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Interactive comment on "Simulation of soil carbon dynamics in Australia under a framework that better connects spatially explicit data with ROTH C" *by* Juhwan Lee et al.

Anonymous Referee #1

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This manuscript presents a simulation work on soil C dynamics using the RothC model over Australian croplands and grasslands. This topic is within the scope of the journal.

The manuscript has a strong potential, as it uses a large and continental-scale set of plant and soil data for model parametrisation, simulation and prediction. However, the manuscript suffers important issues of orientation of study objective, modelling and redaction, rendering the nice dataset not well valorised.

In the manuscript, the proposed framework that allows bridging dataset and the model plays a central role in driving the study's storyline (see LN1-3 as the beginning of Abstract, LN54-64 as the key sentences for knowledge gap identification in Introduction

and a whole Section 4.2 related to the framework). Too much emphasizing the framework makes the manuscript very technical, rather than scientific. First, there are no alternative frameworks presented as a control for comparison, so the advantages and drawbacks of the framework cannot really be validated. Then, the novelty of such a framework is unconvincing. There are numerous studies on soil carbon modelling performed at regional, national or bigger levels in literature. In such a kind of study, gathering, synthesizing, processing and standardizing large-scale climate, plant and soil datasets from diverse sources are usually common and necessary steps in the modelling process. The flow chart in Figure 1, as well as the associated discussions does not seem to be particularly special or innovative to what has been routinely done in literature. Therefore, selling a framework makes the manuscript scientifically weak and structurally unbalanced.

In parallel, the nice dataset over the continental scale could have been used to address very appealing ecological/agricultural questions, such as impacts of land-use and grazing on the long-term soil C fates. The manuscript indeed presents some figures (Figures. 3-5) including these treatments, but there are no scientific questions driven behind and no associated knowledge gap could be found in Introduction. Nor were these effects fully discussed in the current version of Section 4.1 of Discussion, which is, again, fairly technical and, in most of time, centred to the model. As one of the most famous pool-based models, RothC itself and the associated modelling skills are well-documented. In my opinion, given the nice dataset, it would have been more original to focus on specific questions about land management than on the model or a "framework".

The model's initialization procedure (LN160-163 and LN169-171) needs to be clarified too. It is well-known that the settings of initial relative sizes of soil C pools have a huge impact on the final outcomes. If I understand, at time 0, the authors used site-dependent (presumably observed?) carbon quantity with relative pools sizes corresponding to those at their theoretical equilibrium condition provided by the model, right? It is not very clearly said in the text. Have the simulated C dynamics or changes ever been compared with those (presumably?) measured at the 73 sites from 1991 to 2000 (LN161)? This may be a good manner to check and validate the "equilibrium condition" hypothesis, which has been considered strong and untrue by increasing studies.

It is a very good idea to carry out an uncertainty analysis to test the impact of biomass DPM/RPM ratios on model results (LN166). However, the choice of biomass DPM/RPM ratio (LN184) is disputable. Despite some plasticity, a species' DPM/RPM ratio shall be quite stable depending on its taxonomical and functional features. For example, legumes which are richer in N and lower in C:N ratio shall have generally higher DPM/RPM ratios than grasses (e.g., rice, wheat. . .). But the authors' manner of choosing DPM/RPM ratio (". . . selected the DPM/RPM ratio based on the minimum deviation of TOC"; see LN 184) may risk picking unrealistic values for species. This is because the model's fit/bias is not only dependent on biomass DPM/RPM ratio, but also on the settings of relative soil C pool sizes at time 0 (whose influence on model fit is even much more important). Therefore, it would be more reasonable to choose species' DPM/RPM ratios according to the literature data on plant decomposition traits (even though they were not published in Australian contexts). An additional uncertainty analysis would always be appreciated to test the amplitude of impact of chosen DPM/RPM ratios with variance for a given setting of relative soil C pool sizes at time 0.

The removal of the 388 sites may need some more justifications. Why did these sites (not the others) yield such unrealistic values? Why 10 Mg C/ha as the threshold?

LN 209: Which model type did the authors choose for the test of the environmental factors? How do these factors cross or nest among each other? And how were they treated in the model? Additional information about this may be represented in Materials and Methods and Figure 6. When looking at the Figure 6, it is not surprising to see that Clay, MAT, MAP and PET stand out, as they are all directly or indirectly involved in the model as key inputs/parameters. What would be much more meaningful is to do the same test over the residuals (i.e., measured C changes minus modelled C changes

for the 73 sites 1991 to 2000). This helps see which environmental factors should be further taken into consideration by the model.

Overall, the manuscript tackles a timely and important question in the current research context. However, due to the unconvincing scientific orientation, unbalanced structure and ambiguous modelling procedures, I don't think the manuscript is mature enough to reach the journal's standard. I sincerely suggest the authors make good use of such a nice dataset and rework on the question and modelling processes.

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