Responses to reviewers' comments

Dear Dr Akihiko Ito,

Thank you very much for accepting our manuscript for open discussions in your journal. The interactive discussion process is very helpful. The two anonymous referees provided constructive comments that are helpful for improving the manuscript. The manuscript was also commented by two other colleagues. Dr Göran I. Ågren and Dr Hatem Ibrahim showed their interest of the study, and kindly commented on the paper. All these reviewers concluded that we presented an interesting study and the manuscript was well written. We appreciate all the comments from these reviewers, and have carefully addressed them point-to-point to revise the paper. We have marked all the modifications in the manuscript in BLUE colour for easy recognition by you. You can find our detailed responses in the following sections.

We look forward to hearing from you again.

Regards,

Zhongkui Luo (Zhongkui.luo@csiro.au), Enli Wang, Hongxing Zheng, Jeff A. Baldock,

Osbert J. Sun, Quanxi Shao

##

Comments from Anonymous Referee #1

Luo and coauthors present a nice analysis that examines the challenges in parameterizing and reducing uncertainty in soil C models that are used for land management and policy decisions. Even with an spatially and temporarily robust dataset from agricultural sites around Australia they find that well calibrated models over the observational period still show significant uncertainty in trying to make future projections about the fate of C in a changing world.

Response: We appreciate these comments which highlight the importance of this study.

While I strongly agree that better understanding and structural representation of microbial physiology, C quality, and management effects are needed to reduce uncertainty in soil C projections (section 4), I'm not convinced the data presented clearly support these conclusions. A significant amount of confusion is generated because the manuscript presently conflates model structural uncertainty, parameterization uncertainty, and forcing (or scenario) uncertainty in the analysis (see Hawkins and Sutton 2009). In me estimating focusing on the first one of two of these types of uncertainty would strengthen the conclusions being made here.

Response: Thanks you very much for your comments and suggestion. Yes there are several types of uncertainties and it is complicated and extensive task to deal with all the uncertainties together. As the reviewer suggested, focusing on one or two types uncertainties would strengthen the conclusion. Indeed, our current study focused on the uncertainty induced only by model parameterization and model initialization. In the revision, we carefully clarified this objective. More details are given in the following sections in responding to the reviewers' comments.

General comments

• *Parameter uncertainty*: A significant amount of effort went into reducing parameter uncertainty in the model at each site (Figs 1 & 2) I'm surprised that sensitivity analyses of temperature, moisture, and N scalars weren't considered in this model since previous work demonstrates that model results are strongly determined by these parameters (e.g., Todd-Brown et al. 2013; Exbrayat et al. 2014). Would consideration of these parameters in the optimization routines better constrain the projected uncertainty, or compound the equifinality problems mentioned in section 3.1?

Response: We agree with the reviewer that how the model simulates the response of soil carbon change to temperature, moisture and nutrient availability (e.g., nitrogen) is important. Different models usually use different response functions to modify their potential decomposition rate. The uncertainty related to these response functions is more associated with model structure, thus was not included in this study where we focused on uncertainties related to model parameters.

Our main focus in this paper was to assess the uncertainties associated with the determination of model parameters and initialization of the SOC pools using measured SOC data. In the APSIM model, as in other carbon models, actual decomposition rate of each carbon pool is simulated as the maximum decomposition rate modified by soil temperature, moisture and nutrient availability, etc. (as described in lines 117-119). The decay constants of the SOC pools, together with the carbon use efficiency (CUE) of microbes, are the parameters we investigated. Due to the unmeasurable nature of the SOC pools in the model, the uncertainty caused by initialising these pools using measured total SOC was therefore also included.

Due to the many parameters involved, further inclusion of the types of response functions will indeed compound the equifinality problems. Therefore, we decided to look at the impact of response functions in a separate study.

To satisfy the reviewer's concern on this point, we explicitly discussed the above points in the revised manuscript and cited the relevant references. See lines 321-327.

Fig. 2b shows a split in SOC projections for both high and low inputs. One is left to surmise this bifurcation in results is generated by the concurrent split in parameter space shown in Fig. 2d. The authors hint at this finding at the end of section 3.3, it's never adequately discussed in section 3.2, where optimization results are presented.

