Response to reviewer 2 (R2): 'Simulation of soil carbon dynamics in Australia under a framework that better connects spatially explicit data with ROTH C'

Authors: We thank R2 for taking the time to review our manuscript. Below we respond to each of the comments made.

R2: The authors developed soil C calculating system which connects spatial datasets on meteorology, soil, land use and land management with the RothC model. They calibrated the RothC and predicted changes in soil C for 100 years with different soil management scenarios. I think this work is within the scope of this journal and potentially many of audience of Journal would be interested in. This was my first impression after quick read of paper. But after careful reading, I found some severe problems. First, explanation of what the authors have done is not enough throughout the paper especially in "Materials and methods" section. Frequent disconnection in logic between sentences made me difficult to understand what the authors really have done. Development of calculation system is great achievement, but it was difficult to evaluate the validity of the many assumptions in developing the system and future simulation procedure. This might be partly because of English skill but I think not only due to that. Significant re-writing of manuscript with English check by native speaker will be needed for this manuscript.

Authors: We thank the reviewer for noting that our work is in the scope of the journal. We regret to hear that he/she could not fully understand what we did. It would have been useful if the reviewer pointed out exactly which part(s) of the manuscript he/she found to be unclear or illogical. We do not see where our writing is with 'frequent disconnection in logic between sentences'. After re-reading our manuscript, we did not find any of those 'disconnections'. Such general statements are not useful because we cannot check them. To better respond, we need specific page and line numbers.

It isn't entirely clear what the reviewer means by 'Development of calculation system is great achievement...' but it sounds positive, so thank you. The comments around 'validity of the

many assumptions' are confusing and inaccurate: the primary assumption that we made is that of equilibrium conditions of the current soil organic C for the baseline simulations. We believe that our description of the simulations under a standardised framework, which is described and depicted in Figure 1, is clear. Still, of course, we would have been open to improving our explanations and writing if the reviewer had been more specific. Similarly, the comment around 're-writing' of the manuscript isn't particularly helpful. The reviewer needed to point out precisely where our English lacks for us to be able to respond. The corresponding author is practically a 'native speaker', and other colleagues who are proficient in English read the manuscript before submission. Of course, we could 'fine-tune' and 'tighten' some of our writing, but, we argue that, for the most part, our paper is grammatically correct.

R2: Second, future projection of 100 years generally requires the use of climate change scenarios but there is no description on this. I understood that the future projection in this study was conducted by using current meteorological data. This is curious.

Authors: We do not know where the reviewer got the impression that we ran 'future projection'. Please note that we ran long-term simulations, not projections with or without climate change scenarios. Our intent here was to look at the potential for soil C capture, not at the effects of climate on soil C stocks.

R2: Third, the setting of the changing amount of C input in future projection is not realistic. I do not think six times higher organic matter input is realistic scenario. It is natural that increasing C input result in higher soil C qualitatively of course. Quantitative estimation by using realistic scenario (both future climate change and management scenario) with well-calibrated model will be valuable but this study is far from it at this moment. Consequently, I have to evaluate this manuscript as rejection. I am happy if the following comments would be useful.

Authors: Again, what we did was not a future projection. More importantly, it would have been more useful if the reviewer provided some evidence to support why he/she thinks the range of different C inputs that we considered is not realistic. For example, C inputs six times

larger than the baseline are possible with manuring. We selected the rates to represent a wide range of possible C inputs.

It is disappointing to read the reviewer's recommendation to reject our manuscript. Based on the comments made, it appears that his/her advice is based mostly on misunderstandings and misinterpretations that seem to stem from preconceptions.

R2: L85-87: Please explain how you dealt with "BIO" pool of the original RothC, too, here. You mention other four pools but not BIO.

Authors: We could add the sentence as follows: "The BIO pool was initially set to zero (Sparling, 1992)."

R2: L96-97: The original RothC uses monthly precipitation and open pan evaporation to calculate soil moisture condition. Did you change this part by using AWC? If so, please explain.

