

Interactive comment on “Variability of aboveground litter inputs alters soil physicochemical and biological processes: a meta-analysis of litterfall-manipulation experiments” by S. Xu et al.

Anonymous Referee #2

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General comments

This is a great topic and a meta-analysis is justified for it. The authors provide sufficient background to explain the rationale for this study, and they promote interest in the outcome. That said, there are several issues that must be addressed for this manuscript to be considered scientifically sound and well structured, and addressing these issues will necessitate substantial revision.

Specific comments

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1. The most important issue with the meta-analysis is that the authors use studies/papers as their samples and disregard the fact that multiple studies have been conducted at the same site. Using multiple values from the same site severely violates assumptions of the statistical analyses and biases the results. One example of this regards the authors' central conclusion that soil C accumulated to a greater degree in (sub)tropical forests compared to temperate ecosystems. However, Table 1 indicates that this conclusion is based on only 3 (sub)tropical values, and two of these values (as per the supplementary table) are from the same Costa Rican site (Nemergut et al/Leff et al). Thus, although this conclusion may be true, it is poorly supported in this study. To address this issue, my recommendation is to only use the most recent values from multiple studies at the same site and redo the analyses. (However, I believe there are more values available than used here, which may help increase sample sizes).
2. This is related to comment #1: overall, the sample sizes for many inter ecosystem comparisons are very small and make it difficult to be confident in the results. I recommend that the authors revise the text to focus on results concerning the across ecosystem analyses. Differences among ecosystems could serve as fodder for speculation in the discussion. Additionally, since mineral soil samples varied greatly in sampling depth, and sampling depth affects response ratios, it should be investigated whether differences in sampling depth contributed to differences between ecosystems.
3. The lengthy portion of the introduction regarding the non-significant soil C responses to CO₂ induced increases in NPP is interesting but not directly relevant to the study's results. The authors only weakly tie the results to this information and do not explain differences between these observations (no C responses vs. C responses) very well. Therefore, this section of the introduction should be trimmed substantially.
4. There should be a more complete discussion of additional factors (other than altered leaf litter inputs) that may have caused changes in C and other nutrient cycling following manipulations. For instance, the authors only briefly mention the contribution of roots to increased soil C. However, this could be an important effect as nutrients delivered via

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litter inputs could bait roots. The sentence on p. 5256, line 24-28 should be changed or removed since MBC could be responding to roots, and it does not argue that roots are not important for changes to C cycling.

5. P. 5261, lines 4-5 should be revised. I don't believe it is 'reasonable to conjecture...' that changes in litter inputs will necessarily lead to changes in C storage given enough time from the results presented here. The authors make the unsupported assumption that effect size can be substituted for effect time.

Technical considerations

P. 5248, ln 29: are often -> may be

P. 5249, ln 18: processes -> properties , ln 20: literatures -> literature

P. 5253, ln 15: Table S1 is referred to as Table A1 in supplement

P. 5256, ln 1-2: reference needed to support this sentence

P. 5257, ln 1: forest -> forests

P. 5258, ln 10: ecosystem -> ecosystems

P. 5260, ln 4: surprised -> surprising

Interactive comment on Biogeosciences Discuss., 10, 5245, 2013.