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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.

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"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."

Response: We appreciate the supportive comments to our study.

"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."

Response: Again, we appreciate the positive comments and agree with the strength of the presented dataset. Specifically, we agree with the reviewer that the presented data in the tables are necessary and a straight forward way to enable other researchers (e.g. modellers) to make use of this dataset. We would like to see this as a supporting argument in light of the previous reviewer's comments on reducing the number of tables and Figs.

"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."

Response: We have revised the manuscript and streamlined it and paid particular attention to the formulation of the research hypotheses and the associated discussion. Particularly, we have streamlined the discussion to link it to the logical flow of the hypotheses and the main conclusions and now include a section in the conclusions, 'limitations and future research considerations and applications', which provides a much needed space for some 'loose ends' and also follows the previous reviewer's request to critically comment on our chosen methodology. However, we do see great difficulties in merging all figures into one whilst still allowing a clear overview of the dataset, as pointed out by Rev#1. We feel that the Fig. panels in their current form offer a clear and logic framework for the data presentation (e.g. linking fluxes to climate and showing different temporal flux patterns); we have made some suggestion for improving readability in our comments to Rev#1.

"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)."

Response: This comment is contradictory to Rev#2, who asked for the averages; we interpret this as a good reason to include both: the readership is divers and both averages and time series are needed for interpretation. We feel the seasonal averages are an important but yet mostly understudied issue in carbon dynamics – we need a better link to phenology, looking too closely (i.e. at high temporal resolution data) might lead to missing the bigger picture, particularly when comparing seasons across different years in relation to forest C dynamics. There is also a need to show these different time scales as several of the key forest carbon models operate at either hourly, daily or monthly time steps, thus the Figs. add not only better temporal understanding, but also directly relate to different model frameworks.

"There does not appear to be any particularly compelling dynamics apparent at one timescale that are less apparent at another."

Response: It is not clear to us what the reviewer is talking about specifically, but maybe it is figure 5 or table 2? We feel that in fact there are very different dynamics at different timescales as shown in the rest of the figures. Not only do the ranges of fluxes vary depending on the timescale (which is important to relate site fluxes to other published literature) but also their dynamics (i.e. when does root respiration etc. 'kick in' in relation to GPP or climatic events and how constant are the fluxes at different time steps?).

"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."

Response: We appreciate this comment, which we have addressed by revising the introduction and discussion section as outlined in the above comments to Rev#2 and

in relation to the highlighting of supporting data for the hypothesis in the discussion as requested below.

"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."

Response: We do feel that it is important to include a basic Q10 analysis, firstly, to underline the argument of the paper of different environmental responses of the component fluxes across the seasons, and secondly, to support the necessity to link autotrophic fluxes to GPP and the subsequent explanations and discussion around the 'overflow-tap theory via the mycorrhizal symbiosis. However, we agree with the suggestion that it is too extensive; we have therefore downscaled the discussion on this issue. Moreover, for a general understanding we feel that a temperature response summary as part of one table is not too extensive but serves an important purpose; it relates to the WCA (i.e. similar Q10 used for de-trending) as it shows the overall effect of the de-trending (and relates to the interpretation of the WCA results as pointed out by Rev#1) and is overall an important way of showing a wide readership what goes on in relation to temperature and what and when not (seasonal changes). Notwithstanding this argument we now include an important reference to this issue in the conclusions part ‘future considerations’, Phillips et al., GCB 2011, and also considerably shortened and reworded the relevant Q10 discussion.

"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."

Response: The issue is that all time series of soil fluxes and canopy fluxes were de-trended for temperature. Therefore we tried to remove as much of the oscillations which are explained by temperature in all time series (see comments above to Rev#2). Thus the variation that is left (i.e. residuals) is not explained by changes in temperature and should be explained by something else. When comparing the de-trended time series of, for example, total SR and GPP any temporal correlation between them is therefore assumed to be a predominant link between photosynthesis and SR. However, other factors unknown to us could also affect both components, which would require further specific research. We have clarified the corresponding sections in the manuscript (M&M) and also in the new section in the conclusion to the discussion (limitations and future research considerations and applications) as outlined previously (Rev#2); we now also quote a new reference that is in press about this topic (Vargas et al., NP, 2011).

"I think it is overstated in section 2.6 that wavelet analysis was applied on the “temperature independent time series” of hourly fluxes."

Response: We agree that technically speaking this is incorrect and have therefore changed the wording to time series ‘de-trended’ for temperature.

"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 multi-collinearity (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."

Response: The reviewer is correct on the issues of calculating a Q10, as we have clarified in response to reviewers 1 and 2. Also the reviewer is correct that there appear not to be many obvious differences in panels A-C, therefore we included the enlarged Fig. 10 which shows the differences for the 1-4 day periods in more detail. There are clearly several periods of either exclusive Rr or Rm correlations. The strength of the WCA is to link two time series and identify temporal correlations even when there is small power between them. In other words within the time series in panels A-C there is small power (not significant in these graphs) at other timescales, but only with the coherence wavelet analysis one can highlight those temporal correlations. The limitations on how to de-trend for temperature are known and the reviewer is correct and we now acknowledge this in the “limitations and future applications” section alongside the added reference Phillips et al., GCB, 2011 regarding the SR flux temperature responses and Q10.

"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."

Response: We acknowledge the recommendation to keep the 'overflow-tap' theory as a central aspect of this paper through better linking introduction and the remainder of the manuscript to this topic and the associated hypotheses. We have followed this advice in the revised manuscript, but rather than introducing the 'overflow-tap' theory upfront, we lead to it by outlining the background and related work first. Overall, the structure of the manuscript sections (results and discussion) follows the outlined main aims (to which the now included hypotheses relate) as stated at the end of the introduction. We agree that the presentation of the apparent Q10 values has clear limitations (as stated in the manuscript), yet we feel they add an important angle to the overall understanding and interpretation of the fluxes for this site and the WCA. However, we now focus the discussion more on the rest of the results to highlight the concluding remarks (i.e. the theory) and have down-scaled the interpretation of the Q10 results. However, we do not think the Q10 data need to be shown in more graphical detail in this publication; this would be an entirely new manuscript and we plan to use the presented data as part of a further temperature-focused analysis along the lines of the Phillips et al., GCB 2011 publication. We feel the manuscript has thus been greatly improved and streamlined as requested and are grateful to the reviewer's comments; as recommended we provide arguments in support of the hypothesis throughout and see the issues related to limitations and future research considerations as addressing some of the arguments questioning our hypothesis, mainly due to outstanding research.

Reference:

C.L. Phillips, N. Nickerson, D. Risk & B.J. Bond (2011) Interpreting diel hysteresis between soil respiration and temperature Global Change Biology, 17, 515-527.

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