

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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Referee comment: This is an interesting manuscript, tackling an important issue of how to attempt defining the contribution of ECM to soil respiration and its environmental responses. I like the approach. However, I also share all of the previously raised concerns/comments with the other reviewers. Below I shall try to only contribute additional comments/issues. I am certainly looking forward to a publication of this manuscript, which I think shows important data, but should (and hopefully will) point towards an improved alternative methodology in a constructive way.

Author reply: We thank the referee for his or her encouragement!

Referee comment: 1637 L22 substantially to... L28 make clear that the 2007 study only

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refers to a seasonal study but the 2012 study to a long-term annual study. This also relates to your discussion (pay attention when you cite what percentage contribution values reflect what approach/method (ie seasonal fluxes vs. annual)).

Author reply: We have clarified the difference between the two studies in both the introduction and discussion as requested.

Referee comment: Abstract: why us 'when air filled ...was low ... why not just volumetric SM?

Author reply: We used air-filled pore space to stress the physical controls on soil gas diffusion; however, the extra verbage is not necessary and we have removed it.

Referee comment: Introduction: I think your sentence on that no mat soils also contain ECM fungi (non rhizomorph species) is important as it directly relates to interpreting your percentages (as those soils also include ECM SR, the overall ECM SR percentage will be > 9%. Maybe clarify this in the discussion when comparing to other mesh/collar studies. End: replace 'is associated' with 'does this equate to'.

Author reply: We agree it is critical to underscore that our measures of Piloderma mat respiration underestimate total mycorrhizal respiration, because non-mat soils are not devoid of fungi. We have re-iterated this point in an expanded discussion of study limitations.

Referee comment: M&M You also have AM fungi present (Thuja), so again, the mycorrhizal flux contribution seems tricky. Maybe discuss this again later?

Author reply: The goal of the study is clearly stated to quantify mat contributions, not all EcM contributions.

Referee comment: Soil insertion - as previously pointed out by the other reviewers, there is now an increasing evidence there that most studies seem to have 'cut off' a large proportion of Ra (so studies like Subke et al include those reduced fluxes).

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Author reply: We agree with the referee that soil collars are one of the more important limitations of the study. However, as stated in the methods our collars were very shallow (1cm). Also, we are also only aware of one study that has demonstrated reduced Ra from collars (Heinemeyer et al. 2011, European Journal of Soil Science). Are there others?

Referee comment: 1642 L5 replace name of forest with 'same forest'. Author reply: Assuming this refers to 1644 L7, we added the word "forest," but the referenced study covered many forests stands across numerous watersheds, so stating HJ Andrews is more correct than simply stating "forest", because it encompasses a much larger areas than the stand our study occupied.

Referee comment: 2.4 I also find the spatial heterogeneity question is important to address (soil moisture and throughfall will vary a lot and maybe that explains where you find matts? Any ideas?).

Author reply: We have dedicated a section of the study to spatial heterogeneity (see sections 2.5 and 3.3), but found no significant differences between mat and non-mat areas. Perhaps these results were missed by this and other referees because they are unsatisfying! We have also taken up the topic of where mats are likely to occur based on other literature in the discussion.

Referee comment: What temp probe was used what time interval? What diameter soil corer was used?

Author reply: We have added the specifics that were missing. The time interval of temperature measurements was already specified: simultaneous with respiration measurements, which were monthly. (Additionally, continuous hourly temperature measurements with buried Type-E thermocouples were made in the vertical partitioning profiles, see Fig. 2.) The diameter of the soil corer was also already specified: 8cm.

Referee comment: Diffusivity is dependent on soil moisture, so this flux approach

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is very uncertain indeed (for certain soils). Maybe pickup on this in the discussion? Maybe a 'GasSnake' approach of directly measuring fluxes with semipermeable tubes will be a possible way to get better in situ data without this uncertainty (see AgrForMet Heinemeyer et al 2012)?

Author reply: We agree with the referee that diffusivity estimates are very uncertain, and membrane-based flux measurements at the surface and in the subsurface would be a wonderful opportunity for future improvement. We have addressed these points in an expanded discussion of the vertical partitioning results.

Referee comment: 2.5 Was living root biomass determined or all and how (scanner, grid line intercept?)?

Author reply: We have added text to section 2.5 to explain that dead and live roots were not separated, and root mass was determined by dry weight, corrected for non-combustibles.

Referee comment: 1646 L11/11 I prefer to have it the other way round ie what (y) depends on what (x).

Author reply: With apologies, we were unable to make sense of this comment and do not see what it refers to at 1646 L11

Referee comment: 2.6 again, explain in Fig legends if you really used expt or linear regressions - I am confused.

Author reply: This appears to have been a common point of confusion for all the reviewers, and we hope the revisions in the methods and figure caption clarify our statistical approach.

Referee comment: 1647 L9 relative or absolute increase?

Author reply: Both! We have clarified this in the text, and show in Fig. 7.

Referee comments: 3.4 Here the Heinemeyer 2007 study is seasonal. L20 do you

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mean soil types here? I guess the soil type is the same at close proximity but the horizons might be different (humus type etc.). Reconsider. 4 L23... maybe this is where the methodology of diffusion tubes (GasSnake) might make a difference as the here calculated diffusion makes lots of likely uncertain diffusion assumptions. 1651 L2 Sorry, but how did you derive the 40% (I might have missed this). L10 Again, pay attention that this study was seasonal at the main ECM activity (i.e. fruiting body season). Then also include the annual estimates of the 2012 publications (BG) which show also large peaks but overall much less contribution. Fig 6 p-value; r²? Fig 8 r²? Fig 7 maybe the negative values are a reflection of the uncertainty due to the estimation of the diffusion coefficient? (soil moisture and porosity changes)...discuss?!

Author reply: We have included the specific mycorrhizal respiration estimates from the two Heinemeyer studies. We also mention the Gas Snake approach for avoiding collars, as well as for obtaining subsurface fluxes. We have taken up the discussion of diffusion coefficient uncertainty, which we agree is large. We have provided p-values for slopes in in Figs 5 and 6, but it is not customary to report R² for mixed effects analyses (e.g. see , but only significance of the fixed effects. E.g. for discussion of goodness-of-fit for fixed effects models see here: <https://stat.ethz.ch/pipermail/r-sig-mixed-models/2010q1/003375.html>

Referee comment: Fig. 9 I would prefer it the other way round (x axis should be y).
Author reply: Done.

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