Response: Thanks for this point. The bifurcation in results was generated by the split in parameter space, which was caused by using different objective functions in the optimisation. We expanded the discussions on the reason and consequence of the bifurcation pattern showed at Brigalow in section 3.2 (see lines 354-359).

Optimized CUE values seem quite high in Figs. 1ab, 2, especially given conclusions by Sinsabaugh and others (2013) that CUE values in soils should be considerably lower? I wonder if better constraints on the prior distributions of parameter values may lead to different conclusions? I'm not sure such analyses are warranted here, but discussing this dependency of prior distributions in Bayesian analyses seems warranted.

Response: Sinsabaugh *et al.* (2013) suggested that CUE prediction should consider "resource composition, stoichiometry constraints and biomass composition, as well as environmental drivers”. In this study, we assumed a prior distribution of CUE ranging from 0.2 to 0.8. Although this range is relatively wide, the purpose is to cover the potential change of CUE. In addition, the mean of our derived cue was around 0.5 that is consistent with the estimates in terrestrial ecosystems.

However, we accepted the suggestion to discuss the potential limitation of prior distribution. See lines 416-421.

• *Structural uncertainty*: Similarly, it looks like all the sites have very different management practices (SI Table 1), but the effects of these different management practices are implicitly represented by site-level parameter estimation for steady state and temporal changes in soil C for each of the sites & treatments. Instead, I wonder if a single model would be better, with “global” decay constants that are modified by scalars for different management practices- in addition to temperature, moisture, and N scalars that already being used? With so many unconstrained parameters this approach may run into the same equifinality problems, but also may better constrain management effects on future soil C storage? I’m not asking that environmental or management scalars be evaluated here- but their potential importance should be discussed. Instead, my larger concern comes in how uncertainty analyses were conduced and the inferences drawn from them.

Response: Thanks for this suggestion. Management practice has substantial effects on soil carbon dynamics through its effect on soil carbon input (in terms of both quantity and quality) and soil properties. We mentioned this issue in several places through the manuscript (e.g., lines 436–438, 440–442 in the Conclusion section). See more on this point in our responses to the following comments on C input (the key final consequence of agricultural management).

• *Forcing uncertainty*: It’s not clear what actually generates the uncertainty shown in Fig. 2? It seems as though SOC parameters were optimized (Fig. 1), but that uncertainty in the crop response generated wide uncertainty in plant productivity, and therefore soil C inputs (which were not previously optimized). As the authors hypothesize in section 2.5, first order models are very sensitive to soil C inputs (again Todd-Brown et al. 2013). Projected inputs varied by more than a factor of two (section 3.2). Thus, uncertainty shown in Fig. 2a does not surprising- if this is what’s actually generating the spread in projections? If so, I’m not confident that conclusions about persistent uncertainty in soil C projection (section 4) are well supported by this analysis? To control for differences in plant inputs, could the authors increase residue by 10% for different parameterizations of soil C at each site and quantify the variation in SOC projections?

Response: Thanks for the careful review. For carbon input during the model calibration period, we used the observed amount of crop residue as a forcing input (i.e., there was no uncertainty in carbon input), which was described at the end of section 2.2.

For the projection period, no changes were made to the model parameters that control plant growth and therefore carbon input into soil. The changes were only made to the model parameters that control SOC decomposition and initialization (i.e., rate constant for humic pool, microbial carbon use efficiency, fraction of inert organic carbon etc), which are the only causes for the uncertainty in projected SOC.

In order to assess the uncertainty in soil C projections under a typical level of C input, during the model projection period, we simulated the soil C dynamics under different

N application rates thus different C input levels (see the first paragraph of section 2.5: lines 241-248). For example, in Fig. 2a, the simulated C input under 0 kg N/ha scenario (i.e., low C input) was low but similar when different soil parameter ensembles were used. Under optimal N input, C input was high and similar as well. The purpose of the nitrogen scenarios is to ensure that that the uncertainty in C input under a specific N management is small. And thus, the uncertainty in C projection (Fig. 2a and b) is induced by the uncertainty in the optimised model parameters.

