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Interactive comment

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 #3

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The cycling of organic matter through soil ecosystems is highly simplified in land surface models. This is a major source of uncertainty in projections of the terrestrial carbon sink under global climate change. Measurements of the radioactive carbon isotope 14C provides a powerful constraint for soil carbon models which include a radiocarbon tracer component. This manuscript documents the addition of a radiocarbon tracer component into the ORCHIDEE land model in order to enable radiocarbon constraints in it and in the IPSL Earth System Model it is coupled with. This study then demonstrated applying this constraint to the model based on several vertically-resolved soil radiocarbon profiles.

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General comments:

The paper represents a substantial advance in the ORCHIDEE/IPSL model, which is an important tool in climate science, and has broader implications for other models. As such, it is well within the scope of GMD, and would represent a meaningful contribution to the field. However, there are several issues that would need to be addressed before I could recommend it for publication. I have detailed these issues below, and I hope that by addressing them, the authors will return with an improved presentation of this worthwhile research.

Major issue 1:

There are a couple of major issues with the Model_Test_He experiment. He *et al* (2006) suggested scaling the passive pool turnover time in IPSL/ORCHIDEE by 14, while scaling the slow-to-passive transfer coefficient by 0.07. I applaud the authors' effort to test this suggestion. However, the manuscript lacks a detailed explanation of exactly which quantities were scaled, and which of the arrows in Figure 1 corresponds with the first column of Table 2. The reduced complexity models of He *et al* consisted of three pools in series, whereas Figure 1 implies that ORCHIDEE has three soil pools that each independently exchange with a single pool of free DOC. Therefore, it seems that ORCHIDEE does not have a single transfer coefficient between slow and passive pools.

Furthermore, as pointed out in RC1, there seems to be an arithmetic error in the scaling of this transfer coefficient. The first and third rows of Table 2 imply that ORCHIDEE has some parameter with a value of 0.07 (this parameter being what needs improved explanation). Multiplying this by the scaling factor suggested in He *et al* would yield 0.0049, but it seems that 0.049 was used instead. The result is that the passive pool turnover time is increased by an order of magnitude without an equivalent adjustment to the inputs to this pool, leading to a large accumulation of radiocarbon-depleted SOM. This explains why the Model_Test_He experiment is so far off in Figures 3 and 5, and why the standard bias is so high in Figures 4 and 6.

I would encourage the authors to re-run this experiment with the correct values and keep it in the manuscript (and, unlike RC1, I have no problem with the name). I understand that the recommended values were for a previous version of IPSL/ORCHIDEE, and that some of the changes since then (yielding ORCHIDEE-SOM, detailed in Camino-Serano *et al*, 2017) make the recommended changes superfluous by accounting for priming. Nevertheless, I think that testing these recommendations is a worthwhile exercise, even with this updated model version, and I would be interested in seeing it done correctly.

Major issue 2:

There is insufficient explanation of the depths at which the observational (field) data were sampled, and how that was compared with the model output. Figure 1 explains sufficiently the depth of the soil layers in the ORCHIDEE model (though an explanation in the main text would be welcome as well). The depth of the field measurements can be seen in Figures 3 and 5, but not with enough resolution to really understand. Was each field profile sampled at the exact same depths as the layers in ORCHIDEE, or is there some interpolation going on between one or the other?

The statistics in Section 2.6 are all over a dimension i, which I assume to represent the layers over depth, but this is not clearly stated. Given the importance of this i, we need more detail as to what it is. I would prefer to see an additional table or additional information in Table 1 to indicate how many samples were taken at each site and at what depths. And, most importantly, some explanation in the methods of how layer depths were harmonized between the model and observations, including an indication of the size of i (i.e., the n in the equations of Section 2.6).

Moreover, the specific depths at which the observed and modeled layers are compared should be clearly visible in Figures 3 and 5. The field observations are shown as points, with a single depth. Were measurements taken just at those single depths? Or

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were entire layers sampled with an upper and lower boundary depth? The model is presumably providing an average concentration of carbon (and radiocarbon) for entire layers, but the lines in Figures 3 and 5 make it seem like the data are continuous rather than discrete.

