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Interactive comment on "The use of radiocarbon ^{14}C to constrain carbon dynamics in the soil module of the land surface model ORCHIDEE (SVN r5165)" *by* Marwa Tifafi et al.

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Answer to comments from the reviewer #2. We thank reviewer for the constructive evaluation of the manuscript. Please find below our answers to questions/comments.

Anonymous Referee #2 Received and published: 18 August 2018

This paper presents ORCHIDEE-SOM-14C, a new version of the IPSL-Land Surface Model, and tests it against data from four different sites. It makes an important contribution by implementing the isotopic tracer 14C in the model. This is a valuable addition to the ORCHIDEE-SOM model, which simulates depth-resolved soil carbon dynamics

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Discussion paper

from 0-2m below the surface. The authors also demonstrate how the new model can be used to constrain SOC turnover times and internal model processes. In particular, they implement two variations on the model ("Model_Test_He" and "Model_Test_Diffusion"). They follow the suggestions of He et al (2016) to slow turnover in the passive pool and reduce the flux from the slow to passive pool (pending comment by reviewer #1). They also implement a version of the model with depth-dependent bioturbation rate following Jagercikova et al (2014). Conceptually, this paper is a nice demonstration of how F14C data could be used for comparison against different model implementations. However, there are significant issues which should be addressed both with the implementation (see Reviewer #1 comments) and interpretation of the results (see below) prior to publication.

ANSWER: Thanks for the positive comments please see our answer to reviewer #1.

In its current form, this paper does not convincingly demonstrate that there are meaningful differences in the modeled profiles across sites, or that any differences reflect the modeled differences in climate, vegetation or soil properties. Figures 3 and 4 demonstrate that the model can broadly fit a generic soil profile. However, it is unclear if the model can reliably capture differences between sites (for example, in Fig 3, the model reasonably fits only two of the four profiles). Comparison to a somewhat larger number of published soil F14C profiles is needed to support current statements that the model can "reproduce soil organic carbon stocks and radiocarbon profiles" (for example, line 29). This additional analysis would significantly strengthen the paper. It would also be particularly interesting to see if the model is able to capture the wide differences in bulk soil 14C seen across soil taxa (for example as explored in Mathieu et al (2015)). Alternatively, if the authors feel that comparison to a wider suite of soil profiles is beyond the scope of the current work, the current model-data comparison should be rephrased as a proof-of-concept contribution. In either case, the discussion should address potential controls on the soil F14C profiles (for both data and model). For example, despite the important role of minerology and clay content in controlling the age of soil C, these

topics are not mentioned in the current discussion. Relatedly, more discussion and exploration of the model processes and parameters that control the 14C profiles would be an important addition to this paper. Although I acknowledge that comparison to a wider suite of soil profiles may be beyond the scope of the current work, I would like to see more exploration and discussion of these issues prior to publication.

ANSWER: We agree that such an evaluation would be a good step forwards but one the difficulty to run the model over such a large database is that very often some boundaries conditions of the model are missing and we have to estimate them with large scale database that may not be accurate for a given site. In this study we decided to carefully choose some sites which have enough data to feed the model and which are also representative of different situations. We aimed to go for such large-scale evaluation but we thought that it would have been more useful to have first a model description papers evaluated on well-chosen site. We changed several parts of the document in the revised version of the manuscript to explore more this weakness of our study see for example: “Nevertheless, the model evaluation performed here on only four sites should be considered as proof of concept and more in depth evaluation are needed, in particular using a large 14C database available at global scale (Balesdent et al., 2018; Mathieu et al., 2015). Indeed, the F14C is largely controlled by pedo-climatic conditions such as clay content, climate and mineralogy (Mathieu et al., 2015) and the range of situations we covered here is relatively limited.” or “Furthermore, here we used only one averaged value over the soil profile for soil boundary conditions (texture, pH, bulk density) but those variables are known to impact the F14C (Mathieu et al., 2015) and change with depth (Barré et al., 2009) and depth-varying boundary conditions may also help to improve the model.””

The authors make a good case for the addition of depth-varying parameters, both conceptually (eg line 69) and in the results, by making the important contribution of implementing He el al’s suggested parameters in a depth-dependent context and updating the diffusion formulation. However, although the updated diffusion formulation is a key

contribution of the paper, the impact of this model improvement should not be overstated, as the difference between the two different model profiles relative to the data is not large (fig 3 &4). The modest gains suggest that adding other depth-varying processes in the future could be valuable. Although implementation of depth-varying parameters is clearly important, diffusion alone is not a singular model fix, and the discussion and conclusion should be broadened where possible to reflect this (for example, “mainly for diffusion” in line 40 and 468 is misleading/overstated).

