We thank the reviewer for providing valuable comment to improve our manuscript. Our response can be found in gray paragraphs.

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.

We also agreed that general model which can cover from mineral to peat soil is important to be developed. Unfortunately, in this study we could not present the results for the high OC cases because we focused on the development of the soil probe sensor for the 50MHz frequency and there are no dielectric measurements in this wavelength obtained from peat soil in the previous literature including Bircher's study.

As stated in the discussion section, we will improve this model as a frequency dependent model to compare the simulated with the measured dielectric constants obtained from very high organic or peat soils such as Bircher's study. However, the main idea of the current study will be consistently applied in the future study that increases OC, increases bound water fraction, and consequently decreases dielectric constant regardless of the frequency difference of the soil probe.

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

Thank you very much for pointing out this error. We updated the validation statistics (p. 1, lines 28-29).

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.

We believe that SMAP might play very important role in obtaining the spatially and temporally varying SOC map which has not been obtained before from satellite measurements. The reason for this belief is that regardless of the coarse resolution of SMAP measurements it can provide an unprecedented and unique benefit to estimate SOC for following three reasons.

1) Microwave can only detect SOC underneath vegetation which other shorter wave sensors cannot perceive.

2) The temporal dynamic of OC evolution obtained even from low-resolution satellite image will be helpful in various modeling and observation studies.

3) The limitation of the low-resolution issue can be overcome by recent downscaling approaches, machine learning methods to make a synergy with other ground, spaceborne and satellite data.

We added above description in P. 2-3, lines 76-84.

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, Physical 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.

There are various backup studies (Saxton, Chadburn, Schaap) to show that greater SOM increases the wilting point and porosity as our proposed equations. This relationship between the organic matter and the soil parameters might become more complex in organic rich soil if the type of OC is also important and the dominancy for the OC type change in high SOC region. In this regard, this complex relationship considering sapric, hemic and fibric as referred by the reviewer ((e.g. Verry et al 2011, Physical Properties of Organic Soils)) should be defined in the future. We added this explanation in p. 6, lines 196-202.

As the reviewer's comments, we added the range of OC for the validation (1% - 15 %) and the sensitive experiment (1% - 30% SOM) for the clarification of the future users for our approach in p.18, lines 458-460.

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

Thank you very much for the relevant reference. We added this in p. 9, line 251.

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.

We added more detail description for "back-scattering albedo" the with the reference (p.9, lines 231-232)
Also, we fixed them as the reviewer pointed out (p. 19, lines 496-504).

Also, we use the terminology "organic soil" or "peat soil" in the comparison to other models as mentioned their references. But we emphasized that our approach is based on "organic-rich mineral soil" because of relatively low OC amount used in our validation (p.1, line 18 ; p.5, line 181; p.13, 342; p.18, lines 463-464)