

SOIL Discuss., referee comment RC1  
<https://doi.org/10.5194/soil-2021-84-RC1>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on soil-2021-84

Anonymous Referee #1

---

Referee 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-RC1>, 2021

---

The authors of this paper attempted to predict the distribution of soil organic carbon (SOC) in temperate forest soil profiles using percolation theory, a theory commonly used to describe material transport in disordered porous media. Soil is an inhomogeneous medium composed of complex components. From the perspective of medium, it is feasible to apply this theory. However, SOC is not a single substance and has very complex forms: particulate, dissolved and adsorbed. Therefore, if the authors want to achieve the goal in the title, they should study SOC in different forms separately. In this paper, the soil profile of some typical forestlands in Changbai Mountain was measured by stratified soil sampling, and SOC was studied as a solute index. The study design is unreasonable. The measurement index is simple and single, and the workload is insufficient. The core viewpoint of the study is seriously inconsistent with the vertical distribution mechanism of SOC on profiles. Therefore, it is recommended not to be published. Specific comments are as follows:

1) The vertical distribution of soil organic carbon (SOC) in forest ecosystems has two meanings: one is vertical distribution along elevation and the other is vertical distribution in a profile. In order to avoid ambiguity, I suggested that the author emphasize vertical distribution **on profile** in the title and article (L1, 8...).

2) L26-32 talks about the influence of vertical factors on soil organic carbon. Vertical factors here refer to the influence of altitude rather than the vertical depth of the profile studied in this paper. Should be deleted!

3) L76-80 mentioned that the vertical transport of SOC is mainly in the form of DOC leaching, so why not take DOC as the research index in this paper?

4) The full names of the six stand types of L100 should appear before giving abbreviations.

5) L115 there were only two replicates for each forest at the standard standscale, which was not enough for field experiments.

6) L124-125 soil samples were only screened through 10 mesh (1.7mm), while the standard method for determining SOC needed soils been through 100 mesh (0.15mm).

7) L130-137 This paper only selected the data of 5 published articles for fitting analysis, which could not constitute a reasonable meta-analysis.

8) L207-212 Here, spatial variability, environmental factors and other unrelated topics are discussed.

9) L215-216 There is no need to emphasis the dominant species in stands.

10) The goodness of fit of curves in the results of L219-220 is not high, and the tolerance range in Table 3 is very wide, so almost indicators of the forest soils reduced along the profile depth can appear this curve, such as fine root biomass and microbial biomass, which are not distributed according to the principle of percolation theory. Therefore, the method of this paper is not appropriate.

11) 226-227 The shallow soil was more disturbed by the external environment, while the SOC in the deep soil was mainly from the roots. The influence range of litter was generally considered to be in the Leaching - deposition (B) layer. The deeper soil has more adsorption sites, so the vertical transport of DOC in the soil is also affected by soil adsorption.

To sum up, the design of this study is unreasonable; the measurement index is single; and the workload is insufficient. It is suggested that the author increase the workload and select reasonable indicators such as DOC and mineral nitrogen for the study.