Authors: Thank you for the comment. We did not modified the original routine that calculates topsoil moisture deficit from rainfall and open-pan evaporation. We used AWC to derive evapotranspiration (ET) from pan evaporation when a plant is present and then to calculate a potential biomass production that is water-limited using the ET. To clarify, we could revise as follows "The available water capacity (AWC) of the soil to a depth of 1 m is needed to modify evapotranspiration from pan evaporation when a plant is present and used to run a crop model (see below)."

R2: L98: What kind of soil properties did you estimate by visible-near infrared spectra.

Authors: We can revise as follows to clarify "The soil properties ..." to "The C fractions, clay content, and AWC ...".

R2: L101-105: You must explain more about land cover here including definitions of cropland, modified or native razing land and native environments (which appeared in Figure 2).

Authors: Thank you for the suggestion, we agree and can improve the definition of the selected broad land uses in section 2.3.2 as follows: "We defined cropping as land under

broadacre crops. Modified grazing was defined as land used for livestock grazing on improved pastures with exotic vegetation cover. Native grazing was defined as land used for grazing on native pastures. Natural environments include the areas for nature conservation, indigenous uses, and other minimal uses."

R2: L127-132: I did not understand this part.

Authors: Here, we described how the dominant crop/grass species were determined based on the activity data derived by Unkovich et al. (2017). We could rewrite: "For each of the periods 1990–1994, 1995–1999, 2000–2004, 2005–2009, 2010–2014, we calculated the cumulative frequency by regime. We used it to randomly select the crop or grass species (both annual and perennial) through time with a probability approach. The probability to have a certain crop was dependent on the cumulative frequency assigned to each crop type and regime." Would this clarify?

R2: L136: How did you relate evapotranspiration value with pan evaporation value? Explanation needed.

Authors: Thanks, yes we could clarify as follows: "Daily evapotranspiration was estimated by multiplying pan evaporation with a ratio of soil water content over plant AWC and the maximum evapotranspiration by crop or grass."

R2: L150: Please show specific value of shoot to root ratio and show reference.

Authors: Thank you. We used a generic root-to-shoot ratio of 0.3, not a crop-specific value due to a lack of data for Australian conditions. However, we have added Bolinder et al. (1997) to the references to show this chosen value within the typical ranges.

R2: L151-155: Many assumptions here. Where did you get the value 1.25, for example? Please show references for each assumed value.

Authors: These assumptions were based on typical cropping and modified pasture management practices in Australia and are the ones typically considered in the APSIM model. According to the Department of Primary Industries and Regional Development (DPIRD),

grazing pastures should be based on the pasture growth of dominant pastures (e.g. clover). Their optimum growth occurs at about 1400 kg DM/ha, with the recommended minimum of 1000 kg DM/ha, to maximise pasture production. We can provide the references as suggested.

R2: L157: How did you calculate 0.049?

Authors: Simply, the number represents the small amount of C inputs from sparse vegetation in arid and semi-arid climates. The monthly C inputs amount to roughly 0.5 Mg C/ha/year. According to Wang and Barrett (2003), which is now cited, a typical C production at these areas is relatively low, ranging from 0.1 to around 1 Mg Mg C/ha/year.

R2: L167: What does "each site" here mean? "73 sites" in L161? Or 4431 sites in L177? Explanation is not easily understandable.

Authors: These sites are the 4,431 sites, which should be clear from the context in which the sentence is written. However, we could be more specific and write "4,431 sites"'.

R2: L169-171: I think 100 years are too short to reach equilibrium. How did you set 100 years here?

Authors: The reviewer misunderstood. We initialised the model with measured C fractions to represent the pool structure of ROTH C. We did not use a typical spin-up simulation to establish the relative size of the conceptual pools. The 100 years is for the baseline simulations assuming no change in environmental conditions and land management that would affect decomposition. In fact, we set the simulations to ensure that both dynamic pools were at equilibrium over the 100 year period. The reason we choose 100 year was because we wanted to make predictions over the this period, which corresponds to the Australian Emission Reduction Fund permanence period for carbon farming projects.

