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

Anonymous Referee #3

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This study estimates the contribution of ectomycorrhizal (ECM) mats to forest soil respiration in a Douglas-fir stand by using natural spatial variability. Previous studies have focused on partitioning of soil respiration mainly on the heterotrophic and autotrophic components but few have ventured to quantify the contribution of mycorrhizae (e.g., Heinemeyer et al 2007). Furthermore, this study adds on our understanding of the role and controls of ECM to CO₂ fluxes at the ecosystem level (e.g., Heinemeyer et al 2012 Biogeosciences, Orwin et al 2011 Ecology Letters, Vargas et al 2010 New Phytologist). I agree with most of the comments from two reviewers previously posted in BGD so I will keep my comments brief.

The authors ask one overarching question: (#1) Is there an incremental increase in soil surface CO₂ flux from Piloderma mats compared with non-mat soil?, and follow up their

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research with 3 complementary questions. Questions #2 and 3 relate to the biophysical controls of mat respiration and the last one is related to upscaling the contribution of ECM to the stand level. I believe that this study would benefit from hypotheses linked to these questions. For example: why there should be (or not) differences in soil surface CO₂ efflux from ECM mats? Which would be the expected relationships between ECM respiration with soil moisture and temperature? Why and how ECM respiration would be related to root biomass, soil physical properties or soil enzyme activities?

The authors explain that a 40 PVC pipe (need to describe what is a 40 PVC pipe) was pushed about 1 cm into the organic horizon and that potential of severing of roots or hyphae appeared to be minimal. This situation may be specific of their study site where there is a thick and soft O-horizon but may not be the case for many sites (see Heinemeyer et al 2011) and may warrant discussion for replication of the study in other ecosystems. The authors partitioned CO₂ production at the site using Fick's first law of diffusion and calculate the effective CO₂ diffusivity in the soil as described by Moldrup et al (1999). To the best of my knowledge the description of D_s by Moldrup is for undisturbed mineral soils and it would be hard to implement to model D_s on an organic soil horizon. Thus, the authors calculated the production of this horizon as the difference between the surface efflux and the incoming flux from the A-horizon. This approach is somewhat a black box as there is no certainty that this method is equivalent to the surface flux measured by the chamber method. Although there may not be another better way to test for this it will be important to discuss the limitations to measure soil CO₂ production deeper in the mineral soil and within organic horizons. This is partially addressed by using the Monte Carlo simulations to propagate uncertainties for production from each horizon, but the use of this technique should be explained for the general public. In other words, why the authors used this approach and how this helped to evaluate the uncertainty on their measurements.

Why the authors use chitinase activity as a driver for ECM respiration, and why it was expected a ln:ln relationship? I agree with a previous reviewer that it is not the most

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important fungal carbohydrate. Was it the “best” approach to test fungal activity and relate it with ECM respiration? Could it be a better indicator, or the relationship is simply overshadowed by stronger drivers such as soil temperature and soil moisture? Thus, a section about limitations of the study would benefit the interpretation of the results. This section should include issues such as small sample size, limited sampling period, production of CO₂ in the soil profile, chitinase activity and up scaling challenges for this study.

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

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