Biogeosciences
Discussions

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Interactive comment on "Substrate quality alters microbial mineralization of added substrate and soil organic carbon" by S. Jagadamma et al.

S. Jagadamma et al.

jagadammas@ornl.gov

Referee 1

General comments:

COMMENT 1: This is a valuable and comprehensive study, addressing some key questions in soil organic matter cycling. The authors have conducted a thorough investigation with a range of soil types and amendments, including organic amendments that are not commonly studied. Some specific and technical comments follow.

RESPONSE 1: Thank you for your favorable comments.

Specific comments:

Introduction:

COMMENT 2: 4453, 13. It's not clear what leaf litter is an example of - a substrate that has not yet decomposed? Maybe cut.

RESPONSE 2: 'leaf litter' will be removed

COMMENT 3: 4453, 25. Maybe note that the work by Strickland et al is cited here as an exception to the statement, not a support to the statement?

RESPONSE 3: This statement will be modified to increase clarity. The revised text will read as: 'For example, Strickland et al. (2009b) also demonstrated the complementary action of both the input quality and the decomposer community composition on litter decomposition'.

COMMENT 4: 4456, 16. Perhaps don't use the term "long-term", just state the day. It's relative, right?

RESPONSE 4: Appreciate your suggestion, and the statement will be corrected accordingly.

COMMENT 5: 4456, 18. Clarify with "historical organic C inputs"?

RESPONSE 5: 'historical' is not part of this statement.

Results:

COMMENT 6: 4463, 14-18. Good discussion - maybe results not the best place for it, though – perhaps methods or discussion?

RESPONSE 6: It will be moved to the methods section.

COMMENT 7: 4463, 17. Why not include the archaeal data in the SI? Or, if it is not informative at all, maybe exclude it entirely from the manuscript, including the methods?

RESPONSE 7: Very good point. Thank you. Archaeal details will be completely removed from the manuscript.

COMMENT 8: A note on section 3.4. I am somewhat leery of the direct comparison of the modeling constants across pools. For example, could similar results be obtained by making the slow pool larger, but increasing its rate constant, vs. making the fast pool larger, but decreasing its rate constant? Figure 4 seems to roughly illustrate this trend – increasing pool 1 (A) is accompanied by decreasing k1(B). I.e., it is necessary to interpret the pool sizes and changes in rate constants together - simply noting that Pool 1 is smaller without explicitly stating how its associated k value changed does not necessarily allow the reader to make conclusions about the stability/lability of the C in that soil. An extreme example: if a given soil (X) contained C that is, on average, relatively recalcitrant, but still of widely varying stability, it could be modelled as having a very large pool 1, but this pool would have a low k constant. If compared to a soil with very labile C (Y), a small sub-fraction of which is especially labile, this soil may be assigned a very small pool 1 (representing this small, highly labile sub-fraction), but with a very high k. Considering only the pool size, the reader might conclude that X is more labile than Y (it has a bigger pool 1), but this would be incorrect. This seems particularly important since these pools are not expected to correspond to a "real" soil fraction. Perhaps, though, since, in the example of the Andisol, where the fast pool is smaller than in other soils AND the slow pool has a slower rate constant than in other soils, this point holds true - C cycling in the Andisol is generally slower. In any case, perhaps a careful discussion and justification of the statistical treatment of the rates alone while allowing the pool size to vary would be useful. Or, an alternative - what about holding one property constant (pool sizes or rate constants), and then allowing only the other to vary across addition types? Would that make sense? Or is there a good reference to support/justify the approach taken? Something to consider.

RESPONSE 8: We agree that the discussion and interpretation of pool sizes and rates could be improved. In response to your suggestion on interpreting the changes in pool sizes and rates together, we revised Fig. 5 by plotting the pool sizes and corresponding rates adjacent to each other (Revised Fig. 5 is attached as supplement). This will allow a straightforward and direct comparison of pool sizes and rates in tandem. We note

that the reviewer's concept, that pool size may be inversely related to rate constant, seems mostly true for the fast pool. In addition, statistical information was added in both Fig. 4 and Fig. 5 which will also help more meaningful comparison. We made an error in defining the Pool 2 in double pool model as intermediate pool. It should be defined as slow pool. Accordingly, double pool model includes fast pool and slow pool, and triple pool model includes fast, intermediate and slow pools. This correction was made in the revised Fig. 5. The manuscript will also be revised accordingly.

Discussion:

COMMENT 9: 4465, 21-25. I think this point would be stronger if supported only by fungal qPCR numbers, rather than the F:B ratio. For the reasons discussed by the authors earlier, could we not imagine a scenario where the bacterial community responds to substrate additions by shifting toward dominance of species with low 16S copy numbers, while fungi do not change, thus increasing the F:B ratio? While I do not necessarily think this is the case, this specific point (fungi respond to sugars) might be stronger if the authors just considered the fungal data. Would that be possible with this data? In general: the authors could probably go deeper into the findings with regard to how the different substrates affected native SOC mineralization rates in the different soils, with more speculation on why the effects vary from one soil to the next, and why, for example, they did not find that glucose additions increase SOC mineralization, as many previous studies have found.

RESPONSE 9: We used F:B ratio in order to simplify the interpretation of microbial community change due to substrate addition. Bacterial and fungal gene copy numbers were included in supplementary information (Fig. S2, Fig. S3). It is clear from these figures that the fungal numbers were considerably higher at day 4 in glucose added soils than the bacterial numbers. Therefore the increased F:B ratio at day 4 from glucose added soils were mainly due to the increased fungal gene copy numbers. This information will be explicitly added in the revised manuscript.

Technical comments:

COMMENT 10: 4453, 22. Likely mean "complementary" here

RESPONSE 10: Thanks. We will correct this typing error.

COMMENT 11: 4462, 15. typo - "combined"

RESPONSE 11: This will be corrected.

COMMENT 12: 4466, 17. "influences"/"could also influence"/"structures also influence"[: : :]"and the relative: : :"?

RESPONSE 12: This will be addressed. The revised sentence will read as: 'Along with other environmental and soil physico-chemical factors, microbial community structure could also influence the metabolism of C substrates in soil and the relative access by different groups of microbes'

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/11/C2338/2014/bgd-11-C2338-2014-supplement.pdf

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