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Interactive comment on “Contributions of ectomycorrhizal fungal mats to forest soil respiration” by C. L. Phillips et al.

Anonymous Referee #2

Received and published: 17 March 2012

The manuscript reports findings from a multi-year field campaign of measuring soil CO₂ efflux in a coniferous forest in the North-Western United States. The novelty in the approach is in the judicious placement of respiration sampling points, which allowed a pair-wise comparison of forest floor either with or without fungal mats formed by the ectomycorrhizal species *Piloderma*. This enabled a closer investigation of the flux contributions of such fungal mats, and the specific responses to environmental conditions.

The approach has a lot going for it, as it avoids the necessary artefacts of using exclusion and in-growth approaches, as outlined in the introduction. Measuring in situ from existing mats removes the uncertainty of whether fungal abundances have changed following e.g. trenching and mycorrhizal in-growth (e.g. using mesh approaches). The

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results illustrate that over an entire growing season, CO₂ flux from forest floor areas with dense hyphal mats show higher fluxes than areas without mats, whilst temporal dynamics suggest considerable sensitivity of the fungal flux contributions to soil moisture. What this approach assumes is that the occurrence of fungal mats at any place on the forest floor is random, i.e. that there is no underlying difference between mat and non-mat areas that determine the presence of the mat. I appreciate that these mats form over time, and may also recede from some areas, but underlying soil conditions may have an influence of whether a mat is found or not. This could at least in principle affect the validity of the approach, if underlying soil differences also affect soil CO₂ production, and should be discussed in more detail. The use of paired control areas in close proximity makes sense, as it removes larger scale spatial heterogeneity, but soil heterogeneity on the forest floor is considerable at small scales (certainly below 1 m), and I miss a critical evaluation of this aspect.

Notwithstanding this criticism, the results show interesting temporal dynamics (which are independent of the potential bias from paired comparisons). These dynamics are discussed in connection with soil abiotic conditions, particularly soil moisture. What has become quite clear in recent years is that root as well as mycorrhizal CO₂ flux is strongly substrate dependent, i.e. belowground allocation of recent photosynthetic C may influence the amount of CO₂ evolved from Piloderma mats as much as abiotic soil conditions. I appreciate that the experimental set-up does not include direct measurements of plant assimilation and/or belowground allocation of C, but this aspect should be included in the discussion. Soil moisture effects have clear impacts on plant assimilation fluxes, which may at least partly influence the observed flux responses.

The inclusion of chitin degrading enzymes in the soil is interesting, as it highlights the turnover of fungal biomass in soils. The interpretation of these results has to be done more carefully, however, as there is some circularity in the argument. Clearly, higher abundance of chitin degrading enzymes results from the presence of mats in the first place. To explain hyphal respiration rates by the amount of chitin degrading

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enzymes does not make sense, as both are a result of the presence of mats, i.e. they must correlate, but the causation between them is not necessarily there. ECM fungi have been linked to soil priming, i.e. the decomposition of organic matter for obtaining nutrients (see e.g. Cheng and Kuzyakov work, or papers by Subke on mycorrhical C supply and priming). Clearly, these organisms have the capacity to utilise autotroph-derived C in form of root exudations whilst decomposing (or facilitating decomposition) of soil organic matter. Authors refer to rhizospheric respiration, rather than autotrophic respiration, in order to make clear that there is a continuum between plant roots and the C they provide to the rhizosphere, and the associated organisms that metabolise this exuded c as well as soil organic matter. The claim that EcM are generally regarded as “autotrophic” (line 397) is too simplistic, and this section (396-404) should be carefully re-written to account for present knowledge of rhizosphere C transfer and priming.

Overall I think that the approach is interesting, and the estimate of temporal variations influxes as well as overall contributions are useful for comparison with other literature. As i point out above as well as in the detailed comments below, more work is required before the paper is publishable. The authors should discuss the relative merits of their approach more completely, and clarify several aspects (e.g. replication of profiles). Also the interpretation of results has to be done more carefully in places to avoid over-interpreting correlations as causally linked events (chitin degradation and respiration).

Detailed comments:

L. 17: I’m not sure I understand what an “incremental increase” is. Sounds like a double-nomer, i.e. any increase is incremental. Consider omitting the word ”incremental”.

64: Please update the Heinemeyer 2011 citation, which is now published as a full paper in Biogeosciences (in 2012).

89: I’m not sure you can assess causation with your approach. You can certainly obtain differences, and interpret them, but additional measurements would be required

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to establish causation.

91, 92 & 93: Delete “incremental”.

90-96: The research questions contain foregone conclusions – addressing questions 2 to 4 is only meaningful if the answer to question 1 is “yes”. As you clearly assume that there is a difference, I suggest you start with question 2.

104: Delete Zeglin reference, unless this is now in print.

130: No comma after 50

145 (and throughout manuscript): Leave a space between numbers and units.

160: What are the dimensions of “schedule 40” PVC pipe?

192-198: Please clarify how many such profiles you had. The error bars in Fig. 7 suggest true spatial replication for mat and non-mat locations – is that the case? Why aren't there error bars on data in Fig. 8, however? In case there was no spatial replication, how reliable are the estimates?

218-219: “Substrate quality” is a pretty ambiguous term, and not everyone would agree that C and N content sufficiently describe it (I for one wouldn't). I suggest you state simply that you determined %C and %N of soil cores.

246: Having an exponential moisture dependent is very unusual, and it is highly unlikely that soil CO₂ efflux increases exponentially across the entire soil moisture spectrum. Unless you can show that this is the best model (compared to possibly a linear moisture response, or, more realistically, a saturation curve), I don't see any justification for using it. As you present no results for the regressions described here, I wonder if it is necessary to include these. I don't see what the advantage of doing these regressions is compared to taking direct estimates of pair-wise differences for mat fluxes from your data, and possibly fitting a regression through these. Using the difference of two regression curves strictly requires you to include a propagation of errors associated with

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each regression to derive R_m , which you avoid when you use direct estimates for flux differences for your analysis.

281: “the mat” rather than “mat the”.

287-291: Delete the word “increment” throughout the section.

291: 8% of what: average mat respiration, or soil respiration? It would be more meaningful to give the increment in terms of absolute flux, not as a %age.

292-293: This is not shown. Figure 7 does not contain soil moisture, and none of the regressions in Fig. 8 are significant. Please remove this claim.

323: The p-value suggests no significant difference.

332-336: This calculation is highly skewed, as it bizarrely regards soils underneath tree trunks as not part of the ecosystem, and having no relevant soil CO₂ efflux. Surely the contribution of mats has to relate to total soil area, and if part of the soil surface is covered by fallen trees, then the proportion of mats is still responsible for the same amount of surface flux. Mat respiration has to be multiplied by its true areal extent, which is 42%, not 56%. Please correct this.

440: the references are incomplete.

560-565: Only include papers that have been accepted for publication.

Figure 5: The regression lines appear linear, despite your data analysis indicating exponential response functions. Please make this consistent. Explain regression lines in the legend, and give regression fits and functions.

Figure 6: What is the regression response? Is this a significant line? Figure 4 shows gravimetric O-horizon moisture of up to 300%, which doesn't match your x-axis here. Please clarify. Why is the logarithm of respiration shown, rather than respiration directly?

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Figure 8: Where regressions are not significant, you should not include lines (i.e. top two panels), as these suggests trends that have not truly been detected. For the O horizon at least, it would be useful to show mat and non-mat locations with different symbols (open vs. filled circles).

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

BGD

9, C365–C370, 2012

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