SOIL Discuss., author comment AC2
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Reply on RC2

Fang Yu et al.

Author comment on "Prediction of the vertical scaling of soil organic carbon in temperate forest soils using percolation theory" by Fang Yu et al., SOIL Discuss., https://doi.org/10.5194/soil-2021-84-AC2, 2022

Dear reviewer,

Thanks for the comments. In response, we will make several changes according to the suggestions and criticisms. Our point-by-point response to the reviewers' comments is given below. In most points we agree with the reviewer but not for all.

1) L48: “coarse”.

This will be addressed. We apologized for the typo.

2) L49: Please enter the citation of “Trumbore et al., 2006” in references.

This will be addressed.

3) L50: As far as I read, neither Gill nor Joslin study the total SOC in the subsurface layer of soil.

We presented incorrectly here. What we attempted to present here is that studies from Gill and Joslin suggest that higher root litter inputs generally occur in the upper soil layers.

4) L53-57: I'm not as optimistic as the authors about the vital importance of C leaching as a potential driver for the redistribution of C in the soil. For example, Jobbagy and Jackson (2000, p. 433) consider the decrease of SOC turnover with depth a much more plausible explanation (in fact, they find an inverse trend between precipitation and SOC depth). I would recommend that the authors consider other possible explanations in the manuscript (see also comment to L221 – 224).

Root growth is limited by nutrients and water, there are evidences that root growth also follows the percolation theoretical scaling (Figure below, referenced from Fig.5 in Hunt, 2016). The turnover of SOC affected by microbe activities is certainly a factor, and we agree with the reviewer that it should be considered in the manuscript.

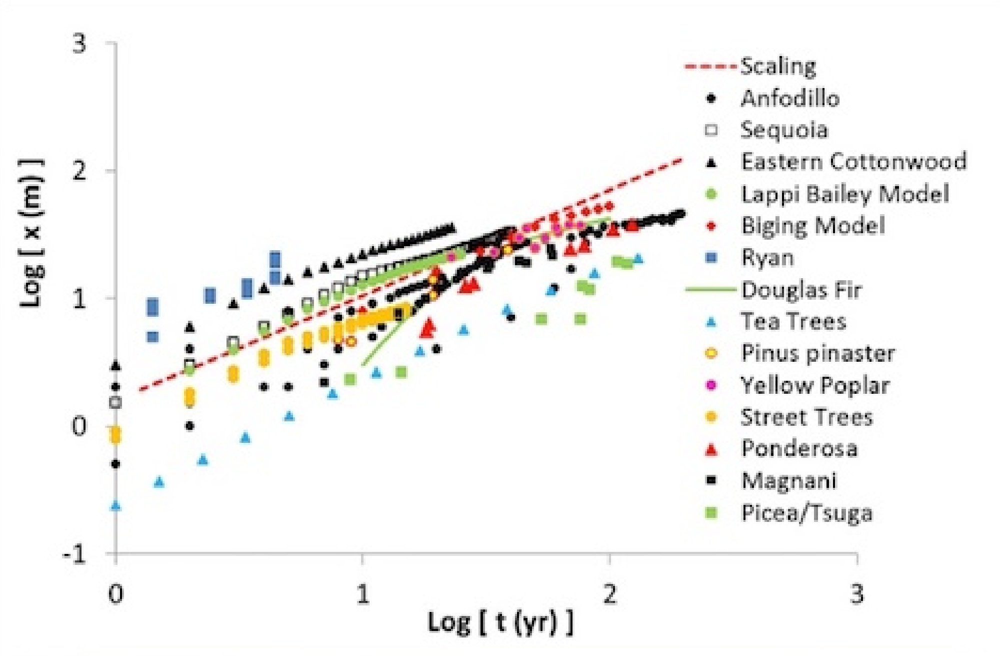


Fig. 5. Comparison of Eq. [5] with the growth curves of a number of species of trees.

Reference: Hunt (2016). Spatio-temporal scaling of vegetation growth and soil formation from percolation theory. Vadose zone journal VZJ.

5) L81: Doesn't the percolation theory also depend on soil texture? The physical process of water infiltration is highly dependent on it (eg. hydraulic saturated conductivity).

