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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.

S. Xu et al.

lingli.liu@ibcas.ac.cn

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Referee 2: Comment: 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. 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. Us-

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Interactive Discussion

Discussion Paper



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Comment

ing 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).

Response: We agree with the referee's reservations about using multiple values from the same study and the issues of sample size in meta-analysis. We have amended our analysis accordingly to include only the most recent data from studies with multiple publications (Appendix B). This resulted in the exclusion of 3 studies, but the exclusion of 33 observations. However, a second literature search resulted in 5 additional independent studies being included in the analysis; hence the total number of studies increased (70), but the number of observations decreased (from 473 to 440). Furthermore, the additional data from different studies changed the number of observations for each ecosystem subgroup. As there is little consensus on the acceptable number of data points for meta-analyses, we followed the guidelines for systematic review by (Fu et al. 2011) and only performed analyses where there was a minimum of four data points (Table 1).

Comment: 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

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whether differences in sampling depth contributed to differences between ecosystems.

Response: We have revised the manuscript so that the results section focuses primarily on overall responses and only mention differences among ecosystems if two or more subgroups had sufficient data points to justify analysis (see previous response). Contrasting responses among ecosystems are more thoroughly explored in the discussion. We agree that soil depth will have an impact on the strength of the response and have taken this into account in our analyses. However, the majority of the data is derived from measurements taken at the soil surface (0-10 cm), where the largest responses would be expected; we have included a statement about soil depth in the discussion .

Comment: 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.

Response: We have omitted that section in the revised version of the manuscript and link the introduction to the discussion of the study's results.

Comment: 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 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.

Response: We have expanded this section to explain more thoroughly why we believe litter inputs to be the principle driver behind the observed results and to discuss changes in e.g. root biomass and root respiration.

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10, C2574–C2577, 2013

Interactive
Comment

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Interactive Discussion

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Comment: 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.

Response: We have removed this statement entirely.

Minor comments: P. 5248, In 29: are often -> may be - Corrected as suggested

P. 5249, In 18: processes -> properties , In 20: literatures -> literature - Corrected as suggested

P. 5253, In 15: Table S1 is referred to as Table A1 in supplement - Corrected as suggested

P. 5256, In 1-2: reference needed to support this sentence - Corrected as suggested

P. 5257, In 1: forest -> forests - Corrected as suggested

P. 5258, In 10: ecosystem -> ecosystems - Corrected as suggested

P. 5260, In 4: surprised -> surprising - Corrected as suggested

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

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Discussion Paper

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