we received after presentations at AGU, EGU and other smaller venues. Because the main thesis of this research falls at the intersection soil biogeochemistry and soil physics, lack of adequate familiarity of concepts on both sides appeared to have been a roadblock in effectively communicating the main message. The discussion around nitrification was needed because Stark and Firestone (1995)–one of the key papers that we relied for developing the water-potential dependence–used activity of nitrifying bacteria as a model system. We added additional statement to clarify this: "They used nitrifying (ammonium oxidizing) bacteria as a model system, in which nitrification rate was considered as a surrogate for microbial activity."

Comment 4: 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 important motivation of the paper and could be illustrated 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.

We added clarifying sentences and phrases in the introduction and the methods sections to this effect.

Minor comments:

1. 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. **[We added the missing preposition 'with'. The citation database was updated so that all names are capitalized consistently.]**

2. Page 2, line 3: "films is dependent" grammar? "Moisture sensitivity curve" is probably not accurate terminology. I suggest to define moisture sensitivity term. **[The incorrect verb was fixed. The term 'moisture sensitivity' curve has been used by others as well (e.g., Lawrence, C. R., Neff, J. C. and Schimel, J. P.: Does adding microbial mechanisms of decomposition improve soil organic matter models? A comparison of four models using data from a pulsed rewetting experiment, Soil Biol Biochem Soil Biol Biochem, 41(9), 1923–1934, 2009).**

3. In page 4 line 18, "biophysical rates". Here it is not clear what authors mean. **[Corrected as "biophysical factors"]**.

4. 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. **[More explanations given as suggested]**.

5. 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. **[Subheadings were added as suggested].**

6. 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. **[Corrected as suggested]**.

7. 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. **[We added “The dashed-lines of the Franzluebbers soils denote compressed samples.” Also we defined PWP and FC]**

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