Additionally, in the APSIM model, crop growth is predominantly controlled by soil water and nitrogen availability, given a specific climate. In our simulation, water availability is a function of rainfall and soil water holding capacity, and is not changed by those optimised parameters. For nitrogen availability, the model can well simulate soil C dynamics under all parameter ensembles (Fig. 1). As the modelled N dynamics couple with C through a constant C:N ratio for each pool, the simulated N could follow the similar pattern to C. This means the N supply for crop growth will be similar under different parameter combinations. We added more details on these points in section 2.2 (see lines 122-123, 150-152).

Subsequently, what if temperatures warmed [or soils dried] over the 100-year projection window, how would the temperature sensitivity of decomposition vary depending on tradeoffs between humus decay rates and partitioning to inert C? There is some speculation towards this effect in the middle of p 4261, but it's not clear how the authors generate climate uncertainty effects on soil C storage here? By isolating these variables, uncertainty in parameter estimates and/or model structure could be isolated (if this is the focus of the paper, as implied in the abstract), and would avoid confounding forcing uncertainty in the analysis.

Response: In this study, we did not quantify the uncertainty related to climate change, and repeated the historic climate from the last 100 years to drive the model. This information was added to the manuscript (see lines 252-254). As clarified in above responses, our study focused on the uncertainty induced by model parameterization and initialization. See our response to the next comment about the results on the climate factors.

• *Uncertainty attribution*: I have to admit that I'm not really clear what the intercept (α) and slope (β) parameters are showing (sections 2.6 & 3.3)? The authors conclusions seem to strongly rest on the change in α and β over sites and time (first paragraph of section 3.3). First order models can exhibit false priming (as in Fontaine et al. 2011) because initially increase soil C inputs enter pools with faster turnover times (FOM in this model), thus increasing soil respiration rates more than may be expected. Over time, however, as more C enters larger SOM pools with slower turnover times (humus and inert pools here) and the system begins to achieve a new equilibrium state the crop residue effects (Cr) on percentage uncertainty (Up) should increase. It's not clear if this is what's going on here, but I'd suspect this may explain why α and β both increase over time (Fig. 4b)? Similarly, sites with "well behaved" parameter estimates that have a narrow range of values for rdhum and finert (e.g., Tarlee, Fig 2a,c) likely have low α & β values, whereas sites that generate bimodal distributions of parameter combinations (e.g., Brigalow Fig. 2b,d) will have larger α & β values? Is that what's being shown in Table 1?

Response: In this study, we used a multilevel regression model to test that whether the uncertainty in soil C projections induced by model parameterization and initialization correlates with management in terms of C input (which was simulated under different

N application scenarios as described previously), and how this correlation changes across space (e.g., climate) and time (e.g., time period of the projection). The purpose of this analysis is to address why uncertainty change across space and time. To avoid the potential misunderstanding, we carefully revised the relevant sections to make the interpretation of the regression model clearer (see sections 2.6).

Briefly, we assumed that the uncertainty in soil C projections relates to C input (C_R), i.e., $U_P = \alpha + \beta\ C_R$. At the same time, we assumed that the relationship is different across experiments (Fig. 4a) and time period of the projection (Fig. 4b), i.e., the coefficients α and β are different among experiments and time periods of the projections. We further assumed that the attributes at experiment level (e.g., rainfall and temperature, and the generalized variance of the optimized parameters under the specific experiment) can explain the variation of α and β among experiments (Table 1). These assumptions were tested using the abovementioned multilevel model. Fig. 4 and Table 1 showed the relevant results.

• *Unsubstantiated claims*: In the same paragraph (bottom of pg 4259), there's discussion of 'optimal agricultural management'- which as something to do with residue management and N application? The authors also make what seem like widely speculative claims on the potential changes in agricultural soil C changes. Details of how these extrapolations were generated are lacking from the text, and I recommend removing this seemingly tangential finding from the text.

Response: Thanks for the suggestion. Yes it seems that the material is not directly relevant to the purpose of the manuscript. As suggested, we deleted the mentioned discussions.

