

Response to Associate Editor

AEditor: Your manuscript was meanwhile assessed by three new reviewers. All three reviewers note that your work is of high technical quality and of importance but request improvements before a potential publication. The three reviewers offer points of criticisms and very valuable advice on how to further improve the manuscript. I ask you to carefully consider the issues raised by the reviewers and to prepare a revised manuscript and a point-by-point reply to the review comments. The manuscript will then be sent to the two reviewers that asked for major revisions.

Authors: Thank you for recognising the technical importance of our work. We have revised the manuscript and provide point-by-point responses to all of the comments made. Please note that we also improved the title of the paper to better represent what we did. The new title is 'Assessing the response of soil carbon in Australia to changing inputs and future climate change using a consistent modelling framework'. We note that we have a new co-author in the manuscript. The first author of the manuscript, Dr Lee, left our group and so we had reduced capacity to complete the revision. Dr Mingxi Zhang helped to perform the new simulations that account for climate change.

AEditor: Reviewer #3 asks that the underpinning data and codes are made publicly available. It is becoming more and more praxis and also requested by Biogeosciences to publish input data sets, results, and codes on a public server, preferentially with a doi attached. I ask you to follow this practice to the extent this is reasonable.

Authors: The research reported in this paper is the first of three planned publications from an on-going project that started three years ago. There's been significant investment in the collection and collation of data, the laboratory and sensor measurements, the development of the various code to implement the framework and the simulations, etc. Until we have completed the project and reported our planned outputs, it isn't sensible for us to release the data or code. Of course, we are always receptive to collaborations and would support innovative uses of the data and code, under specific agreement, depending on the request and

situation. Therefore, at this stage, we'd prefer to keep the statement as 'on reasonable request to the corresponding author'. We provide a more complete response to referee #3, below.

AEditor: Reviewer #4 raises three important scientific issues. (i) Estimating the effect of changing inputs on SOC stocks over a 100-year time horizon without consideration of climate change appears not meaningful. This is certainly a valid criticism as global warming is continuing. A relatively small effort, with results for a small number of climate scenarios and a 'mean' input, might inform the reader about the sensitivity of your results to different climate conditions. These additional results may be presented in an appendix and briefly discussed in the main text or presented in the main text. (ii) The reviewer also recommends to discuss shoot-root ratios under altered input and to compare these ratios to literature values as an overall quality control. (iii) Finally, reviewer #4 notes that some of the input scenarios are partially unrealistic. This point should at least be reflected in the discussion.

Authors: We have revised the manuscript as suggested. (i) We run additional simulations that account for climate change. These are described in a new section '2.3.6 Simulation: the potential for C sequestration under a changing climate' and the results are reported in the new section '3.3 Effect of changing climate on soil organic C' and new Figure 6. Of course, the new results are also discussed in the Discussion section. (ii) We provide a response and clarification to the comments around the shoot-root ratios, please see below. Please note, in our framework, optimising C inputs for the baseline would be less sensitive to this parameter as we have tuned the amount and quality of plant-derived C inputs. (iii) We have revised the C input scenarios by limiting these to $2 \times$ the baseline C inputs, and have revised the relevant sections of the manuscript as well as the figures.

AEditor: Reviewer #5 appreciates the technical aspects of your work but calls for an improved presentation. The reviewer critiques vague and subjective statements, and the lack of a clear definition of the problem, the outcomes, and the novelty. Please consider these comments very carefully and revise your manuscript accordingly.

Authors: We could understand most of the points made by the referee and we have

responded and revised the manuscript accordingly. As suggested, we have removed subjective expressions from the script.

AEditor: Please follow the guidelines of the journal when preparing your response: The author's response in case of "minor" or "major" revisions must be submitted as one separate *.pdf file (indicating page and line numbers), structured in a clear and easy-to-follow sequence: (1) comments from referees/public, (2) author's response, and (3) author's changes in manuscript. Regarding author's changes, a marked-up manuscript version (track changes in Word, latexdiff in LaTeX) converted into *.pdf and combined with the author's response should be provided.

Authors: We will do so. Thank you for the information.

Authors: We thank the three referees for taking the time to review our manuscript. Below are our responses that also indicate the specific revisions made.

