We thank the reviewer for her/his comments. We will include your suggestions in the revised version of the manuscript.

The quantile approach relies on a strong assumption that SOC will be same under similar soil forming factors. If SOC values are different, it much be induced by management. But the problem is that we would never find two soils with the same forming factors. Numerous factors (e.g., climate seasonality) regulate SOC dynamics and thus SOC stock at a typical site. At the same site, SOC would also experience temporal changes. In this study, only very few potential predictor variables of SOC were considered. Other variables such as soil parent material, land use history, climate variability are not included.

This is a regression model that uses global information to represent soil forming factors in a continuous scale. Of course, given that we are dealing with real numbers, it is very unlikely that two locations have *exactly* the same values for all the factors but a regression model does not rely on that.

The process related to carbon sequestration, stabilisation, etc, is quite complex and a global model will always be underspecified. Our model is purposely simple for a simple reason. By just adding a few extra covariates, the model becomes "ill-defined" since we do not have enough data to cover the covariate space. For instance, land use history is very important but most samples sites have a single observation in time.

That said, the covariates used do explain a large part of global SOC variation. We will expand the "Data sources and preparation" section and add some references to clarify that.

More importantly, the approach adopts another strong assumption of steady state. If the soil is not at the steady state, the approach will be invalid, because a mediate soil (50% quantile) may experience SOC loss. The SOC would be an overestimation of the real 50% quantile, and vise versa.

This model is static, so the reviewer is right. We do assume a more or less "steady state" for croplands, which would be the assumption of any static model. However, this is probably a good approximation for two reasons. First, given the nature of our soil database, which comes from surveys not purposely designed to monitor SOC changes in time, soil samples targeting agricultural land are more likely located in well established farms. Second, part of the process of generating the landcover information used in this work (MODIS) has a spatio-temporal smoothing, which ensures that a landcover is more or less persistent (at least a few years).

Probably the worse case would be some samples taken after only 2-3 years under farming. However, the larger proportion of the carbon loss happens during that period, so the problem would be attenuated.

We are happy to expand the discussion to raise this important point.

The manuscript paid little attention to the potential uncertainties in the relevant estimations. Two major uncertainties I think should be explicitly tested are: the approach used to estimate BD and prediction uncertainty by the quantile CNN model. To my knowledge, BD has been reported for some soil profiles, why was BD estimated using a pedotransfer function? Could you please test the credibility of the BD estimates which are vital for estimating SOC stock?

In general, most pedotransfer function (PTF) are not universally applicable and the uncertainties vary in space. We think that an uncertainty analysis with that level of detail is beyond the scope of this paper. However, we agree with the reviewer that a general sense of the error induced by the PTF is useful. We do have BD records for part of the database and we could use to get a general estimate of the uncertainty. We will add that to the revised version of the manuscript.

It is important to note that this addition will not change the conclusion (that croplands have a limited capacity to offset carbon emissions and that the capacity changes with climate change predictions).

In page 3, a bootstrapping routine was mentioned. The reader cannot find anything descriptions on the purpose of this routine. To predict SOC stock?

We did that to generate multiple SOC stock maps (a map for each bootstrap iteration) and estimate a mean prediction. We will clarify that in the revised version. In addition, we will add a deviation estimate (standard deviation) to the reported mean values.

The author very briefly described future climate projections. As the SOC estimates were conducted at the global scale, I believe historical climate records are required to run the GCMs. How were the

climate projections used in their models for predicting SOC stocks?

We did not run the GCMs. As mentioned in Section 2.4, we used estimates from the WorldClim 2.1 database. We will expand that section to clarify that.