Correct. The more useful form of describing solute transport in soil is $(t/t_0)(x/x_0)^{Db}$, *where* x_0 *is the pore size, which depends on the soil texture, and* t_0 *is the time it takes for solute to pass a single pore, which equals to* x_0/v_0, *and* v_0 *is the infiltration rate. That means that water infiltrates soils with different textures (with same saturation condition) will show parallel lines on the log-log plot (time versus distance) but with different intercepts. Here in the proposed paper, we demonstrates the power-law form of SOC distribution along soil profile, and the determination of the intercept still needs more investigation.*

6) L82. The study of Sheppard et al. does not seem refer to the transport of SOC in temperate forest. Please, modify the sentence or move the reference elsewhere.

Good point. We referenced the value of D_b *in 3D saturated conditions from Sheppard et al. But the original presentation was confusing. This will be addressed.*

7) Table 2: Please, indicate here the soil type (WRD or Soil Survey Staff classifications). This could be interesting to understand the vertical distribution of SOC.

The main soil types are sandy loam and loamy clay in the region.

8) Table 2: Are these temperature the annual average temperatures? I find these (< 5ºC) too much low for a broad-leaved deciduous forest. Please, indicate the Koppen climate classification.

Correct. Temperature showed in Table 2 is the annual average values. All study sites are temperate continental climate.

9) L122: Why was the C horizon not sampled?

There have been conflicts of the definition of "soil depth".

From previous work applying percolation theory to soil formation, "soil depth" refers to a particular weathering horizon like the Bw (e.g. Hunt, 2015). Here, to apply the same theoretical framework, we follow the same criteria.

Reference: Hunt, A. G.: Soil depth and soil production. Complexity, doi: 10.1002/cplx.21664, 2015a.

10) Figure 1b: I think the point "TS-P1" is really CS-P1.

The main point here in Figure 1b is to present effects of precipitation on SOC content. CS-P1 and LX-P1 have similar temperatures and slopes, but different precipitations. TS-P1 is not appropriate to present here because except for different temperatures, the sampling slopes are not close neither.

11) L163 – 169 and Figure 2: The effect of comparing a 12% slope (CS-P) vs. 17% (LX-P2) does not seem very convincing… In my opinion, it is very difficult to assess the relative influence of both factors (slope and precipitation) on the SOC.

The sampling slope is in "degree". We attempted to explain the effect of topography on SOC content, and more precipitation and lower sampling slope seemed favor the conservation of water, which has a positive feedback on SOC production. When comparing JL-P1 and HG-P1, more precipitation in JL-P1 didn't result in higher SOC since the sampling slope is steeper.

12) Table 4: It would be useful to show the mean SOC value for the first meter.

