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Biogeosciences Discussions

# *Interactive comment on* "Contributions of ectomycorrhizal fungal mats to forest soil respiration" *by* C. L. Phillips et al.

**C. L. Phillips et al.**

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Response to Referee 1, Erik Hobbie.

Referee comment: In this paper, Phillips et al. estimate the contribution of ectomycorrhizal (ECM) mats to forest soil respiration in Douglas-fir stands. The approach of using natural variability in mat density to assess mat properties such as respiration is a good one, as it avoids issues associated with experimental manipulations and lab studies. They estimate that 9% of total soil respiration is contributed by the ECM soil mats (primarily of Piloderma) that they studied.

Author reply: We appreciate that you consider this study a valuable contribution.

Referee comment: It would be worthwhile to know what other studies have reported



for this research site (HJ Andrews Experimental Forest in Oregon) for colonization of Douglas-fir – is it heavily dominated by Piloderma? Or does it have comparatively few root tips colonized, but extensive extraradical hyphal development? It is classified by R. Agerer (2006, Mycological Progress, 5: 67-107, Fungal relation-ships and structural identity of their ectomycorrhizae) as a short-distance exploration type, but possessing hydrophobic ectomycorrhizae, which is usually associated with more extensive extraradical hyphal development.

Author reply: We could not find a study comparing root and extradical abundance of Piloderma; however, these questions indicate you would like more discussion of Piloderma foraging strategies, to assess how the respiration measurements relate to potential carbon substrates. We have taken up the topic of mat foraging strategies in a new section added to the discussion.

Referee comment: This paper contributes to ongoing efforts to put the role of ectomycorrhizal fungi into an ecosystem context by providing quantitative estimates of their influence on various ecosystem-scale properties, and as such, will help to constrain recent modeling work that explicitly includes contributions from mycorrhizal fungi (e.g., Orwin K.H., Kirschbaum M.U.F., St. John M.G. & Dickie I.A. (2011). Organic nutrient uptake by mycorrhizal fungi enhances ecosystem carbon storage: a model-based assessment. Ecology Letters, 14, 493-502.)

Author reply: We appreciate the observation, and have added the citation above to discussion on the context of our study.

Referee comment: Chitinase activity correlated with soil respiration. It would be interesting to discuss why the slope of the log/log plot was not one, but 1.48. That is, at higher CO2 effluxes, there is increasingly (CO2ËĘ1.48) more enzymatic activity. Is this related to temperature?

Author reply: On a natural scale the relationship was not curvilinear, rather it was funnel shaped with a greater spread at high NAGase and respiration values (see natural scale figure attached). Analyzing these data on a log-log basis pulled in the spread and allowed us to apply linear regression tools. In my experience it is a common feature of respiration data that there is more variability in respiration (at least on an absolute basis) at high respiration values.

Referee comment: Although as the authors point out chitinase activity correlates with fungal biomass, it is not the most important fungal carbohydrate, with the proportion of fungal beta-glucans much higher than that of chitin, which is generally no more than 10% of fungal biomass, at least in sporocarps. Thus, it is somewhat misleading to stress the importance of "chitin" as a C and N source. Protein and beta-glucans are probably quantitatively more important as, respectively, fungal N and C sources. Thus (50/10), chitin may be more of an indicator of fungal-derived C and N resources than the actual "driver".

Author reply: We agree with Erik on the importance of beta-glucans (the building blocks of chitin) and proteins. We have addressed these points in an expanded portion of the discussion.

Referee comment: The CO2 sink recorded for the A horizon in non-mat areas at most of the six sampling dates indicated on Figure 7 (and also one date for mat areas) is puzzling. What is the explanation – diffusion of CO2 to shallower or deeper horizons? Loss in soil solution or during uptake by plant roots/mycorrhizal fungi (e.g., R. Teskey work, Aubrey D.P. & Teskey R.O. (2009). Root-derived CO2 efflux via xylem stream rivals soil CO2 efflux. New Phytologist, 184, 35-40,), hydraulic lift?

Author reply: As stated in the text and re-iterated by other reviewers, the uncertainty surrounding these partitioning estimates are large, due in particular to estimates of soil gas diffusivity. The apparent sink in the A horizon is not different from zero except on one fall sampling date. Possible explanations include that wetter soil in the O-horizon produced a high level of CO2 that diffused into deeper horizons. We have also added the possible explanation if increased CO2 storage and decreased permeability through



the overlaying horizon. The Teskey group has demonstrated apparently large xylem CO2 fluxes but, we are uncomfortable speculating on the magnitude of the effect in our system, as it has never been investigated.

Referee comment: On 1647/10, the authors suggest that aerobic respiration was repressed by moisture – are they suggesting that the environment went anaerobic? What about increased storage of CO2 in water – could that be quantitatively important?

Author reply: Yes, we believe increased CO2 storage (or decreased gas diffusivity) at higher moisture could certainly be important, and we have added the possible explanations to the text.

Referee comment: For Figure 8, could multiple or stepwise regressions of CO2 production vs. temperature and moisture be useful?