Also, discussion of the potential effect of cultivation history on CUE seems very speculative (bottom of p. 4261). Although it's an interesting idea, with only a single site under "long" and "short" cultivation history, the results seems spurious at best, with no mechanism as to what would drive such changes in microbial physiology as a function of land use practices.

Response: The purpose of this discuss is to highlight that land use history likely affects both the composition of carbon pools and microbial processes, because land use history regulates soil environment and carbon input in terms of both quality and quantity.

In the revision, we further clarified the limitation of our results and the need for more research to confirm the findings; see lines 363-368.

Technical comments

• *Precise language*: The phrase 'carbon composition' is mentioned several times in the abstract (p. 4246, l. 14, 22, & 23) as well as several times in the main text (e.g. p. 4250, l. 9; 4250, l. 14; etc.) but this term is somewhat ambiguous. Is this referring to the chemical quality of SOM, its physical accessibility to microbes, or something else? Can the authors use more precise language for this phrase?

Response: Thanks for the careful review. We checked the whole manuscript, and replace "carbon composition" with more meaningful words, like carbon pools etc (e.g., 22, 29, 320, 438, and 451).

• *Structural Clarity*: The authors refer to the 'calibration' of their model and the 'calibration period', however, this procedure is never really described in the methods. I suspect that ‘calibration’ and ‘optimization’ (described in section 2.4) are being used interchangeably here, but this may not the true? Care should be taken to clarify language so readers can accurately understand results and discussion in the context of the numerical methods being applied. Maybe subheadings in section 3 that match those in the methods would help clarify results. (e.g., 3.1 Sensitivity analysis; 3.2 Optimization; etc).

Response: Thanks for the careful review. We followed this suggestion and changed the ‘calibration’ to ‘optimization’ in order to make the terminology consistent.

Similarly, it seemed as though results from DE optimization were going to be compared to the Bayesian approach (top of page 4252). It seems like Fig. 2 presents results from the DE optimization and Fig. 3 shows results from the Bayesian approach; however, from a comparison of the two methods is not clearly presented. I think this is actually discussed at the bottom of page 4261, and in SI Fig. 4 (section 3.3), but this text should be move up to the optimization section (section 3.2), as described in the methods (section 2.4).

Response: Thanks for the careful review. Change was made accordingly. See lines 360–380.

Use of model abbreviations in the text that are not clarified in the model conceptual diagram (SI Fig. 2) unnecessarily obscures findings for readers who are not intimately familiar with the model. The model is simple enough to deduce the abbreviations being used, but could be made more direct by labeling parameters of interest on SI Fig. 2, and / or simplifying the parameter names (e.g. *k*carb, *k*cellulose, *k*lignin… to describe the first-order decay constants of each pool).

Response: Thanks for the careful review. We accordingly updated Fig. 2 in the supplement and the parameters names (see Table 2 in the supplement).

• *Technical clarification*s: How does material get into the “Inert C pool”? This isn’t clearly described in the text of evident in SI Fig. 2, but it’s an important parameter in the model according to the sensitivity analysis (section 3.1, SI Table 2). Similarly CUE (which I’m used to seeing capitalized) is adequately described in the in the text, but not evident in SI Fig. 2. One is forced to assume that CO2 fluxes from each pool are equal to 1-CUE, and therefore the same for C losses out of each pool. But this should be clarified in the description of the model and it’s wiring diagram.

Response: Thanks for the careful review. Both inert C pool and CUE was defined in the revised version. See lines 114–117 for the definition of inert C pool and lines 120–123 for the definition of CUE.

I’m used to seeing plots like Figure 1a with the axes reversed, since here we’re interested in how the model (independent variable) can predict observations (dependent variable). Figure 1b is nearly unintelligible. Is this showing the 3 dimensional parameter space for the optimized parameters to generate steady state SOC pools in Fig 1a? The legend says that colors are described in Fig 2, but no description is provided there- forcing readers to assume that colors represent different ranges for the fraction of C allocated the inert C pool (Fig. 2c)? The one relevant finding one may draw from this figure is that turnover of the humic pool (rdhum, which I would suggest calling *k*humic) is inversely related to the fraction of C allocated to the inert pool (fintert). This apparent covariation, however, is never discussed (e.g. section 3.3).

