This manuscript represents an important contribution to the field of fine-resolution global digital soil mapping. The methodology and its application potential are commendable, and the work is generally of publishable quality. The paper can be significantly strengthened by addressing several key issues related to the methodological description, which currently lacks sufficient detail, and by improving the consistency of writing and terminology throughout the text.

Specific Comments:

Please add continuous line numbers throughout the manuscript. This will greatly facilitate referencing specific locations during the review and revision process.

RE: Thank you for the comment. The line numbers are generated based on the ESSD template / recommendations hence this is required by the journal and beyond our power.

Abstract needs better clarification. Please provide the abbreviation of SOC in P1 Line 5. It is not clear about the spatiotemporal Machine learning. How time is incorporated into this model? Is it a 3D+T model? Why using 68% probability for quantifying the prediction uncertainty? Soil carbon density (P1 Line 9) and soil carbon (P1 Line 11) should be corrected as soil organic carbon density and soil organic carbon due to the presence of soil inorganic carbon in many soil samples. It is reasonable that authors did not consider the temporal changes of soil texture fractions and soil types, while bulk density is highly correlated to soil organic carbon and therefore its temporal changes should be considered if you taking 5-year time intervals for soil organic carbon. Please provide the full names of RMSE and CCC for the first time. It is not necessary to indicate the RMSE in log-scale since this information is useless. It is not clear why authors only present the most important variables for soil organic carbon density and pH.

RE: We have added explanations of all abbreviations in the abstract (SOC, CCC, RMSE etc). Regarding "How time is incorporated into this model?" — time is incorporated using 3 important steps: (1) most of covariates we use are time-series of images (annual time-series) to represent ecosystem/climate dynamics / points are carefully selected to present different time-periods (2000–2024), (2) we overlay points and covariates using explicit time-reference (spacetime overlay), (3) we fit and evaluate model performance both using spatial, temporal and spatiotemporal cross-validation. But it is important to emphasize that we do NOT use time as a covariate (see P17L12–18), although we do use soil depth as a covariate.
Indeed we agree that Bulk density should be also mapped at 5–year intervals. This is on our list for the next update of predictions.
We prefer, however, to keep RMSE in both original and log-scale (both are relevant as in any Generalizer Linear Model-type modeling; log-scale RMSE is further used for simulations in Fig. 20 on P49). Regarding the statement "RMSE in log-scale since this information is useless", indeed RMSE in log-scale is abstract and difficult to interpret, however, for log-normal variables RMSE in original scale is often sensitive to high values (e.g. <1% very high values can double or triple RMSE), so that it becomes difficult if not impossible to compare performance of two models; log-scale RMSE allows comparing predictive performance of models where SOC training points come from either agricultural or forest soils (often 2–3 times higher SOC). Log-normal distribution is common in statistics and in order to simulate values following log-normal distribution one needs s.d./RMSE in log-scale (see e.g. https://stat.ethz.ch/R-manual/R-devel/library/stats/html/Lognormal.html). In summary: for log-normal / skewed distributions variables, both RMSE and log-RMSE are useful, as are both median and mean useful. It depends on the context. We added some explanation why we use log-scale RMSE in section "Cross-validation and quality control".
Regarding the question "Why use 68% probability for quantifying the prediction uncertainty?" our answer is: because this is the 1 standard deviation range (assuming a normal distribution; https://commons.wikimedia.org/wiki/File:Standard_deviation_diagram.svg#/media/File:Standard_deviation_diagram.svg) and hence the prediction range from the 68% probability can be directly compared with CV RMSE, which we find practical in this case. The QRRF method allows users to specify any arbitrary probability (usually some number between 68% and 99%) so there is no limitation in the sense of whether we could also derive 90%, 95% and/or 99% intervals.

P3 Lines 5-7: This work did not solve this issue during predictive modelling.

RE: That is correct, however, we do use annual bare surface coverage and tillage index as covariates, so there is certainly some representation of agricultural management practices in our list of covariates. See section “Preparation of covariate layers” P16L1–24 for more details.

P3 Lines 21-23: Authors overlooked the maps from Australia. It would be better to include the work by Grundy et al. (2015). Grundy M.J., Rossel, R.V., Searle, R.D., Wilson, P.L., Chen, C. and Gregory, L.J., 2015. Soil and landscape grid of Australia. Soil Research, 53(8), pp.835-844.