ANSWER: We agree with this statement and we add a paragraph in the conclusion to detail what should be the next step in the implementation of depth-varying parameters: “Here we presented the effect of a depth-varying diffusion constant but other parameters are depth dependent and should be represented in the next version of the model. For instance, belowground litter production in the model is simply represented by an exponential law without any representation of the effect of resource distribution on root profile (e.g. water or nutrients). This is a complex task in a land surface model aiming at running at large scale with a classical resolution of 0.5° but the soil modules of land surface models are quite sensitive to the NPP (Camino-Serrano et al., 2018; Todd-Brown et al., 2013) and a better constraint on the profile of the below ground litter production would probably improve the model performance.”

I agree with Reviewer #1 on the major technical issue presented. This should be corrected prior to publication. The contribution of implementing the He et al (2016) suggested parameters is a good idea, and a nice contribution to the paper, so I would suggest retaining this model fit after updating the values as suggested by reviewer #1. In general, figures could be made more professional, and a careful reading for grammatical errors is needed prior to publication.

ANSWER: The error was a typo mistake in the manuscript but we carefully checked the code and it was correct.

In summary, this manuscript should be considered for publication after major revisions,

including the technical fix presented by reviewer #1, model comparison to additional soil profiles, and/or an updated discussion of the results. Minor comments are listed below.

Specific comments: Line 40 & 468: “mainly for diffusion” is misleading as discussed above

ANSWER: This is removed in the revised version.

Lines 71-84: In introduction, cite other work using radiocarbon profiles to constrain soil models (e.g. Braakhekke et al, 2014; Ahrens et al, 2015)

ANSWER: We added the citation the papers suggested by the reviewer: “Different authors have already succesfully implemented radiocarbon in soil models and were able to clearly show that the introduction of pools with turnover time of thousands of year were unnecessarry to fit radiocarbon data (Ahrens et al., 2015) whereas Braakhekke et al., (2014) showed that after a reparameterization of the models based on radiocarbon data the prediction of their model was quite different with more carbon in top soil and less in deep soil compared to the model without radiocarbon.”

Line 136-137: Please clarify, as this seems contradictory: “SOC diffusion is actually a representation of bioturbation processes (animal (and plant) activity), whereas DOC diffuses through concentration gradients.” This text suggests that implementation of SOC diffusion would not be based on a concentration gradient, while the Fick’s law formulation provided (138-140) relies on a concentration gradient. Also, what do you mean by “the amount of carbon in the pool subject to transport”?

ANSWER: Both are based on a concentration gradient but the mechanisms we aimed to represent are different since it is bioturbation for the SOC whereas it is “real” diffusion for the DOC. We clarified the sentence: “SOC diffusion is actually a representation of bioturbation processes (animal (and plant) activity), whereas DOC relies more on a non-biological diffusion. Both diffuse through concentration gradients.”

Line 181...: 14C data collection: -Please clarify: was new data collected for this paper or is this published elsewhere?

ANSWER: More details are now given see answer below.

-Please include a table of 14C data values, including sampling depth increments

ANSWER: We added the table 5 to present those data in the revised version of the manuscript.

-Please provide more methods details on soil collection and processing or reference to appropriate publication.

ANSWER: For the French sites information can be found in Jagercikova, M., S. Cornu, D. Bourlès, O. Evrard, C. Hatté, and J. Balesdent (2017), Quantification of vertical solid matter transfers in soils during pedogenesis by a multi-tracer approach, J. Soils Sediments, 17(2), 408–422, doi:10.1007/s11368-016-1560-9. The information is now added. For the two other sites, data are not published yet so we added more details. See for instance for the Misiones sites: "Details on measurements and sampling can be found in Tifafi et al., in prep. Briefly, the soil was sampled in May 2015 at different depth: 0-5cm, 5-10cm, 10-15cm, 15-20cm, 20-30cm, 30-40cm, 40-50cm, 50-60cm, 60-80cm, 80-100cm. All sampled were crushed and air-dried. Once in the laboratory, they were homogenized, crushed, randomly subsampled and sieved at 200μm. Then 14C measurements were made using a new Compact Radiocarbon System called ECHoMICADAS (Environment, Climate, Human, Mini Carbon Dating System) following the recommendation of Tisnérat-Laborde et al., (2015)."

