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Geoscientific Model Development Discussions

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.

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 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 rates,

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.

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.

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 imple-

menting 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).

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.

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

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)

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"?

Line 181. . .: 14C data collection:

-Please clarify: was new data collected for this paper or is this published elsewhere?

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

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

-How were litter and roots handed? Included/excluded? How does that correspond to model results?

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 $\sim$2 half lives of 14C and doesn't consider atmospheric variation prior to 1700.

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

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?

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

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.

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.

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.

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

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

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?

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

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

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.

-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

-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?

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. 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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