Author reply: We examined multiple regression with temperature and moisture, but it considerably muddled the trends. Some depths showed temperature and moisture interactions, others did not, but the temperature range is much greater in shallow the deep horizons. Figure 8 is this a simplified view of drivers, but its purpose is to pull out the main temporal pattern apparent in Fig 7 of more absolute production in deep horizons under low moisture.

Referee comment: On a related issue, the authors appear to hypothesize a shift from ECM respiration to free-living heterotrophic respiration (49/9) and cite the depth distribution of ECM fungi from Erland and Taylor (2002) as justification. This statement appears to confound absolute abundance with relative abundance. The abundance of microbes (including fungi) declines as C concentration declines in soil profiles. However, the relative importance of ECM fungi is less in surficial litter, more important in deeper organic horizons and upper mineral horizons, compared to saprotrophic fungi (Lindahl B.D., Ihrmark K., Boberg J., Trumbore S.E., Högberg P., Stenlid J. & Finlay R.D. (2007). Spatial separation of litter decomposition and mycorrhizal nitrogen uptake in a boreal forest. New Phytologist, 173, 611-620.).

Author reply: The referee makes an important point about relative versus absolute influence of EcM with depth. We agree that the relative importance of EcM fungi may not diminish with depth in some systems; though in our system the relative importance of Piloderma likely does diminish in the mineral horizon, since Piloderma mats are absent from the mineral horizon! We have rephrased to reflect the key point: that changes in vertical partitioning can impact the interpretation of surface fluxes. Depending on the specific system and the depth at which organisms tend to colonize, decreasing moisture and CO2 production in surficial horizons can either increase or diminish the apparent contributions of certain biota.

Referee comment: On Figure 5, the temperature profile appears somewhat quantizedâA ÌĘTËĞmaybe just my perception. Please give the r2 for the 3 lines.

Author response: The temperature profile does appear quantized, for the simple reason that we did not capture the brief shoulder seasons when soil temperatures were in the 5-7 degree range on any of our sampling campaigns. This gap is minor, however, and should not substantially effect the analysis.

We have added the values for the slope and significance; however, it is not customary to report R2 values for mixed-effects analysis as there is no single convention for reporting goodness-of-fit for fixed effects.

Referee comment: 38/25, 27, 28. Use of “incremental” may be confusing. The authors specifically mean (Mat – nonMat)/nonMat x 100%, I think. This should be clearly defined at some point, and use of “incremental” here carefully considered. It is vaguely defined in the legend of Figure 3.

Author response: We agree the use of “incremental” has confused several reviewers, and we removed it throughout, as well as providing a precise definition for “relative difference between mat and non-mat soil” in the methods.

Referee comments: For Figure 7, give the six dates sampled on graph. Figure 8 in



text is referred to as Figure 9. 37/13. Western (lc) 37/22. Substantially “to” total 38/2. Avoid “and/or”. Replace w/ or 38/24. Rewrite dangling participle. 40/7. Genus 42/5 & other lines. O-horizon, C-horizon, etc. should not be hyphenated. 46/10. Is “wet-up” accepted terminology – will non-native speakers understand this? 46/18. Give r2 for figure 6. 47/21. 66% on figure, 68% here. 48/25, 49/8. No comma. 50/2. “advantageously”? check meaning. 50/17. No hyphen. 50/20. Delete “nevertheless” References. Genus names need capitals. Also Douglas-fir, NIST.

Author response: We have made all the changes above, except for Western Oregon is the correct capitalization for a geographical region. We rephrased “wet-up” to “rewetting.” We removed “advantageously”, and corrected the discrepancy between the figure caption and text at 1647/21

Interactive comment on Biogeosciences Discuss., 9, 1635, 2012.

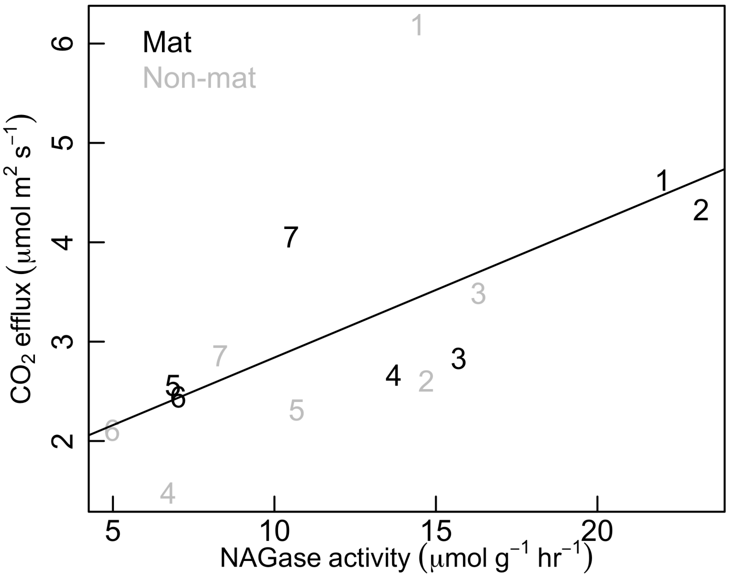
Mat
Non-mat
CO2 efflux (μmol m2 s−1)
NAGase activity (μmol g−1 hr−1)
2
3
4
5
6
5
10
15
20
1
2
3
4
5
6
7

**Fig. 1.**