RE: We have now added mention of the work on P3L24.

P4 Line 29: This work covers the period from 2000 to 2022+, so how to evaluate the impact of land use conversion for SOC and pH on a scale of 25+ years?

RE: We are currently updating all predictions to 2000–2024 (this is in fact 25 years), hence we use the sign “2022+” (indicating 2022, 2023, 2024 etc). Our determination is to keep on updating these maps; making them open and enabling open development communities that can contribute their own ideas and data. We have now clearly mentioned this in the manuscript (see P56L29).

P5 Line 20: the difference between observations and measurements should be better clarified.

RE: We have added an additional text to try to clarify the difference between observations (e.g. observations of diagnostic horizons, root types, soil structure types etc) and measurements (strictly generated using in-situ sensors and/or laboratory machines) on P6L4.

P5 Line 21: Once you match the O&M with covariate layers by year, it means that you overlook the legacy effect from environmental covariates (e.g., land use change), which can be quite important for soil properties, such as soil organic carbon. I understand that this consideration would pose a heavy load for computing, but at least you should address this limitation in the discussion.

RE: That is a valid point. Indeed there is a cumulative effect of some management practices, not to mention that many soils are formed due to the past/historic events (glaciation, big floods and over-floods etc). We have added some discussion to emphasize these limitations on P46L24.

P5 Lines 25-26: It is not clear that whether authors included the profile issue in performance evaluation to avoid data leakage.

RE: In this work we consistently for any type of validation in this work take whole profiles out for either training or validation (as clearly stated in “Cross-validation and quality control” on P23L11–14). We are familiar with the fact that using training samples at the same profiles can lead to serious over-fitting (e.g. https://doi.org/10.1016/j.spasta.2015.04.001).

P7 Line 26: Please correct SOC as SOCD or SOCd. Indeed, the full name should be specified in P7 Line 22.

RE: We have added the full name as requested.

P8 Line 4: in t m-2?

RE: Correct. This was a typo. Thank you!

P10 Line 5: A recent released dataset from Chen et al. (2025) may be helpful for your future work. Chen, Z., Chen, L., Lu, R. et al. A national soil organic carbon density dataset (2010–2024) in China. Sci Data 12, 1480 (2025). https://doi.org/10.1038/s41597-025-05863-3

RE: Thank you for providing this information. We have downloaded the data set and will add it in the next update. Such data sets can significantly help increase accuracy of global models. It is fantastic that the authors have decided to share this data as open data via Zenodo; this can make a difference for any group doing global modeling & assessment of SOC. We also cite this article now on P48L12.

Figure 3: Please use either SOCD or SOCd in the manuscript.

RE: The abbreviation has been updated.

P15 Lines 28-30: What is the advantage for estimating SOC density directly from SOC content? SOC together with other variable control the variability of bulk density, only take SOC to estimate SOC will limit the prediction accuracy. SOC [kg m-3] should be corrected as SOC density [kg m-3].

RE: As explained on P15L27–32, we use SOC for gap-filling SOCd only for soils with low SOC i.e. not as a general solution for gap-filling missing SOCd values. Our research results indicate that correlation between SOC and SOCd for soils with <0.4% SOC is high (R-square exceeding 0.96; see Fig. 2c on P12), hence we consider that risks of over-/under-estimating SOCd are low.

P16 Lines 11-12: variable resolutions in 1 kilometer resolution? Scale is not an appropriate term here.

RE: Corrected (see P16L25–29).

P17 Line 15: Since it is a 3D+T model, it is important to demonstrate the time span of soil data to support spatiotemporal modelling.

RE: Time-span (density) of soil profile/samples is provided in Fig. 6c.

P24 Lines 14-25: Please also include R2 in the accuracy evaluation. Why not report the accuracy of silt content here?

RE: Thank you for the comment. Now added on P25L22.

Figure 11: It would be also interesting to demonstrate the difference of model performance across different continents, which would be helpful for the design of future direction.

RE: We have added the accuracy metrics per continent on P31 Table 2.

P53 Line 11: assessment indicates that (R2) the best achievable. R2 is a typo here?

RE: It is not a typo. R2 is connected with research objective R2. We have added extra text to avoid confusion so now it is "Research objective #2".