R2: L171-172: "from their initial values by a fraction of 1/100" is not clear explanation. From which value (minimum) to which value (maximum) for example? Please explain more in detail.

Authors: Please note that the amount of C inputs initially derived by the model was different for the 4,431 sites. For example, if the default value is 1 Mg C/ha, the default value would

change by 1/100 at the first iteration. And then at the second iteration (and still in transition phase), the modified value at the previous iteration would change again by 1/100 and so on. We note that inadvertently, we had written 'a fraction of 1/100...', where in fact we simply meant 1%. 'We could change the sentence to "The monthly input of plant residues and farmyard manure changed from their initial values by 1/100, and then this step was reiterated from the modified values until equilibrium was achieved."

R2: L182: Monthly variation?

Authors: Yes, we calculated the range of variations in the simulated TOC stocks on a monthly basis.

R2: L183-184: Why 10 Mg C ha-1 to exclude?

Authors: Our selection of this threshold was based on the National Carbon Accounting System (NCAS) dataset, where we found the range of yearly changes to be up to 10 Mg C. Thus, we selected this as the threshold. We described the conditions that need to be satisfied for modelled soil organic C reaches equilibrium. For these 388 sites, one of the dynamic pools, POC or HOC, failed to be constant with time. We could clarify the sentence as follows: "We considered 10 Mg C ha^{-1} as the threshold based on the range of measured annual changes in TOC." We did write that the 388 sites were characterised by large TOC stocks (median 75.04 Mg C/ha), but we do not know why these sites had such large changes in the dynamic pools. We do not yet know whether or not these are unrealistic. One possibility is that the pool composition of large organic C stocks is not fully constrained by the decay rates and environmental factors (see Figure 6).

R2: L184-185: This sentence should be in "Results".

Authors: Thank you for the suggestion, but no, we need it in the materials and methods because we excluded these sites from further simulations.

R2: L188-19: 100-years of future prediction generally uses future climate change scenarios. Why the authors did not do so? Did you use just current meteorological condition for future

100 years?

Authors: We did not use future climate change scenarios because that is not the purpose of this particular manuscript. Yes, that is correct, we calibrated the model and then ran simulations to look at the effects of changing C inputs.

R2: L190-191: Is 6 times greater C inputs achievable? This is very large amount so you have to discuss if such amount of organic matter could be available in terms of resource availability.

Authors: This comment is similar to one the reviewer made previously. We performed the simulations using a realistic range of C input changes that correspond to a wide range of activities. We agree that manure addition might be unrealistic for most systems, however, it provides a feasible upper limit. We thank the reviewer for his/her opinion on the need to think about resource availability, but we do not believe that such commentaries will strengthen our argument or manuscript.

R2: L195: Why 11-year moving average? Explanation needed.

Authors: Moving averages are generally computed for environmental data such as climate to smooth out the decadal trend in the data. We used 11 years because it is generally thought that it takes around 10 years to capture meaningful soil C changes due to management changes.

R2: L198: 100 years is not enough to reach equilibrium in many cases. How did you judge if it reached equilibrium or not? Explanation needed.

Authors: This comment is similar to one made previously and we responded. The reviewer needs to understand that the model was site-specifically initialised with measured C fractions—no spin up simulations needed.

R2: L213-214: I could not read median value from this figure.

Authors: We apologise for that. We could add the median values to the caption of the figure.

R2: Figure 3: Some of characters of horizontal axis are overlapped and not visible.

Authors: That is strange. We cannot see the issue in our figures. Perhaps the editor can help?

R2: Figure 4: Title of figure is not easily understandable.

Authors: If it helps, we could change the caption to: "Changes in total, particulate, and humus organic C ..."

R2: L246-247: please show data to support this sentence.

Authors: We do not think that showing data on this would clarify or strengthen our point, and would simply be redundant. If the editor thinks it would help, we could provide climate maps to cross check with Figure 4.

R2: Figure5: TOC in left panels should be ROC. TOC=POC+HOC+ROC. Is this correct? Definition of vulnerability should be explained in Figure caption, too, even it is in main text, so that figure can be self-understandable.