Response to reviewer 5

Referee: Abstract: The opening of the abstract is hard to follow, I can imagine any number of "disconnects" not disconnections, but the abstract doesn't state the problem being addressed. I also think this is a technical issue and put a modeling issue before the science question. The abstract should begin with a clear statement about what the authors are actually studying. Something like "Soil organic matter is a key reservoir for carbon, and its vulnerability to global change depends also on many anthropogenic management activities" ..

Authors: We have revised the opening of the abstract to "Land use and management practices affect the variability of soil organic carbon (C) to global change through associations among climate, vegetation and soil. Soil process models are useful tools to simulate C dynamics, but it is important to bridge any disconnect that exists between the data used to inform them and the processes that they depict."

Referee: This is pretty generic: "However, a better understanding of soil organic C dynamics is needed to determine the size of the soil C pool accurately and to assess the potential for

those opportunities”. Can you be more specific about why better understanding is needed?

What are the big uncertainties? This statement can be said about anything, but without specifics, why bother saying this? This is an opportunity to be specific, not generic.

Authors: We revised the sentence as follows: “However, these opportunities depend on regional interactions between soil, climate, land use and management … A better understanding of the effect of these interactions on soil C is needed to assess the potential for those opportunities.”

Referee: Is science a popularity contest? Is this really what you mean to say? Other reasons for their continued popularity might be that there is ample documentation on them; they are relatively simple and general and are therefore also well understood. I would think, and it would certainly strengthen this paper if Roth-C and Century were not popular because they are simple and easy to use but because they are well-tested, accurate and robust (which I believe they are), and I believe there’s little evidence that more complex models are more accurate for this type of study.

Authors: Of course, science is not a popularity contest. We meant that these models are the most commonly used for the reasons that the reviewer mentioned, which is the reason we used this particular model in our study. To reflect this, we have removed “for their continued popularity” and revised the sentence to “Other reasons for continued application of these models might be that there is ample documentation on them; they are relatively robust and well tested and are therefore also well understood.”

Referee: A key part of this paper is the integration of the ROTH-C model and data, yet the introduction is strangely vague on this subject. What deficiencies arise from poor data synergy, and what approach is proposed? The paragraph around Line 60 is very superficial.

Authors: We disagree that our introduction is vague. This section states the challenges with soil C simulation modelling and cites relevant literature. The reviewer should note that the issues are not restricted only to the ROTH C model. In the introduction, we have a paragraph, starting with “Simulation of soil C dynamics with biogeochemical models can be challenging.”

Then, we indicate a direction that we intended to follow, and then the last sentences in the same paragraph we write (about frameworks for simulation modelling): “Their development should also allow for their efficient updating, with new measurements, data and models, as they become available ..., and enable a more systematic approach for calibration and validation, making simulations more reliable and reproducible.” Note also that the next paragraph describes our motivation and aims, which clearly state our proposed approach.

Referee: Figure 1: How is this different? The input data sets look very standard and can the figure show what’s novel in this approach? Eg: remote sensing, spectroscopy, etc

Authors: How is this different to what specifically? We believe that the figure is important because it provides the reader with an easy-to-follow diagram that shows the steps that are included in our framework. The novelty of our approach is the integration of different data (from remote, proximal sensing, new data products, etc), which represent different scales and resolutions, and how they were processed, simulated, aggregated, etc.

Referee: I always discourage statements like the following: This gave us confidence in the performance of the model in Australia. What does “confidence” mean? Good enough for a paper, good enough to make management recommendations with financial consequences? I much preferred statements about the likely uncertainty, and openness about where and when the model could fail! Confidence is like belief, it has very limited place in a scientific paper!

Authors: We revised or deleted the word “confidence” in the ms, except when referring to confidence intervals. Specifically, in the abstract, we’ve revised “improve confidence in the model’s estimates” to “improve the model’s estimates”. In the section 3.1, we’ve deleted the last sentence, which is “This gave us confidence in the performance of the model in Australia.”. And in the section 4.1, we changed the relevant sentence to “... we empirically assessed how well the baseline simulations matched the model’s corresponding dynamic pools, which suggest that the model is able to represent various Australian soils. ”

Referee: Figure 3: these model fits are SO good they raise concern, when simulating so notoriously variable a quantity as SOC, some discussion of why and whether that would be

expected away from the calibration sites! In the caption, it states after optimization but it would be very easy for a casual reader to assume this is the likely model accuracy, especially after the “confidence statement”.