-How were litter and roots handed? Included/excluded? How does that correspond to model results? ANSWER: Roots were removed when visible. In the model we used only the active, slow and passive pools to calculate the F14C but as mentioned by reviewer #1 structural litter might have been included in the calculation. Nevertheless, structural litter in the model can be part of the litter produced during the on-going year

but can also be few years old. Fix a threshold to determine which part of the structural litter would have been included needs underlying assumptions difficult to test. We therefore considered that only the soil carbon pools must be included in the calculation.

Line 245-255: How are soil F14C values handled in the spinup? What is the potential influence on initial soil 14C values? Spinup is only âĹij2 half-lives of 14C and doesn't consider atmospheric variation prior to 1700.

ANSWER: F14C were considered as stable before 1700. We considered this is a reasonable assumption since the variations observed from 1700 are mainly anthropogenic. The initialization procedure may indeed impact the results. If needed, we can perform a sensitivity analysis to the initial F14C.

Line 301: Please mention somewhere how comparisons are made between data and model, given differences in depths

ANSWER: We added this information: "The intervals of soil depth of the model outputs and the measurements were homogenized by interpolating linearly the data to common depth intervals defined for each site. The simulations and data were then compared for each depth interval."

Line 309-313 & Table 3: Visually, and discussed in the text, the sites Misiones and Feucherolles appear to have quite good fits for total soil carbon, while the fit is the worst for Mons, and also poor for Kissoko. However, the correlation coefficients are highest for Mons, but lowest for Kissoko. Is this a meaningful metric?

ANSWER: The good correlation coefficient for Mons is due to the relative good representation of the shape of the profile even though the mean bias is quite important as it is shown in Fig. 4. To clarify this point we added few words on this aspect at: "The correlation coefficient for Mons is relatively high compared to other site (Table 3) whereas Fig. 3 shows that the model performance was not very good for this site. This is mainly due to a large SB whereas other MSD components were rather low."

Table 3&4: Is there a reason all values have been rounded to end in .05 or .00?

ANSWER: It was pure random and following the recommendation of reviewer #1 we change the units of the total carbon from kg C m-3in kg C m-2 and the values from Table 3 do not all finished by .00 or .05

Line 320-326/Fig 3: Any comments on why the model does so well in one French Luvisol (Feucherolles) and so poorly on the other (Mons) for total soil carbon? From the site description the sites sound very similar.

ANSWER: This model like all the models following a similar structure are quite sensitive to the litter production. For Mons a net primary production (NPP) of 6.7 t ha-1 yr-1 was estimated by the technical institute for pasture in this region of France based on the annual yields, whereas the model predicts a NPP of 7.5 t ha-1 yr-1. The large over estimation might be a consequence of a bias in NPP. As far as we know no NPP estimation is available for Feucherolles. We added this information: "For Mons a net primary production (NPP) of 6.7 t ha-1 yr-1 was estimated by the technical institute for pasture in this region of France based on the annual yields, whereas the model predicts a NPP of 7.5 t ha-1 yr-1. The large overestimation of the SOC stocks may therefore be due to an overestimation of the NPP."

Line 334: "The vertical profile of the SOC stock simulated was thereby globally not very far from that of the data". This seems like an overstatement based on results in Table 3. For example, although reported model total soil carbon is 1.7 and 2.1 overestimated at two sites with better fits, it is overestimated by a factor of 8.5 and 4.6 at the other two sites. ANSWER: We rephrase to avoid overstatement. See line: "The vertical profiles of the SOC stock were fairly represented by the model" Fig 3: Relatedly, what depth ranges are used for comparison between data and model? How does this influence the results? For example, model and data look quite similar in Fig 3 for Misiones and Feucherolles, but the mean total soil carbon is reported to be overestimated by nearly a factor of 2.

ANSWER: This information is now added in the method section “The intervals of soil depth of the model outputs and the measurements were homogenized by interpolating linearly the data to common depth intervals defined for each site. The simulations and data were then compared for each depth interval.”

Lines 364-366: Interesting, and nice to build on He et al (2016) using a depth-resolved approach

ANSWER: Thanks for the positive comments.

Line 392: More explanation of the results/implications of the priming effect mentioned here would be interesting, but not required

ANSWER: Since we did not run our model without priming we prefer to not increase the discussion section as it is to avoid over-interpretation.

