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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.

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Answer to comments from the reviewer #1.

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

Anonymous Referee #1 Received and published: 14 June 2018

Improvement of the soil modules in global carbon cycle models is a recurrent need claimed by the scientific community. The land surface model Orchidee is one of the important tools to analyse and predict future changes of the Earth's climate and bio-

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Discussion paper

sphere. A recent study highlighted that current Earth system models predict a too young age for soil organic carbon. The present work introduces the radiocarbon isotope in the model to better constrain Orchidee. Based on the use of radiocarbon, the study furthermore improved the model itself, and model prediction, through a better representation of carbon movement within the soil profile. The article is fully relevant, clearly written and illustrated, and worth publication in GMD.

ANSWER: Thank you very much for the positive comments

But one point requires a significant change. Once this point fixed, the paper could be acceptable with only minor corrections. Important point. (parameterization of the 'Model_Test_He') Line 233 authors state "... multiply by 14 the turnover rate and by 0.07 the flux...". And later Line 236 and in Table 2: "decrease the flux from 0.07 to 0.049. Consistency would either decrease the flux from 0.07 to 0.0049, or multiply it by a factor 0.7. I suppose that the initial intention of authors was to multiply by 0.07 the flux, so that the steady state stock of passive would be kept similar (multiplied by 14 x 0.07 = 0.98), but with a F14 much lower. Here it seems from the results that the stock of the passive pool was multiplied by a factor almost 10 (less than 10 because of the duration of the spin-up), as expected by a factor x 0.7 for the flux. The over estimation of both carbon content and age is obviously expected with such a parameterization. In the present state, I further recomment not to use the name of a person in the surname of the model. Finally, I recommend that the authors either (i) remove this model_test from the paper, which would then be accepted with minor revison, or (ii) recalculate using a flux to passive 0.0049 instead of 0.049. Option (ii) is preferred, but is not mandatory, since the other parts bring significant results; note that option (i) would not affect the summary nor the conclusion.

ANSWER: Actually this was only a typo mistakes in the manuscript but we carefully checked the code and it was correct. We therefore corrected the manuscript.

Minor and typographical points. Table 1. Be clear in the legend on what was averaged.

Do "over the profiles" means a calculated mean for (0- 2.0 m)"?

ANSWER: We did not have data up to 2m so we calculated a mean for the different available layers that we applied to the entire profile of the model (0-2.0 m). We clarified the legend: “Table 1. General description of the studied sites. The mean bulk density, pH and clay fraction values calculated from the different soil layers depths available in the data were used as input for each site. For Mons and Feucherolles sites, min and max values of pH and clay fraction are provided between brackets”

Table 3 Data are in kg C m-3, which is a unit for local concentration, not for carbon stock. Is it: (a) kg C m-2, i.e., the carbon stock per unit area; or (b) the average concentration over the 0-2.0 m profile (then the Stock would be 2 times the mean concentration value)? Option (a) would be preferred.

ANSWER: The table 3 was modified and all the results are now presented in kg C m-2.

Line 786 Legend fig.6: indicate the variable in object (= F14C).

ANSWER: This is now added in the legend

Line 134. A brief statement of the formalism and parameterization of the priming would be welcome.

ANSWER: We modified the text in the revised version as following: “Briefly, priming is described following equation 1 with DOCrecycled being the unrespired DOC that is redistributed into the pool i considered for each soil layer z in g C m-2 days-1, kSOC being a SOC decomposition rate constant (days-1), and LOC being the stock of labile organic C defined as the sum of the C pools with a higher decomposition rate than the pool considered within each soil layer z. We therefore considered that for the active carbon pool LOC is the litter and DOC, but for the slow carbon pool LOC is the sum of the litter, DOC and so on. Finally, c is a parameter controlling the impact of the LOC pool on the SOC mineralization rate, i.e., the priming effect. The equation was parameterized based on soil incubations data and evaluated over litter manipulation

experiments (Guenet et al. 2016)."

Line 179. In Eq (6), STRUC was excluded of total 14C. Was it? In the Century model, STRUC usually accounts for 10- 20% of C in 0-20 cm layer, and is therefore non negligible (your figure 7). It is considered as retrieved as material < 2mm (for a large part) and therefore often included in the "measured" total carbon. This exclusion may affect the comparison between observed and modelled values of F14.

ANSWER: In the model, structural litter may come from leaves or root litter production of the ongoing year. Soil scientists, before measurements, generally remove it. We agreed with the reviewer that a part can still be present after few years but we were not able to clearly define a time step when structural litter is less than 2mm and therefore integrated in the measurements. To avoid overestimation of modern C we decided to not integrate the structural litter in the final calculation. If needed we can perform a sensitivity analysis adding or not the structural litter in the final calculation to estimate the impacts on our results.

Line 218. "OCC (wt/wt)" would be better than "OCC (wt %)"

ANSWER: Done

Line 232. "turnover rate" is an unclear term (might be the reciprocal of turnover time). Here turnover time?

ANSWER: Correction is done in the revised version of the manuscript.

Lines 314-326 and throughout: MSD values aren't in kg C m-3, but in kg2 C m-6 (variance not standard deviation); or use squareroot(MSC)

ANSWER: We corrected the MSD units in the revised version of the manuscript.

Line 325. Arenosols are not very specific and are broadly represented on the planet. Remove "for such specific conditions". Replace by "Probably due to an overestimation of decay rates by ORCHIDEE in sandy soils?

ANSWER: This is now corrected in the revised version of the manuscript.

Lines 367-401. See Major comment.

ANSWER: As explained above, the error was in the table but not in the model.

Do is the boundary condition at depth 2.0 for constant diffusion affect the base of the profils?

ANSWER: It is difficult to answer this question because changing the soil depth of the model would not only affect the carbon but also the hydrology, the plant uptake and in fine the carbon inputs.

Typography

ANSWER: All the typo mistakes are corrected in the revised version. Line 71. "this" Line 158. verify in the final edition the greek symbol delta (not ok in my pdf) Line 186. "1.5_m" (= separate the units throughout) Line 227. "(2016) (=no square brackets) Line 242. "et al. (2014)" (spaces) Line 255. Point missing; also lines 326, 337 ... check. Line 315: spaces before and after "=" (throughout the text) Line 447 'processes

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