Authors: Our manuscript describes the methods and steps that we took for the model optimisation and we reported the sites where the optimisation failed. We also hope now there is no confusion arising as we addressed the concerns over the word, “confidence”.

Referee: Figure 4 and 5 are very nice. The estimate of importance in Figure 6 is quite unclear, and could be expanded or even defined in the caption.

Authors: We have added “The importance of each soil variable was assessed based on the usage of each individual variable in the rule conditions and the model for Cubist.” to the caption, as suggested.

Referee: Around Line 305: I suggest avoiding “performed well” “additional confidence” and replace all of these statements that are subjective with explicit statements of model skill. Statements about “performing well” or “confidence” should be reserved for the very end of the discussion where the use of the model and output for managers is mentioned. In the results and discussion, focus on skill metrics, not subjective value statement.

Authors: We revised the use of subjective expressions throughout the manuscript. We've changed the sentence on line 310 to “The model explained 73–98% of the variation in the size of the C pools in soils that are under cropping and 86–98% of that under grazing, while the simulation under natural environments ...”

Referee: This statement remains difficult for me. What is the disconnect? By this point statements like “Soil measurements are very local and sparse, yet soil information is required at farm to national levels”. Not “disconnect”. There are specific challenges, not well-summarized by “disconnect”. This whole paragraph starting section 4.2 is redundant with earlier statements and adds no detail. What do these data sets add? I can see the authors grasping for a point here, but this entire section could be summarized by “Incorporating both local and mapped information enables the model to be run for large areas, or with more specificity for

particular sites". This has been done many times with ROTH-C (and Century)—these authors have developed a particular coding and data management approach, but it is very similar to gridded Century with its schedule files.

Authors: In this study, among many challenges, we are focusing on two challenges: 1) a lack of details in the previous approaches and 2) a need for a framework that better incorporate new measurements to simulate soil C dynamics depending on the specific choice of scales. We understand the comment.

Referee: Future needs is so generic as to be useless

Authors: We disagree with this comment. The points that we made are specific to the the framework built with the ROTH C model by listing what's still missing that requires in the workflow of the framework, as well as the direction that we think should take place for further model improvements in the near future.

Referee: The conclusions are repetitious with early text and could be strengthened significantly. There is a functional disconnection between measurements, data and biogeochemical models (Blankinship et al., 2018), but by simulating under a framework, like we did here, we can bridge that disconnect.

Authors: Thank you for the comment. We have done as suggested.

Response to reviewer 4

Referee: I reviewed the revised version of the MS, also taking note of the previous reviews and changes that were made to the new version.

This is a very interesting, technically demanding study where the controlling parameters input rate and input quality for soil carbon stocks in Australian ecosystems were evaluated by using RothC and an elaborated set of external data. The overall structure of the work, its general quality, and the fact that it seamlessly follows a number of previous Australian studies on the same topic, where different data sets and methods have been developed that were also used here, is appreciated.

Authors: Thank you.

Referee: In line with a previous comment, I see a major drawback in that the authors did not use climate change (CC) scenarios for calculating the effect of increasing C inputs to SOC stocks. Considering the well documented effect that CC will have on future SOC in previous modeling approaches from other regions, it seems not plausible to rely on a repetition of time windows of past climate to simulate the response of SOC to changing conditions which, by nature, can only take place in the future. I therefore suggest that the authors use CC scenarios for evaluating the effect of higher C input on SOC storage. In this context, also the discussion starting in line 362 could be extended towards climate as an important determinant of long-term SOC changes.

Authors: We have now run the simulations considering projected future changes in climate (1.5, 2 and 5.0°C) over a 100-years. The relevant sections of the paper were revised accordingly: New methods section ‘2.3.6 Simulation: the potential for C sequestration under a changing climate’; new results section ‘3.3 Effect of changing climate on soil organic C’; new Figure 6 and also revised the abstract, discussion and conclusions.