Lines 407-408: “Using a fixed diffusion constant implicitly suggests that soil fauna activity is uniform over the entire soil profile”. Please add more explanation of the link between fauna activity and the diffusion term formulation for the reader. This diffusion term will vary with depth and across sites, because the Fick’s law formulation also relies on the concentration gradient with depth. For example, in Kissoko, for much of the profile there is almost no change in total soil carbon with depth, so the diffusion term here would be zero. Does that imply that there is no soil fauna activity? Or simply that soil fauna activity does not result in a change in the soil carbon profile?

ANSWER: Here we were wanted to talk about the diffusion rate and not the entire diffusion fluxes. We clarified the sentence: “Fick’s law of diffusion is classically used in models to represent bioturbation assuming that soil fauna activity may be represented following the Fick’s law of diffusion (Elzein and Balesdent, 1995; Guenet et al., 2013; Koven et al., 2013; O’Brien and Stout, 1978; Wynn et al., 2005). Using a fixed diffusion constant (D in eq. 2) implicitly suggests that soil fauna activity is uniform over the entire soil profile. This is generally the case of several models of diffusion especially used at

the level of an ecosystem (Bruun et al., 2007; Guimberteau et al., 2017; O'Brien and Stout, 1978). However, soil faunal activity vary naturally with depth and the diffusion constant should be depth-dependent (Jagercikova et al., 2014)."

Lines 449-454: Well-stated summary of model contributions

ANSWER: Thanks.

Line 457: Please mention and cite any other land surface models that incorporate soil 14C either here or in introduction.

ANSWER: We added a paper by Koven et al., 2013 in Biogeosciences.

Lines 466-468: "This suggests that, from now on, model improvements should mainly focus on a depth dependent parameterization, mainly for diffusion." Although diffusion did improve model results, the change was not dramatic. Please make sure the language used here reflects the results.

ANSWER: This was rephrased in the revised version

-Broadly, figure aesthetics should be updated to look more professional throughout prior to publication. For example: -Fig 7. Please label x & y axis. Please write depth increments for each bar on y-axis instead of 1-11. Also, in some of the panels numbers 11 and 12 are cutoff (eg 1..) -Fig 3-7: Use more professional titles and punctuation on figures (eg. rather than "Model_Control" , "Model_Test He", etc.) -Fig 7: It appears there are stray line numbers throughout the figures which will presumably be removed once the line numbers have been removed (eg fig 4,6,7) -Update "litter structural below" and "litter metabolic below" to more clear and professional names

ANSWER: All the figures have redo to more professional aspects.

-Fig 7 is instructive and interesting. However, what is the reason for the "litter structural below" to decrease then increase again at the deepest depths in some of the profiles?

ANSWER: The question might be that the diffusion constant D in deep layers has very

low values in deep soil because of the depth-varying equations we used. Therefore the diffusion fluxes are quite limited in deep layers. Furthermore, in deep soil the temperature is rather stable and those layers don't face important temperature increase in summer leading to high decomposition rates. Then, in deep soil the decomposition is limited and diffusion is not strong enough to homogenize the profile.

Language Comments: A careful and significant reading for grammatical errors and typos is needed prior to publication. A large number of very small changes are required.

ANSWER: All the grammatical errors were corrected and a native English speaker read the revised manuscript.

Here are a few examples (not comprehensive): Line 59: "simulate" should be "simulates" Line 71: typo "thIS" Lines 74-77: very confusingly worded sentence Line 81: "have" should be "has" Line 84:"because of the conceptual description by pools non measurable" – fix grammar Line 92: "yielded for the abrupt increase of atmospheric 14C concentration that doubles in 2-3 years." -clarify language Line 198: "Congo Republic" should be "Republic of Congo" Line 337: Missing period at end of sentence Lines 659-660: "over the profile according to total soil carbon" - Meaning is unclear

Additional references: Ahrens et al (2015). Contribution of sorption, DOC transport and microbial interactions to the 14C age of a soil organic carbon profile: Insights from a calibrated process model. Soil Biology and Biochemistry, 88. pp. 390-402. Braakhekke et al (2014). The use of radiocarbon to constrain current and future soil organic matter turnover and transport in a temperate forest. Journal of Geophysical Research: Biogeosciences, 119(3). Mathieu et al (2015). Deep soil carbon dynamics are driven more by soil type than by climate: a worldwide meta-analysis of radiocarbon profiles. Global Change Biology, 21. pp. 4278-4292.

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