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Discussions

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

Comment 1: As acknowledged by the authors, the use of combined gas and aqueous diffusion limiting functions to predict respiration-soil water relations had been proposed by Skopp et al. (1990) and used in many occasions later. The matric potential-dependent function capturing reductions in microbial activity is a more novel addition, but similar functions have been recently proposed and used to capture respiration-soil water trends observed in laboratory studies (Yan et al. 2016; Manzoni et al. 2016). It might also be worth looking at other recent papers (some not available at the time this contribution was submitted) using a comparable approach, though with equations

derived in different ways (Tang and Riley, 2013; Yan et al. 2018; Moyano et al. 2018). Considering these previous papers, some statements in the Discussion and Conclusions section seem to overstate the novelty of this contribution (P18, L4-5; P19, L9).

> Thank you for directing us to the recent sources. We agree with the reviewer about some of the similarities with these sources and made changes accordingly. We added a sentence acknowledging that the diffusion limitation on substrate accessibility that we adopted in Eq. (10) is consistent with prior models (Tang and Riley, 2013; Yan et al. 2016; Manzoni et al. 2016). In the recent paper of Yan et al (2018), the effect of soil texture is captured by the empirical parameters that were fitted to the three soils. It is possible that the effect of SWC is implicitly contained within these tuned parameters as well. Our model was designed to directly address the physical effects of pore size distribution (as described by water retention curve). Other factors that are likely to depend on moisture status including enzyme activity and microbial community structure were not included. This was done to limit the number of tunable parameters and test to what extent water retention characteristic alone can explain moisture sensitivity. The complete moisture sensitivity function is given in Eq. 12 (typographic errors noted by this and the first reviewer have been corrected).
>
> $$K(\theta,\psi) = e^{\lambda\psi}\left\{\kappa_{a,\min} + (1-\kappa_{a,\min})\left(\frac{\phi-\theta}{\phi}\right)^{1/2}\right\}\left(\frac{\theta}{\phi}\right)^{1/2}$$
>
> The differences in moisture sensitivity amongst all the soils considered in this study are shown in Figure 5. These Figures are comparable in pattern to those of Yan et al (2018; their Figure 5). The major difference being, in our model the shape of these curves is dependent only on the SWC parameters and $\kappa_{a,\min}$. The latter was kept consistent across all soils for simplicity

and because the available data was not adequate to test how this parameter varies with depth and/or sample size. In testing our model, the shape of the dimensionless moisture sensitivity curve was prescribed *a priori* based on independently acquired SWC parameters and fixed value of $\kappa_{a,\min} = 0.2$. Thus, the main contribution of our work, which is also a major departure from the models of Yan et al. (2018), Moyano et al. (2018) and their predecessors, is the absence of moisture-sensitivity parameters that are tuned to match with respiration data. This does not negate the importance of moisture dependence of enzymatic and/or microbial activities represented in these other models. To emphasize the above new contribution of this work, we added a new paragraph and a new figure in the discussion section showing the moisture sensitivity curves of the 12 US textural classes. SWC parameters for these soils were derived from Schaap et al (2001).

Comment 2a: The model description is not always clear and there are several inconsistencies in the way parameters are defined. For example, in Eq. 10, the aqueous diffusivity D_W does not have the dimensions of a diffusivity $(L2/T)$, but is non-dimensional. The symbol C_A in the same equation is not used elsewhere. In Eq. 11-13, which are used to fit the data, C_A does not appear, so 'accessibility' does not play a role, unless C_0 is interpreted as the 'accessible' organic carbon (but that is defined as 'initial active carbon'). Moreover, the units in Eq. 11-12 do not match up: with K defined as in Eq. 12, the exponent in Eq. 11 is not non-dimensional, but has the same units of C_0. Towards the end of the manuscript, a "curve lambda" is mentioned (P19, L3), but lambda is only used as a parameter before. Overall, these issues make the reading and interpretation of results difficult.

As stated in the sentence preceding Eq. 10 accessibility "scales with the *relative* aqueous diffusivity", which implies that it is normalized by diffusivity of saturated soil. The expression in Eq. 10 is that of tortuosity, which by definition is dimensionless. For clarity the sentences above Eq. 10 were revised as follows:

"We assume the fraction of active SOC pool that is accessible to decomposers scales with relative aqueous diffusivity. Therefore, the accessible fraction of the SOC pool is proportional to the liquid phase tortuosity. Here, we use the Bruggeman expression for tortuosity,"

Eq. 12 had typographic errors that caused the confusion raised. The effects of matric potential and accessibility (Eqs. 7 and 10) were inadvertently left out in Eq. 12, but have now been added (see also response to Reviewer #1). In addition, the equation represented a closed-form solution of the right-hand-side of Eq 11 for constant ψ and θ. These was typographic errors in the manuscript but we verified that the codes we used for calculations were correct. The corrected Eq 12 is given above.

Comment 3: Some choices of the soil moisture characteristic curves appear arbitrary. How were unimodal vs. bimodal curves selected? At the dry end of the soil moisture characteristic curves in Fig. 4, for example, there appear to be a sharp decrease in water content – possibly a sign that a bimodal curve could work better? I would suggest selecting curves using a more objective criterion based on goodness of fit and robustness (e.g., AIC).