Response: Fig. 1 and its legend were updated based on these comments. The correlation between *finert* and k_{hum} were further discussed and clarified. See lines 372-374.

It's unclear how the spatial distribution of the uncertainty analysis (Fig. 3) adds to the story being told here since it's never discussed in the text (section 3.2). As such does the map of individual study sites and their magnitude of SOC change communicate much? If not, maybe these projected results (and uncertainties) could just be added to SI Table 1, along with observed, optimized SOC pools?

Response: Thanks for this comment. Fig. 3 shows the spatial pattern of simulated soil C stock and its uncertainty under low (zero) and optimal N input levels, highlighting the importance of local climatic and soil conditions, and management practices. Although the numbers can be incorporated into the mentioned table, the information on the spatial patterns will be missed. This spatial pattern also links with the discussion in section 3.3 on the attributes controlling the variability of the uncertainty across the divergent climate, soil and experimental conditions. As such, we kept Fig. 3 in the manuscript.

Since Fig 4b is discussed before Fig 4a (section 3.3) can these panels be switched?

Response: Thanks for this comment. We rearranged some statements to ensure that Fig. 4a was cited before Fig. 4b.

References:

Hawkins, E., and R. Sutton, (2009) The potential to narrow uncertainty in regional climate predictions. *Bull. Amer. Meteor. Soc.*, **90,** 1095–1107.
Exbrayat J F, Pitman A J and Abramowitz G (2014) Response of microbial decomposition to spin-up explains CMIP5 soil carbon range until 2100 *Geosci. Model Dev*. **7** 2683–92
Fontaine, S., et al. (2011) Fungi mediate long term sequestration of carbon and nitrogen in soil through their priming effect. *Soil Biology and Biochemistry*, **43**, 86-96.
Sinsabaugh R L, Manzoni S, Moorhead D L and Richter A (2013) Carbon use efficiency of microbial communities: stoichiometry, methodology and modelling *Ecology Letters* **16** 930-9.
Todd-Brown, K. E. O., et al. (2013): Causes of variation in soil carbon simulations from CMIP5 Earth system models and comparison with observations, *Biogeosciences*, **10**, 1717–1736, doi:10.5194/bg-10-1717-2013, 2013.

Response: Thanks for these references. We incorporated the references that are closely related to our study into the revised version.

##

Comments from Anonymous Referee #2

This is an interesting paper illuminating the difficulties in making model projections even when past can be described accurately.

Response: We appreciate this comment.

Specific comments

1. Is there any reference, where the APSIM model is described in detail, see also comments below?

Response: Yes, we have added two references including a currently published paper. See line 104.

2. It is not clear from the manuscript if a constant CUE, independent of pools is assumed.

Response: Thanks for the careful review. The model assumes a constant CUE for all C pools. This was clarified in the manuscript. See line 120-123.

3. One parameter, rdhum, representing transfer to an inert pool, is used. This parameter must be better explained. Is it a rat r a fraction of other transfers?

Response: rdhum is the potential decomposition rate constant of humic organic carbon pool in the model. It does not represent the transfer to inert pool. We explained this parameter further and defined it clearly in line 317-318 and in the Supplementary Table 2.

4. Is the inert pool a constant 35% of SOC or will its size depend on the rate of transfers to this pool?

Response: Inert pool is defined as the pool that does not decompose. This value is initialized at the start of the simulation and will not change during the simulation. A clear definition of the inert pool was added in line 114-117.

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Comments from Dr G. Ågren (goran.agren@slu.se)

Luo et al. present an interesting study of model projections of soil organic carbon (SOC). In spite of very good model fits to past SOC development, the projections diverge drastically. I think this illustrates what we could call "the curse of equifinality"; there are many parameter combinations, or for that matter models, that fit data equally well but it is difficult to know which do this for the right reason and which do this for wrong reasons.

