

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

**Anonymous Referee #4**

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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.

(I would prefer to see continuous line numbers to refer to any issues but do not seem to see those as one of the previous reviewers?)

1637 L22 substantially to... L28 make clear that the 2007 study only refers to a seasonal study but the 2012 study to a long-term annual study. This also relates to your

C423

discussion (pay attention when you cite what percentage contribution values reflect what approach/method (ie seasonal fluxes vs. annual).

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

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'.

M&M You also have AM fungi present (Thuja), so again, the mycorrhizal flux contribution seems tricky. Maybe discuss this again later? 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). 1642 L5 replace name of forest with 'same forest'.

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 mats? Any ideas?). What temp probe was used what time interval? What diameter soil corer was used? Diffusivity is dependent on soil moisture, so this flux approach 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)?

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

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

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

1647 L9 relative or absolute increase?

C424

3.4 Here the Heinemeyer 2007 study is seasonal. L20 do you 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; r<sup>2</sup>?

Fig 8 r<sup>2</sup>?

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?!

Fig. 9 I would prefer it the other way round (x axis should be y).

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