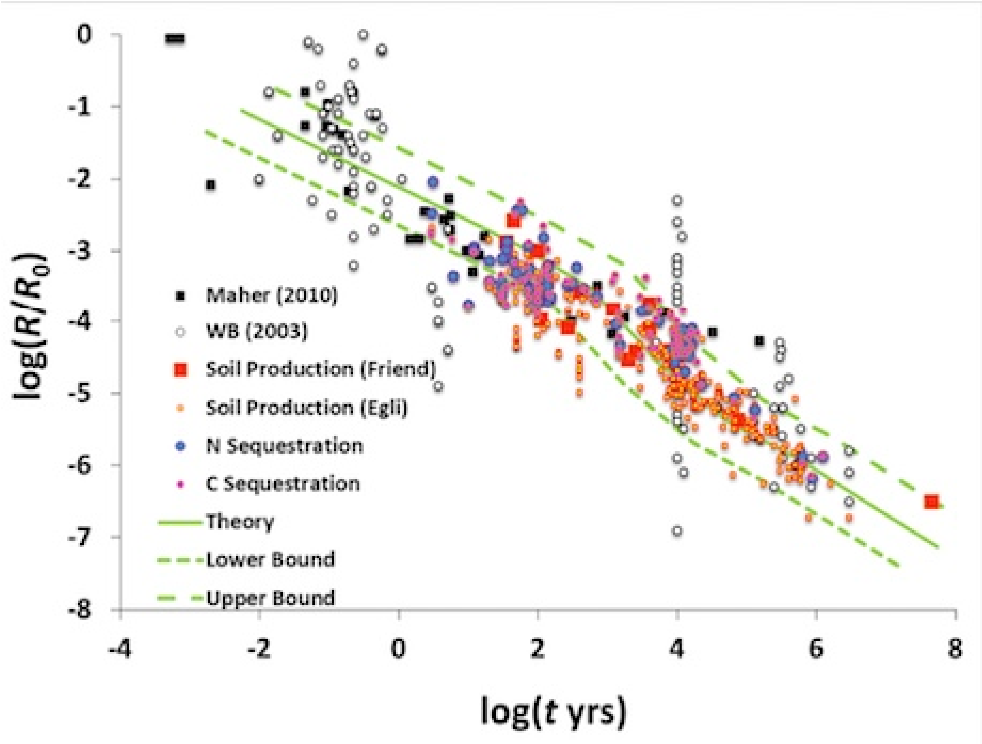
This will be addressed

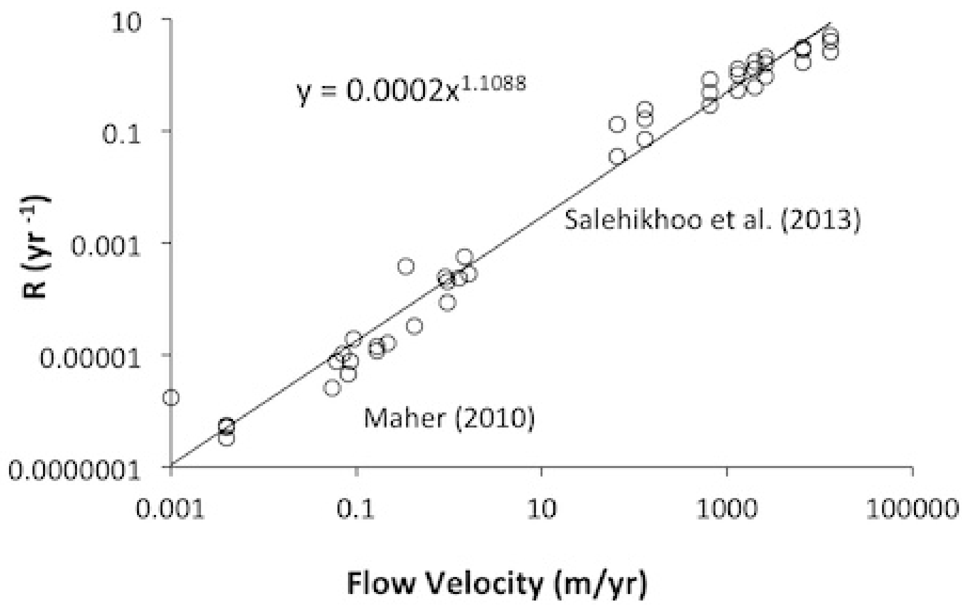
13) L195: "temperate"

This will be addressed. We apologized for the typo.

14) L221 – 224. It is true that the empirical data fit well as predicted by the percolation theory. However, I'm not clear about the relationship between this theory (beyond its name, it is a statistical theory that can be applied to many non-hydraulic phenomena, as traffic jams!) and the soil infiltration process. Could it not be also explained by other "power-law" soil processes such as roots decomposition rate or microbial activity? (see, eg., the vertical distribution of Soil Microbial Biomass Carbon showed by Sun et al., 2020). In my opinion, it would be needed to provide some additional evidence linking percolation theory with the physical process of water infiltration in soils.

The basic theoretical form of solute transport described by percolation theory is x is propotional to t^{D_b}*, where x is the transport distance, t is the travel time, and* D_b *is the fractional dimensionality of percolation backbone, which is a known value that only dependents on dimensionality and the saturation condition. First figure below (from Figure f, Hunt et al., 2020) show the fitness of theoretical scaling of percolation theory and chemical weathering rate data observed either from the field or from laboratory, though it is not the direct comparison of water velocity and the theory, chemical weathering are water limited in subsurface, and it is proportional to flow velocity (as shown in the second figure below(referenced from Figure1 in the same paper)) . We think it is convinced evidence that percolation theory can describe solute transport in soil.*





Reference: Hunt, Faybishenko, Ghanbarian, Egli, & Yu. (2020). Predicting water cycle characteristics from percolation theory and observational data. International Journal of Environmental Research and Public Health, 17(3), 734.