Response: We appreciated these comments. As summarised by Dr Ågren, our study demonstrated that accurate reproduction of past SOC did not guarantee convergent projection of SOC. In addition, we further assessed the relative importance of different model parameters in the uncertainty quantification, and found that the uncertainty caused by equifinality was also impacted by climatic conditions and carbon input (management), which provided an effective way to estimate uncertainty in predictions of soil carbon models across various climate and management conditions.

One way around this conundrum is to focus less on how well models fit data and find ways to constrain allowable parameter ranges and pay more attention to the internal consistency of models. I will here give an example of an analysis of the latter. I have dissected five SOC models CENTURY, (Parton et al., 1987; Parton et al. 1994; Paustian et al., 1992), DAISY (Hansen et al., 1990; Jensen et al,. 1997; Mueller et al., 1997),;ROTHC- 16.3 (Coleman &

Jenkinson, 1995; Jenkinson et al., 1992), VERBERNE (Verberne et al., 1990; Whitmore et al., 1997), and NCSOIL (Nicolardot & Molina, 1994). All five models describe SOC as consisting of between 2 and 5 pools with transfers between them and losses as CO2 (respiration).I have characterised each pool by a quality, which depends on the total rate (respiration plus transfers to other pools) at which this pool is depleted. From this I have then calculated the carbon use efficiency (CUE) as the fraction of C lost from a pool that is transferred to another pool; 1–CUE is the fraction lost as respiration. I have also calculated the dispersion D(q,q'), which describes the fraction of carbon from the pool with quality q' that is transferred to the pool with quality q. The results are presented in Figures1 &2. A more detailed description of the calculations can be found in Nilsson (2004). CUE is in most models independent of the quality of the pool but varies considerably between models but is in the range also found by Luo et al. Model studies in general, including Luo et al., tend to point out CUE as one of the parameters to which model predictions is most sensitive (see also Hyvönen et al. 1998), However, this assumed constancy, albeit the simplest to make in view of our ignorance of its sensitivity to substrate properties, must be strongly questioned as from a theoretical perspective CUE should vary with substrate quality (Manzoni et al. 2012). If CUE is constant in the five models analyses, this is not the case for the dispersion function, where in four of the models (not ROTH-C) the function looks like an alpine landscape. This is problematic because model predictions are also very sensitive to this function (Hyvönen et al. 1998). This is also one of the properties where empirical information is really scarce because of difficulties in measuring it. However, the question is if any of the dispersion functions in Figure 2 are reasonable or if we should expect them to be much smoother and probably monotonic functions? The manuscript by Luo et al. provides no further information on this point. In conclusion, the manuscript by Luo et al. points to a problematic area for the modelling of SOC. Better control on the internal consistency of models could help constraining model prediction by preventing unrealistic parameter combinations.

Response: Dr G. Ågren pointed out an important issue about the internal inconsistency of soil carbon models in terms of CUE and dispersion. This inconsistency mainly results from the difficulty of conceptualizing heterogeneous organic matter and their transformation using conceptual pools in the carbon models. There is in general a lack understanding on how to dynamically simulate CUE in the model. Thus, a constant CUE (different among models as studied by Dr G. Ågren) was assumed in most of models. In this study, we attempted to quantify the uncertainty induced by the variation in this constant CUE.

By studying five common models, Dr Ågren found that the dispersion function quantifying the transformation between carbon pools is markedly different among the models (Fig. 2 in his comments). Its potential effect on soil carbon projections certainly warrants further investigation. The APSIM model used in our current study shares the similar structure for simulating soil carbon dynamics with the models studied by Dr Ågren. In APSIM, the dispersion, i.e., the fraction of C lost from a typical pool transferring to other pools is pool-specific. In our study, we did not address the potential influence of this issue and used the default dispersion function in the APSIM model. One reason is that, as also mentioned by Dr Ågren, we did not have enough relevant information to constrain them. Another reason is that all soil carbon models use conceptual pools with different potential decomposition rates. These conceptual pools cannot be directly measured. This means that the dispersion function would be dependent on how we conceptualize the pools and derive their decomposition rates. However, we acknowledge the important of this issue raised by Dr Ågren. We expanded the relevant discussion on this topic (see lines 374-377) and cited the relevant reference recommended by Dr Ågren.

