

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

Teamrat A. Ghezzehei et al.

taghezzehei@ucmerced.edu

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Major Comments

Comment 1: Page 11, line 4. The authors state that the model of equation 5 can be solved under arbitrary fluctuations of soil water status, i.e. $\theta(t)$ and $\psi(t)$. However, this is not possible to do in close form unless you have a very specific function that shows how θ and ψ change over time; and if you have these functions, it is very unlikely that you will obtain an analytical solution. I would say that this assumption is wrongly stated here, and the authors should acknowledge that the analytical expressions they provide only apply for constant soil water status. Later on page 16, lines 16-17, the authors correctly point out that only numerical solutions are possible for the time-dependent

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case. This is obviously contradictory to what is stated on page 11.

Response 1: We agree that a complete closed-form solution does not exist for arbitrary fluctuation. We left the integral as is in Eq 11 for this reason. We restated the above sentence as: “The SOM dynamics under arbitrary fluctuation of soil water status (i.e., $\theta(t)$ and $\psi(t)$) can be described by rearranging Eq. (5), subject to initial active pool of SOC $C(t = 0) = C_0$, as...”. Also we added the following sentence right after the equation. “Note that closed form solution for the integral in Eq. 11 exists only at steady water content and water potential status...”

Comment 2: The upper limit of integration in equation 11 is with respect to time, but $K(\theta, \psi)$ is not time-dependent as expressed in equation 12. It seems to me that you may want to integrate over θ or ψ , but not t.

Response 2: The dependence of water content and matric potential was stated in line 4 right above Eq 11, therefore it was implied in equations 11 and 12. We now explicitly show this dependence in Eq 11:

$$C(t) = C_0 \exp \left(-\kappa_o \int_0^t K[\theta(t), \psi(t)] d\tau \right)$$

Equation 12 is an expression of instantaneous moisture sensitivity, therefore it is not necessary to express the dependence on time.

Comment 3: The solution of equation 5 is $C(t) = C_0 \exp(-kt)$. I assume that your intention is to be able to replace equations 7 to 10 for k as expressed in equation 6. If so, then equation 11 is missing a minus sign and t.

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Thank you for pointing out this error. The missing negative sign to Eq 11 was added (see above correction).

Comment 4: Why do you need C_0 in equation 12? I cannot trace it back from the previous equations. Also, what happened to λ ? Shouldn't it go here?

These were typographic errors. The effects of matric potential and accessibility (Eqs. 7 and 10) were inadvertently left out in Eq. 12, but have now been added. These typographic errors in the manuscript were not carried over to the codes used for calculations. The corrected Eq 12 is:

$$K(\theta, \psi) = e^{\lambda\psi} \left\{ \kappa_{a,\min} + (1 - \kappa_{a,\min}) \left(\frac{\phi - \theta}{\phi} \right)^{1/2} \right\} \left(\frac{\theta}{\phi} \right)^{1/2}$$

Comment 5: Equation 14 doesn't seem right to me. What you probably want is to compute the integral of the respired carbon, i.e. $C_{CO_2} = \int_0^t R(t)dt = \int_0^t 1 - C(t)dt$.

We believe the integral in the suggested expression is redundant as $C(t)$ refers to the amount of SOC remaining at any given time (see Eq 11). Therefore, the respired C must be the difference between the initial and remaining C levels; i.e.

$$C_0 - C(t) = C_0 - C_0 \exp \left(\kappa_o \int_0^t K(\tau) d\tau \right) = C_0 \left\{ 1 - \exp \left(\kappa_o \int_0^t K(\tau) d\tau \right) \right\}$$

No change was made in response to this comment.

Comment 6: Another limitation I see in this study is the lack of contrast with a related model that may perform poorly with respect to the newly proposed model. To my
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knowledge, the only model that can also deal with these multiple limitations is the DAMM model of Davidson et al. (2014). It would be very helpful if the new model is contrasted against DAMM or other model to more explicitly see the advantage of the new method.

Although comparison with another model, such as DAMM, would provide interesting results the comparison would not address the key tenet of this manuscript—to accurately represent the role of soil structure as described by water retention characteristic. A more appropriate test for this model is to compare model performance against experiments in which the soil structure is manipulated such that contrasting water retention characteristics would be achieved for the same soil. Then comparing our model with other models that utilize only water content or matric potential would be meaningful. We hope that publishing this modeling framework would motivate researchers who may have data needed for such comparison to test the our hypothesis. No change was made in response to this comment.

Comment 7: It is my impression that this model requires the availability of water retention curves for its use. This obviously implies an extra effort in terms of data collection. Can the authors elaborate more on this potential limitation of the method?

Yes, this is a limitation. It was made even more clear to us by the availability of only a handful datasets that we could use for testing our model, despite the fact that decomposition experiments at varying moisture statuses have been done numerous times. When WRC data is not available, a practical solution is to use pedo-transfer functions to determine the parameters of WRC. The following statement was added to the last section of the manuscript:

“Application of the proposed model requires availability of water retention characteristic, which may pose practical limitation in cases when water retention data cannot be readily acquired. Availability of only a handful datasets that we could use for testing the proposed model, despite the fact that decomposition experiments at varying moisture statuses have been done numerous times, is a clear evidence of this challenge. As a stopgap measure, it is possible to use pedotransfer functions to infer water retention parameters based on routinely measured soil characteristics such as texture, bulk density and organic matter content (Vereecken et al, 1989; Schaap et al, 2011; Van Looy et al, 2017).”

Minor comments

1. Page 6, lines 9-10. Which one is eq. 2 and 3 in fig. 1, i.e. red or blue?

We corrected it as: “In Fig 1, Eq (2) and (3) are illustrated by the solid blue line.” We also added the following sentence after Eq 4: “In Fig 1, Eq (4) is illustrated by the solid red line. The corresponding bi-modal pore size density function is shown as red-shaded curve.”

2. Page 6, line 16. Remove point.

Corrected

3. Equation 7. Here it may be good to remind the reader that matric potential is negative, and therefore k can't be higher than 1.

We added: “Note that $\kappa_{\psi} \leq 1$ because matric potential cannot be positive ($\psi \leq 0$).”

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4. Page 14, line 8. Can you provide a justification or a reference for this choice of parameter value?

The parameter denotes availability of oxygen under saturated moisture condition and ranges between 0 and 1. A value of 0 means complete lack of O_2 and a value of 1 means maximum O_2 concentration. For lab incubation samples, this value is expected to be dependent on sample size. In field conditions, soil depth is the most important factor that controls a . At this stage the parameter remains the most uncertain part of the proposed model and needs specific experiments to test and parameterize its value. For this paper, we chose the value of $\kappa_{a, \min} = 0.2$ based on the work of Ebrahimi and Or (Global Change Biology, 2016), which corresponds to the dissolved [O_2] at moisture saturation of ≈ 0.9 .

5. Fig 7. What is the difference between the red and the black lines?

The red lines were inadvertently left. They represent a different approach that we tested earlier in the study. They have been removed.

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