Referee: Estimating or simulating belowground input is crucial for every SOC modeling study. On pages 6-7, the approach used for estimating these inputs is described. A fixed shoot:root ratio is considered to get the belowground part from the crop model (whether or not shoot:root ratios vary for a specific crop depending on external conditions such as management is a matter of debate in the literature [e.g. Hirte et al. 2018], but such a simplified approach seems justified given the poor availability of belowground input data from experiments). The authors then change monthly inputs (page 8) to better understand the response of the SOC pool to it. In consequence, shoot:root ratios will change as well. I suggest to display and discuss the resulting shoot:root ratios, as they provide a means of quality control to the overall approach. Reviews such as the one from Bolinder et al. (2007) might be helpful to put the derived ratios into context.

Authors: We appreciate this comment. The shoot:root ratios are fixed as a constant over

time for both annuals and perennials, although we fully acknowledge the ratios tend to vary by species, management practices, location, time, etc. This is because, in this framework, we use a relatively simple plant model to set the starting amount of C inputs from plant residues and manures as a function of temperature and soil-water conditions so that we could consider different monthly growth rates of the plant. As this model is not directly linked to the soil organic matter routine of the ROTH C model, the routine is not dynamic and does not allow back-calculation of the resulting shoot:root ratios. In addition, their short-term variation would have a little impact on the longer-term soil C baseline when the model is optimised. Nevertheless, we point out the need of more plant properties data because of the need to verify the baseline C inputs by land use. Such data might be of use to improve the overall optimisation process within the framework.

Referee: The selected range of inputs of between 0.25 – 6 times the equilibrium input seems wide, and Fig. 4 indicates inputs of up to 10 t C ha⁻¹ per year. Even though the authors acknowledge that allocating inputs to sites/soils where higher additional storage might be achieved (line 360) is aimed for, an increase by more than 2-fold seems highly unrealistic given issues of transport or nutrient input. For comparison, a recent study from the temperate zone came up with an estimate of c. 3.7 t C ha⁻¹ for croplands and grasslands (Jacobs et al. 2020). Therefore, I consider these input scenarios as partially unrealistic and suggest to downscale them a bit.

Authors: We now report the simulations with a range of inputs of up to 2 × baseline C inputs, and revised the relevant sections of the manuscript and figures.

Response to reviewer 3

Referee: The authors have identified a genuine (and insufficiently noted) problem, i.e. that there is a general disconnect between models and observations of soil carbon dynamics. Their approach to resolve this is pragmatic, making use of a long-standing model (RothC) whose design, and (relatively low) level of complexity, are appropriate to the task. After site-specific initialization and some calibration, they were able to show very good agreement between

observations and simulations. They then used the model to answer an important practical question regarding the potential for changes in land management practice to deliver carbon sequestration benefits.

Authors: We thank the reviewer for his/her understanding of the motivation of our research and our approach.

Referee: The manuscript is generally well written and clear in its statements of objectives, assumptions and methods. There are many open issues about how best to model soil carbon dynamics, given that we now know that the conceptual categories used by this generation of models do not, in fact, correspond to chemically distinct classes of compounds, but rather to different degrees of physical protection from microbial attack. However, like the authors, I am not convinced that any of the recently published alternative formulations provides a useful way forward for applications of this kind. Meanwhile, work like this needs to be done, while in practice very little of it is being done anywhere. The originality of this research thus does not lie in any particular advance in modelling or theoretical understanding, but rather in the way it uses an established modelling framework to answer pressing real-world questions underpinned by a sound observational basis.

Authors: Thank you.

Referee: I would like to raise just one issue about the availability of data and codes. Today, in my view, it is no longer acceptable to make the underpinnings of a scientific paper available only “on reasonable request” – which leaves it open to the authors to deny access. This information should instead be made available via a public repository, thus greatly increasing the potential utility of the research as well as making the results open to alternative analytical approaches.

Authors: We appreciate the reviewers comment, and generally, for single-output projects, we would agree as we also hold a similar view. However, in this case, currently, full release of all of the data and code isn't 'straight-forward'. The research reported in this paper is part of a project started three years ago and the project is on-going. There's been significant investment

in the collection and collation of data, the laboratory and sensor measurements, the development of the various code to implement the framework and the simulations, etc. This paper represents the first of three publications from this project. Until we have completed the project and reported our planned outputs, it isn't sensible for us to release the data or code. Of course, we are always receptive to collaborations and would support innovative uses of the data and code, under specific a agreement that will not hamper our research, which will depend on the request and situation. Therefore, at this stage, we'd prefer to keep the statement as 'on reasonable request to the corresponding author'.