The purpose of our study was not to develop or improve our modelling capacity on CUE and dispersion, but to quantify the uncertainty in predictions, identify potential areas that should be improved, and assess how the uncertainty correlates to climatic and management conditions. We acknowledge that model development is certainly required to improve simulations of CUE and carbon pool transformation.

References:

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Hyvönen R., Ågren G.I., Bosatta E.: Predicting long-term soil carbon storage from short-term information. Soil Science Soc. Am. J., 62, 1000-1005, 1998.

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Parton, W.J., Ojima, D.S., Cole, C.V. and Schimel, D.S.: A general model for soil organic matter dynamics: sensitivity to litter chemistry, texture and management. Soil Sci. Soc. Am. Special publication, 39, 147-167, 1994.

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Figure captions Figure 1. Calculated carbon use efficiency e(q) = CUE as a function of quality q for the five models. Figure 2. Calculated dispersion matrix D(q,q') for the five models. q' represents the quality of origin and q the quality to which this carbon is converted. The sum of D(q,q') over q equals 1.

Response: We thank Dr Ågren for showing his case study and the detailed literature citiation. The relevant references in the above list were cited in the revised manuscript.

##

Comments from Dr Hatem Ibrahim (brahim_hatem@yahoo.fr)

This manuscript is well-written and thoughtfully prepared. Thanks for this nice study.

Response: Thanks for this comments.

I am agree that SOM models, initialization of the SOM pools can also be a major cause of divergent model projections, and I have some questions concerning "the future improvement in soil carbon modeling should focus on how microbial community and its carbon use efficiency change in response to environmental changes": 1- The author compare APSIM to other SOM models like RothC and Century, for this reason it is necessary to classify in the section materials and methods what kind of models are used (the model is linear, non linear, using a quadratic function. . . Pansu et al 2014).

Response: The APSIM model shares the similar structure with other models like RothC and Century (see the relevant description in lines 111-114).To avoid the potential misunderstanding, we modified the relevant model description in the revised manuscript. In terms of the model structure, we have clearly stated that "the decomposition of the soil organic carbon pools is treated as a first-order decay process…". First-order decay indeed shows the function type, which is commonly used in most of carbon models.

2- Current global models do not represent direct microbial control over decomposition "a new generation is required to capture fundamental microbial mechanisms without excessive mathematical complexity" (Todd-Brown et al, 2012), for this reason can future models (conceptual pools) be based on the functional ecology of soil microbial biomass (MB) which increases by assimilation of humic organic matter, fresh organic matter and decreases by microbial respiration and mortality?

Response: This is likely one of the directions for future model development. However, we don't think that better describing microbial biomass alone is enough. Incorporating microbial processes into carbon models needs our advanced understanding of microbial carbon and nutrient use efficiency, microbial stoichiometry, microbial physiology and their response to environmental and management changes. We emphasized issues related to microbial processes in several place of the manuscript (e.g., lines 314-317, 327-330, 363-367, 416-421).

3- Published references lack mechanistic predictions of the continuous transfers of C between plants, soil compartments and the atmosphere. We believe that it is because the functional role of micro-organisms was neglected in many models which focused mainly on total C stocks, rather than on transfers within the microbial and plant OC pools with varying stabilities. Although some models are appearing that take account of microbial activity (Allison et al. 2010; Pansu et al. 2004; Schimel and Weintraub, 2003), and quantify the microbial biomass (MB), (Xu et al., 2013), the influence of detritus on stability of ecological systems (Moore et al., 2004), and "the crucial roles of microorganisms in regulating soil carbon dynamics" (Jizhong Zhou et al. 2011). Author think introduce in the future research the determination MB-C (carbon microbial biomass) using for example the fumigation-extraction method? And demonstrate a direct microbial control over decomposition?

Response: Thanks for these suggestions. Mature techniques exist to determine microbial biomass. In this study, however, we did not describe the relevant methods much as our paper focuses on modelling and the relevant uncertainty. The second question is important, and relies on our understanding of the underlying microbial processes under different environmental conditions. The modelling results in our manuscript also emphasize the importance of microbial processes.