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Discussions

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

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Referee comment: This study estimates the contribution of ectomycorrhizal (ECM) mats to forest soil res- piration 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 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?

Author reply: We appreciate that the reviewer thinks our study adds to understanding of the role of EcM fungi.

We appreciate that the referee wants more explanation, a priori, of expected outcomes; however, prior to online posting in BGD we were asked to remove some explanatory text that the editor felt belonged in the discussion. We are pleased to take up these topics in the discussion and compare our observations with expected results, and have done so. We fail to see a way to provide these detailed hypotheses in the introduction, along with our research questions, without making the introduction overly detailed. We feel the premise of the study is conceptually straight-forward, and therefore the introduction should be kept brief.

Referee comment: 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 think 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 FickâĂŹs 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 Ds by Moldrup is for undis- turbed mineral soils and it would be hard to implement to model Ds 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 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 overshadow 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, chiniase activity and up scaling challenges for this study.

Author reply: We have added a discussion section on limitations of the experimental approach, including these specific points about potential hyphal severing and the shortcomings of vertical partitioning techniques. We have also addressed the concerns about the chitinase correlation in the discussion. Chitinase has been shown previously to differ between mat and non-mat soils, and chitinase was measured here simply to assess whether it varied with another measure of mat activity (respiration). Chitinase is produced by fungi and bacteria, and thus it was not intended to test for respiration by fungi explicitly. As stated previously, and now described in the figure caption, a ln-ln relationship was not expected a priori, but this transformation was used

to address unequal variance to meet assumptions for linear regression.

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