Overall comments:

I think the authors addressed my comments, and the manuscript was improved to some extent. I agreed it is important to have a more general model that can be applied to organic-rich mineral soil globally. In the new version, the authors do emphasize that the proposed model and its validation are mostly done for soils with low range of SOM values. I would think that it remains uncertain whether this model applies to soils with very high SOM content, particularly the validity of the wilting point estimation.

1. please update the statistics in the abstract to be consistent with the results

2. Introduction, P 2, Line 75: Are the authors trying to say that we need dynamic SOC maps, rather than statical maps available from current datasets? I agree that it is important to get global SOC estimates from satellite missions, especially in data-sparse regions where the currently statistical-based SOC maps may fail. Yet I am not sure whether SMAP data can be very helpful in this regard, esp considering its coarse resolution.

3. Eq. (11-12): wilting point is a critical parameter of the proposed dielectric model, which determines the ratio of boundwater to free water.

Are there any references to back up the new equation (i.e. Eq. 12)? Or the authors simply adjusted the original equation to avoid wilting point larger than porosity? Some references supported a rather low witling point of highly organic soils (e.g. Verry et al 2011, Physcial Properties of Organic Soils), so I remain doubtful about the validity of the new equation for peaty soils. I suggest the authors noted in the ms on the range of SOM levels that the equations may apply to.

4. Please note that a new SoilGrids250m dataset (v2.0) is now available (Poggio et al., Soils, 2021).

5. Appendix A: please relable the equation as A.X to avoid confusion. Also, some expressions are awkward. E.g. P8, Line 222: what does "back-scattering albedo" mean? There are still errors in the format (e.g. P8, Line 225). Please go through the paper carefully.