Finally, the absence of explicit field data hinders the reproducibility of the study. The methods are described sufficiently to reproduce the study, and the model source code is available (though the web link has a problem, see below). But the study cannot be truly replicated without having access to the field data that were used. Including the field data in tabular format (perhaps as supplementary material) would go a long way toward making the methods more understandable and facilitating reproducibility.

Major issue 3:

THe authors provide some interpretation of each of the individual results in Section 3, but the manuscript lacks an overall discussion of the big-picture implications of these results and how they serve to advance scientific knowledge. The introduction section provides a compelling motivation for the study, but the manuscript lacks a sufficient discussion of how the current study informs these issues, what can be learned about SOM processes and soil-climate interactions, and what the implications are for the use of ESMs to project future climate change. I would like to see an expanded discussion of how these results fit in with the larger body of literature. The authors neglect to acknowledge that radiocarbon has already been implemented in a well known ESM (the Community Earth System Mode, CESM), and therefore do not discuss how their results relate to the existing work. The authors do cite the paper that would be relevant for this (Koven *et al*, 2013) in the context of diffusion representing bioturbation (line 406), but I would like to see an expanded discussion of how the results from the two papers potentially inform each other.

Minor issues and technical corrections:

Abbreviations: there are some abbreviations that are used without an explicit definition.

In some cases, they are defined later, but they should be defined in the first instance of use. I would avoid abbreviating SOC and SOM in the abstract, since neither one is used again in the abstract and just use the full text instead (but then define the abbreviation and begin using it when it first appears int he main body of the text). The abbreviation "F^{14}C" for fraction modern is used in the abstract, but not explicitly defined. "IPSL" is used several times before it is defined on line 105, and ORCHIDEE is never defined.

Line 71: spurious capitalization in the word "this"

Line 74: The sentence that begins on this line is too long, and should be broken up into at least two sentences to be understandable.

Line 75: "implementing" should be "to implement"

Lines 91-92: The decades should not have apostrophes (e.g., 1950s, not 1950's)

Line 93: Remove the word "since"

Line 94: Should be "As WITH any other carbon isotopes"

Lines 106–113: I am not sure how useful it is to list the names of the sub-components of ORCHIDEE without any further indication of how these components fit in to the present study. Instead, I would prefer to see a description of how ORCHIDEE fits into the larger ESM (e.g., which fluxes and state variables coupled it with the atmospheric model).

Line 158: There is some rendering issue with the δ (delta) symbol in δ^{13}C; please double check.

Line 162: The abbreviations Asample and Aref should be explicitly defined for the sake of the reader who may be new to the concepts of radiocarbon.

Lines 167–179: There is some inconsistency between the main text and the equations regarding abbreviations. The text uses "^{14}C" while the equations use "$carbon14$". I believe these are supposed to represent the same thing, and should therefore have the same abbreviations for clarity.

Lines 184–212: Some measurements include a space between the quantity and the units (e.g., "680 mm" on line 185) while others do not (e.g., "1.5m" on line 186)

Line 192: Define the abbreviation LSCE

Line 194: Define the abbreviation LMC14

Line 197: Define the abbreviation SOERE F-ORE-T

Line 232: The term "turnover rate" is ambiguous. I assume the authors mean "turnover time" since this is what He *et al* suggest should be scaled by 14, which would be the inverse of the decay "rate".

Line 252: What assumptions were made about the atmospheric ^{14}C content during spinup?

Line 256: Were simulations actually run at a yearly time step? Section 2.1 indicates that some model components have a much shorter time step. Also, for comparison with the field data, was the final (2011) time step used?

Lines 339–340: Something is wrong with this sentence grammatically, which makes it difficult to interpret.

Lines 392–393: The 50

Line 408: Remove the word "fact" or add the word "in" before it.

Line 465-466: Please revise this sentence for grammatical accuracy.

Line 477: The provided website address links to a page that has issues with the SSL certificate, and will not load in any web browser without having to make a security exception. Providing the link as http rather than https would fix this issue, though the preferred solution would be maintain the https link and insure that the website has a valid SSL certificate.

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