Multimodality of soil water retention curve can arise due to clear distinction between capillary and adsorptive forces. A fairly recent water retention model by Peters (2013) and Iden and Durner (2013) (now known as PetersâĂŘDurnerâĂŘIden (PDI) model) suggests that most soils should exhibit bimodality as the drying curve of adsorbed water usually exhibits different pattern from that of water held by capillary forces. The transition from capillary dominated retention to adsorption dominated retention occurs at very low matric potential levels ($< -100kPa$). But bimodality can also arise due to structure (e.g., aggregation or biopores) (Durner, 1994) that results in two (or more) distinct populations of pore sizes. The transition between macro-pore dominated retention and micro-pore dominated retention usually occurs at high matric potential. In this paper, the bimodal models were strictly used for soils that exhibit structure related bimodality. An alternative approach would have been to include adsorptive component to all the soils. This would mean that soils that also show additional structural effect need to be fitted with a trimodal model. None of the water retention data that we used have sufficient number of measurements to match the additional degrees of freedom that would be introduced by such model. Therefore we chose to use the classical van Genuchten unimodal model for all soils that exhibit bimodality at the dry end

Minor comments - Please check the whole text for grammar mistakes and inconsistent formatting of citations (e.g., author names in capital, erroneous use of brackets); some of these issues are highlighted below

1. P1, L17: “comparing” **[Fixed]**
2. P1, L22: “Yuste” **[Fixed]**
3. P3, L6: “nitrification rate. . . correlates” **[Fixed]**
4. P6, L1: if alpha refers to matric potential at maximum drainage, I am not sure I understand why D_0 (a function of alpha) refers to the modal rather than maximum pore throat diameter
    (a) This has been clarified as follows: “...is a parameter that indicates the matric potential at which the water retention curve exhibits the steepest slope”. The steepest slope of the curve implies that when a soil is subjected to progressively decreasing matric potential, the largest amount of water will be extracted at $\psi = -\alpha^{-1}$. This also implies that the corresponding pore size D_0 is the most common.

5. P6, L9: “top axis of the figure”, which figure? I would refer to the figure number **[Fixed. Also the sentence was moved down so that it comes after Fig 1 was properly introduced]**
6. P6, L15: “unimodal” P6, L16: extra full stop? This sentence appears incomplete **[Fixed. The latter senetnce was fixed as: “SWC of soils that exhibit bimodal pore size distribution can be described by sums of two van Genuchten curves (Durner, 1994):”]**
7. P7, L12: check use of brackets - “Chowdhury et al. (2011b)” **[Fixed]**
8. P7, L16: “Watson” **[Fixed]**
9. P8, L4: this sentence appears incomplete **[Fixed as “But rather, its effect on SOM decomposition rate (dC/dt) is accounted for through its impact on the accessibility of SOC (Davidson et al., 2012). ”]**
10. P11, L17: “important to note” **[Fixed]**
11. P14, L8: but in Figure 5, $k_{a,min} = 0.8$ as well P15, L4: what does “explained in its entirety” mean? Based on which performance metric? **[The statement in P14, L8 was corrected and now indicated the two values tested in the reported results. The sentence in P15, L4 refers to how the model works. It is not a general statement about moisture sensitivity. In the framework of the proposed model, there are no factors other those explained by the shape of SWC that can explain moisture sensitivity. The sentence was rephrased for calrity as “In the proposed model, sensitivity of SOM decomposition to soil**

moisture dynamics is explained in its entirety by the SWC, which directly dictates air content, water content and matric potential. "]

12. P15, L17: "soils that were. . ." **[The current phrasing is correct, see emphasized words here:"...individual *samples* of the same soil that *were* incubated at different levels...". No change was made.]**

13. P16: to avoid having incubation duration as a confounding factor, only the first data points from the Arnold et al. (2015) study could be used **[That would work if we were only looking for the optimal decomposition rate. But in this model, we also need to know the available SOC pool. Moreover, having multiple measurements over time increases the statistical robustness of the fitted parameter. No change was made.]**

14. P16, L21: more than inter-sample differences, the data from Miller et al. (2005) show strong Birch effect (Birch 1958) – longer dry periods trigger larger respiration pulses. This effect, which is widespread, cannot be captured by the proposed model. **[It is correct that the model does not account for wetting history. In the first wetting cycle, there should not be any difference of wetting-history between the 4-week and 2-week treatments. But, if you look closely at the data it clear that the 2-week rate is consistently lower than the 4-week rate. This can only be attributed to inter-sample differences. We provided two versions of models in which ignored or considered this difference. In both cases the Birch effect was not captured by the model. We added one statement to highlight this fact.]**

15. P17, L15: delete "in the" **[Fixed]**

16. P17-18: the structure of the Discussion and Conclusion section is a bit strange, with two introductory paragraphs and a single numbered subsection **[the headed subsection was unnecessary and is now removed.]**

17. P26, last line of the caption: "diameter" **[Fixed]**

18. Figure 2: check if labels (B) and (C) are correctly placed; the caption is not consistent with the figure and does not explain what panel (d) shows **[Fixed]**

19. P30, caption: no explanation of the difference between top and bottom panel is provided **[Explanation added.]**

20. Figure 6: check panel labels – now only (W), (I), and (D) appear as labels **[Explanation added.]**

21. Figure 7: not clear what is the difference between red and black curves **[The red curves were effective saturation curves (on secondary axes) that we plotted fo diagnostic purposes and were mean to be commented out in the code. They are now removed.]**

22. Figure A3: "bulk density" **[Fixed]**

23. Figure A4: what are the numbers in brackets? Is the number of significant digits reasonable? **[These are matric potential values predicted by pedotransfer function. The numbers are now reformatted.]**

References provided by Reviewer #2

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2. Iden, S. C., and W. Durner (2014), Comment on “Simple consistent models for water retention and hydraulic conductivity in the complete moisture range” by A. Peters, Water Resour. Res., 50, 7530–7534.

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