Biogeosciences Discuss., 8, C922–C924, 2011
www.biogeosciences-discuss.net/8/C922/2011/

Biogeosciences
Discussions

Interactive comment on "Exploring the "overflow tap" theory: linking forest soil CO_2 fluxes and individual mycorrhizosphere components to photosynthesis" *by* A. Heinemeyer et al.

Anonymous Referee #3

Received and published: 12 May 2011

The authors present an approximately 3-year time series of total soil respiration and component fluxes, as determined through selective exclusion of roots and/or mycorrhizae. I agree with the authors that few others have published such data, and despite the unavoidable limitations of physical partitioning methods they are an important contribution that can help to constrain, if not precisely quantify, components of soil respiration. All soil flux partitioning techniques have serious limitations, and the long duration and high temporal density of this dataset make them very valuable.

I think the biggest strength of this manuscript is the raw dataset (i.e. objectives i and ii), whereas the authors' analysis of the environmental drivers is less polished (i.e.

objectives iii and iv). I think an important use of the paper will be in providing data for forest carbon models, as there are few field measurements of mycorrhizal respiration currently available for modeling purposes, and the presentation of the results in tables provides numbers readily useable for that purpose.

As for the interpretation of environmental drivers, I had several frustrations. 1) There were no articulated hypotheses in the introduction about potential effects of these drivers, so the results and discussion tended to ramble and were not sufficiently structured. It seems to me that Figures 1 -7 could be condensed into a single figure containing 4a, 4b, and 5b. There did not seem to be a justification for showing daily, monthly, and annual averages (especially when there are also seasonal avgs in Table 5). There does not appear to be any particularly compelling dynamics apparent at one timescale that are less apparent at another. I agree with the other reviewer that it is odd to state the overflow tap theory in the title, as it is not articulated in the intro, nor addressed in the results.

I also agree with the other reviewer that the presentation of Q10 values was questionable. The authors acknowledge some of the limitations of Q10s, (e.g. citing the Davidson 2008 and Subke & Bahn 2010 papers); however, they also present extensive Q10 results and place emphasis on these results in sections 4.2 and 5. I could not make sense of the first 3 sentences of 4.2, and suggest rewording. The authors do not mention the errors in Q10 calcs that can be caused by hysteresis (e.g. as described in Subke and Bahn 2010), and since the magnitude of hysteresis has been linked to soil moisture (e.g. Phillips C.L. et al Glob Change Biol 2011, Phillips S. et al JGR 2010, and several others) the unavoidable differences in soil moisture between treatments make comparisons of Q10 values questionable. Since some of this manuscript's authors have previously published on the limitations of Q10s, I think it is especially important to address these issues here and demonstrate the validity of using Q10s for this particular study.

Unfortunately, I disagree with the first reviewer that the wavelet results are "compelling."

As I understand, the purpose of this analysis was to show that after accounting for temperature, there is remaining correlation between respiration and GPP (and to demonstrate the timescale of the residual coherence). However, because GPP and temperature are not only correlated themselves, but also oscillate at similar frequencies (daily and seasonal), I'm not convinced the wavelet analysis can provide meaningful separation of these 2 potential drivers. I think it is overstated in section 2.6 that wavelet analysis was applied on the "temperature independent time series" of hourly fluxes. The normalization process involved fitting a Q10 function, which has many problems including: diel hysteresis (putting a single line through a loop), temporal autocorrelation (the influence of temperature may be smaller if autocorrelation is accounted for), and multicollinearity (photosynthetic carbon supply as well as temperature influence the respiration response). If there were compelling differences between panels A-C in Figure 9, then I would suggest that the analysis be included and these shortcomings simply discussed. However, I don't see strong differences between the flux components that overcome the methodological limitations.

Overall, I would suggest streamlining this paper before publication. I would suggest proposing the overflow tap theory upfront, and presenting the results and discussion in a way that highlights evidence for and against the theory. I believe it is not necessary to emphasize Q10 values in order to test this hypothesis. Being able to use these data for modeling is an important outcome, and respiration vs temperature relationships are important for many models; however, I believe it would be more helpful to show/discuss respiration vs temperature plots and the form of these relationships, rather than present Q10 values with reservations.

Interactive comment on Biogeosciences Discuss., 8, 3155, 2011.