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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 manuscript submitted by Heinemeyer and coll. addresses relevant scientific questions that are within the scope of Biogeosciences. The introduced a rather new theory that they call "overflow tap". However, it is more a hypothesis that is nicely discussed but this should be removed from the title that should be more "humble". We are scientists, not sellers! Similarly, the repeated use of "the first time" or "unique" in the discussion is somewhat excessive and irritat!"

Response: We would like to advocate to keep the "overflow tap" mentioned in the title and feel that the actual title reflects a cautionary approach as we deliberately choose

the word 'exploring'. In our previous paper, i.e. Heinemeyer et al., GCB, 2007, we observed this issue and the goal of the current paper is to provide a deeper understanding of this formulated hypothesis and delivers first field-based data that relate to this hypothesis. However, we acknowledge that we did not introduce the concept and hypotheses in the introduction appropriately, as was also pointed out by Rev#3. We have now revised the introduction accordingly. Moreover, we believe that a title may reflect the novelty of studies and here it goes beyond just measuring fluxes to obtain a long-term dataset. This project was set out to address the linkage between canopy and soil fluxes and specifically its relation to the mycorrhizal SR component. We apologize for overstating the novelty issue and reduced the use of the words "unique" and "first time", but we feel strongly about the novelty of this dataset (hourly canopy and SR component fluxes over three years) that provides evidence for the "overflow tap" theory.

"Two main points should be carefully considered before it can be accepted for publication. 1) The authors compared 3 treatments (4 during the last year) that are represented by only 4 sampling points, which is a very low number of replicates considering the well-known high spatial variability of soil respiration and the also well-known spatial heterogeneity of soil properties including distribution of respiratory sources. One way to overcome this strong limitation we will to provide evidence that the 3 groups of 4 collars that will become the 3 treatments don't exhibit any significant difference among them before setting the treatment (so year 2007 data), at both daily and yearly time scale. Only in this case the calculation of Rr, Ra, Rm and Rh by difference among treatments will be relevant. This is therefore a strong prerequisite."

Response: We acknowledge the request for some pre-treatment comparison to show that there were no significant or at least substantial differences between the collars. We allocated the treatments on ranking the annual fluxes form the individual collar locations. The overall variation was fairly large and no significant differences were detected between collar pre-treatment averages (ranging from 2.64 to 2.71 μmol m-

2 s-1 for the three treatment averages with an SE of between 0.26 to 0.39), we now provide those means for the pre-treatment period (i.e. data range is not over a full year and thus not easy to incorporate into Fig. 6) and the coefficient of variation (SE) among the collar treatments of the pre-treatment fluxes (2007) based on hourly fluxes to fulfill this requirement (this is now quoted in the relevant M&M section). However, we point out that space sampling has a temporal limitation, whereas our study (with spatial limitation) opens new insights to the “overflow tap” theory by having a continuous long-term temporal record. We therefore feel that our annual comparison is adequate as we did not detect any differences between treatment flux averages during the pre-treatment period (now shown, see above). We could extend the Fig. 2 with different symbols to the pre-treatment period, but after having tried this, we feel this makes the Fig. 2 even more crowded.

"2) Similarly, the 4 collars (or 12) are located in virtual circle of 10 m radius close to the eddy flux tower, considerably less than the fetch (800m according to the authors). So, except if the authors can provide evidence that the 4 / 12 collars provide average Rsol values that are similar to an average obtained over a more larger area (using portable chamber measurements on a enough high number of collars), they should removed form the results section and from the discussion any relative value between their chamber measurements and the eddy covariance data or difference among these two sources of data (eg Rab, NPP, CUE, RS/Reco)."

