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Biogeosciences Discussions

Interactive comment on "Mineralisation, leaching and stabilisation of ^{13}C-labelled leaf and twig litter in a beech forest soil" *by* A. Kammer and F. Hagedorn

Anonymous Referee #2

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Studies like this, which do not only measure mass loss but quantify the fate of litter-derived C to the soil organic matter and the atmosphere, are needed for our understanding of forest floor litter decomposition to progress. However, despite this study has the big merit to have addressed most relevant litter decomposition questions, it suffers from several major flows that need to be addressed before it can be considered for publication.

I list below my major points of concern:

1. The authors use 13C depleted leaf and twig litter derived from a 4 years FACE experiment. Given the FACE experimental design (branch release of CO2) but most

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importantly the time of fumigation (4 years) and the fact that mature deciduous trees have very high C-reserves, it is most likely that the litter 13C signal was not homogeneous, especially for the twig litter (which would also explain why the 13C in the twigs was less depleted than in the leaves. Result that the authors do not comment at all!). Additionally, most of the C fluxes investigated refer to only about 30%, or less, of the litter C, and to the easily decomposable and soluble fraction. The likely lack of homogenous labeling and the fact that fluxes refer to a preferential group of C molecules, make not acceptable the mixing model applied (Eq. 2), which is based on the assumption that both litter types and SOC would behave equally with regards to discrimination during C mineralization (i.e. CO2 efflux) and DOC leaching. To me the only way for the authors to solve this problem is to: i) clearly acknowledge the likelihood of non homogeneous labeling; ii) measure the delta13C-CO2 evolved in laboratory incubation from leaves and twigs litter and use those as the end members in the mixing model to quantify flitter in respiration fluxes; iii) similarly, measure theïĂădelta13C-DOC from leaves and twig litter as extracted in the laboratory and use those in the mixing model for flitter in DOC fluxes.

2. Unfortunately, the authors did not use highly labeled (i.e. 13C enriched) litter and, therefore, their partitioning may be questionable. The raw isotope data are rarely given, but, as for example in the case of 13C in SOC, the authors mention an isotopic shift within the order of magnitude of the range of natural variation in delta13C SOC at the site (0.2 – 0.5‰ which is certainly too small to justify any attempt of C partitioning. Additionally, while the authors provide statistics (i.e. means and s.e.) on the results of mixing models, it is not described in the data analyses section how these were calculated. Once the authors have all the needed end members for their mixing models (see the comment above), they should apply the Phillips and Greg's (2001), spreadsheet (it is free from download http://www.epa.gov/wed/pages/models/stableIsotopes/isotopes.htm) to calculate the uncertainty on their f values. This would significantly strengthen the results presented and show when isotope data are adequate to trust source partitioning.

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3. Because of the weak label, the authors had to add lots of litter material to the extent that it reached most unrealistic litter C input values, in particular for the twigs input. I understand that the authors had to do it, to see an isotopic signal, yet they cannot use their mechanistic study to extrapolate results at the ecosystem level and quantitatively discuss litter contribution to C fluxes at the site. The best they can do with this experimental design is to discuss the effect of litter quality (i.e. twigs vs leaves) on C mineralization, DOC leaching and eventually fragmentation. Thus, all the sections on up scaling should be deleted (see specific points below).

4. A part from the very high input of twig litter (see above), the other high artifact of this study is that twigs were left to decompose in the absence of leaf litter. There is now a clear understanding that synergistic effects occur when litter decompose in mixture. This is always the case for twig litter which, as the authors state, at the site makes only 30% of the standing litter, the remaining being leaves. While this study provide interesting information on the decay patters of twigs, it does not tell us if the same would happen in the real world, where twigs decomposition occur within the standing leaf-litter layer. At best the authors need to acknowledge this important artifact of their study, justify it and discuss results accordingly.

5. Modeling of soil respiration is done on the sole basis of temperature. Is soil moisture at the site never below the threshold where it controls soil respiration (around 50% WC)? The authors either have to demonstrate that soil moisture never plays a role at the site, or apply a soil model that accounts for both temperature and soil moisture, as generally done when soil respiration from discrete measures is scaled up to annual fluxes.

Additional minor points:

P1044L4 Use the term “depleted’ rather than “labeled”.

P1048L12 “Root” should be better defined as “autotrophic” respiration. What is the root depth, were 30 cm enough to discard roots?

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PL1048L27 A side for the other referee's comment on the keeling plot approach which, I agree, done on only 2 points, and with small air samples (20 ml), may cause lack of accuracy in the estimation of the source ïĄď13C, I also find surprising that the authors did not use reference vials (i.e. filled with reference gas at the time of sampling) to estimate issues related to vials leaking of CO2 and CO2 adsorption/desorption from the septa, which are always an issue when using vials at atmospheric CO2 concentration and for isotope work. The authors should provide the made and type of vials and more details on vials testing (I assume they did test them prior to use!).

P1051L6 Wasn't Jenkinson who first provided the MB k factors? Anyway, even if the reference is correct, in both those studies k factors were calculated for soil extracts and not for litter. I would assume that extraction efficiency my significantly differ in litter (should it not be higher given the lack of mineral adsorption?) and the authors should not use these factors. At the top of my head, I do not recall studies on k factors for litters, but the authors should look for those and eventually use more appropriate k factors. But my suggestion, given that they never use the microbial data for C budgeting, is to simply present flush data. (i.e. fumigated – non fumigated, without multiplying for the extraction factor).

P1051L20 Please refer to the general comment above and apply a more correct mixing model to this study. Also refrain from using delta13C in this setting, this is a symbol used for isotopic discrimination between a source and a product which is not the case here, and calculated with a different formula. Thus, it is misleading here. Fig.6 First of all should be Fig.1 since it is the first to be discussed. Also, as it is I believe is misleading. I suggest the authors to present for the litter bags and isotopic approach, the C losses vs C remaining, on a 100% bases. This way it is made evident the difference between the two methods and the fact that CO2 losses measured by the isotopic approach equal C losses in litter bags, i.e. bags only measure C mineralization fluxes and limit C fluxes belowground. Also, for the isotope approach, the fraction not accounted for can be clearly stated.

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P1054L12-15; P1058-16-20 Remove (see general comment #3)

P1055L26-27 I do not understand this sentence.

P1060L20. Highly speculative. Rephrase with “On the basis of our results, we may hypothesis that . . ..”

P1061#4.3 Despite I believe that the authors are right here, this entire section is based on speculation, what if the isotope data were not accurate enough to close the budget, have you looked at the errors on f? Given that bioturbation was not measured, it seems to me too much to give it a full section in the discussion. The authors may keep their discussion but in a much more speculative framework and providing clear reference to the limit of their approach.

Interactive comment on Biogeosciences Discuss., 8, 1043, 2011.