Authors: Here, TOC also included the DPM and BIO pools. Since we do not discuss ROC we would keep TOC on the plot. We could do as suggested and include the C vulnerability equation in the caption: "The C vulnerability is derived by POC/(HOC + ROC).".

R2: L257-258: Why changes in stock under grazing and cropping will be similar if climate and soil texture have a dominant effect? Not understandable. Explanation is not enough.

Authors: Good point. We could clarify it to "..., possibly because of a similar range of climate and soil texture that have a dominant effect of on the C inputs in these areas."

R2: L259-260; 261-263: This should be due to the difference of DPM/RPM ratio. Please add discussion on this.

Authors: We do not understand this comment. We described that the changes in TOC and POC followed a similar pattern under both cropping and grazing. Please note that the DPM/RPM ratio was optimised during model calibration so here the main driver for the changes would be largely due to changing the amount of C inputs.

R2: L270-271: This sentence is not needed. Should be deleted.

Authors: It isn't clear why the reviewer think the sentence should be deleted. We prefer to keep it, thank you.

R2: L286: I did not understand the relationship between this sentence and sentences before and after.

Authors: We do not see anything unclear in the sentences. The first discusses the site-specific estimation of the model parameters and the second suggests that our approach optimised both the amount and the quality of C inputs to maintain the current baseline soil organic C stocks. No change needed here

R2: L297-298: This comparison does not make sense because the area of each land use is different.

Authors: The reviewer misunderstands and must realise that both studies were conducted in Australia using the same land use classification: cropping regions, areas of modified and native grazing and natural environments are the same!

R2: L304-306: So why you did not use more complete dataset like Viscarra Rossel et al. (2014, 2019)?

Authors: Thank you for the comment. Yes, we could clarify by adding in section 2.3.1: "We selected a total of 4,431 out of 5,721 sites across Australia (Viscarra Rossel et al., 2019) (Figure 2). The selected sites were under the dominant land use, namely cropping, grazing of modified pastures and native vegetation, and natural conservation and protected areas. Native forests and production forestry were excluded because of a lack of simulation capacity."

R2: L306-308: I could not understand why this concluding sentence appears here. It is disconnected from sentences in this paragraph.

Authors: We do not understand the comment. In terms of the sentences in question, there is no 'disconnection'. The first, suggests why our ROTH C baseline estimates of the C stocks and composition differ somewhat from those produced by Viscarra Rossel et al. The second sentence expands, suggesting that unlike those previous estimates, the ones we present here, with ROTH C, can explain the soil processes that are important for estimating the baseline stocks of soil organic C and its composition.

R2: L313: I do not think this is "plausible" as mentioned above.

Authors: We thank the reviewer for his/her opinion, but it would be more useful if he/she could provide evidence to support his/her comment. Increasing C inputs by up to 3.5–12 Mg C/ha is entirely possible. For example, via management changes, e.g. manure addition. As with our previous response, we agree that manure additions might not be practically or economically feasible everywhere, but it does provide our simulations with an upper range. We do not see a problem with this.

R2: L313-315: You must discuss the reason of these difference among land use.

Authors: We did discussed this point, however, we could improve this discussion.

R2: L316-318: You must discuss or explain why soil C become more vulnerable when soil C increases. Sentence of L317-318 does not say anything.

Authors: We have discussed this point. Please see the main text (line 340).

R2: L327-329: You must explain more why this C input level was plausible. Explanation is not enough.

Authors: We responded to this comment already.

R2: L330: I could not imagine how to "manage it locally". Explanation needed.

Authors: We think that this aspect of our discussion is clear. However, if not taken in context, it could be challenging to understand. Paraphrasing our argument around L330, our work has shown that the baseline rate of C inputs into the active POC and HOC pools is site-specific. Therefore, soil management (e.g. via farm management practices like increasing residue retention rates) needs also to be local (i.e. site-specific), else we risk mismanagement and soil C loss. Further, locally derived (i.e. site-specific) C inputs are needed to identify soils that could potentially sequester C.