Response: We acknowledge that the reviewer is probably correct that our chamber fluxes are unlikely to represent the absolute values across the fetch of the EC tower. However, we disagree that this technical limitation disqualifies the results/discussion of this study, mainly because it was not the aim of our study to do so. The goal of this paper is to identify temporal variations and correlations both in the time and frequency domains among GPP and soil CO2 efflux components. This is clearly stated in the revised introduction. In doing so, we assume that there is little or no spatial variation in the temporal dynamics and relative magnitudes of flux contributions, even if the abso-

lute flux sums are likely to vary within the eddy tower footprint. As soil type and forest management are identical across the eddy tower fetch, we consider this to be a valid assumption. There is a clear compromise between sampling in space and sampling in time, particularly considering the considerable cost of such systems. Clearly, the aims and hypotheses for this study require a high temporal resolution of fluxes, and we feel that compromising spatial representation, which was not the aim of our investigation, to optimize temporal resolution is fully justified. We have added some text about this issue in the limitations section.

"Wavelet coherence analysis can be kept assuming no correlation between spatial and temporal variability."

Response: The reviewer acknowledges the WCA is valid under the assumption that the footprint of the auto-chambers is representative of the temporal variability of the fluxes as those from the EC. We fully agree that this is the main assumption, which is discussed earlier in this reply to the reviewers and is now included in the manuscript’s M&M section. In addition we now state the limitation of the technology as is outlined in the comments to Rev#1, and we appreciate that the reviewer accepts our assumption for the interpretation in the frequency domain of the data, which is an important issue to support the discussion and “overflow tap” theory.

"Because the objective of the paper are clearly related to soil respiration, results and discussions regarding Rab and NPP are anyway out of the scope of the paper."

Response: We are confused by this statement but maybe this is due to the introduction requiring more information to the above-below ground C-allocation processes and clearer hypotheses as requested by Rev#1. There is increasing evidence about the link between above (e.g. NPP, GPP) and belowground fluxes, thus an interpretation of the linkages (e.g. overall net C acquisition and competing allocation) between these fluxes is important for the discussion of the “overflow tap” theory. We now include more background in the introduction on linking aboveground fluxes with the theory and the

SR flux components. Certainly, the link between the canopy and soil processes is the overall outcome of this analysis and is clearly within the scope of this manuscript, as acknowledged by the other reviewers.

"Additional comments: a. In many place in the discussion, the authors claim that it is the first time this kind of work has been done using hourly measurements of RS. However, they never provide any data at that time scale that will inform the reader that is really important. There is no information about infra daily variations. If these variations are small (as often found under close canopy in forests), it is then not so useful to have high frequency measurements."

Response: We respectfully disagree with this comment. The time series analyses were performed with hourly data and we provide a figure and table with information about variations between days (in terms of hourly data for the 1-day time period [i.e., intra daily variations]). These results show the importance of small variations that are only captured by analyzing the hourly data. We now clarify this in more detail in the M&M section providing more detail specifically regarding interpretation of variations between and within days and apologize if previously this was not clear enough. To show any of the hourly fluxes in any more detail, the overall graph would be impossible to 'digest' for a reader, and is only of use for analyses and modeling purposes. As far as flux average calculations are concerned, we toned down the significance of hourly measurements, but for the frequency analysis, high temporal resolution fluxes are clearly important and offer important and novel insights into the linkage between above and below ground C dynamics.

"The authors have a 'unique' set of data to check that. They can for instance compare what will be the difference between average of all data over a one year period, and using only data collected between 10 am and 16 pm one day every day, every weeks or every two weeks (my own experience on several ecosystems is that it doesn't change so much)."

Response: We suspect what the reviewer is suggesting is a different paper that could be done with this dataset. In fact, we show that this sample time issue does matter, see our recent paper, i.e. Heinemeyer et al., EJSS, 2011. The by the reviewer expressed concern about discrete spatial sampling would require a different analysis and one could of course answer different questions with the same dataset, but in our manuscript we choose to focus on the overall temporal flux dynamics. The real strength of the continuous time series is that new analyses can be done as a WCA and with discrete samplings one cannot see the multi-temporal temporal correlations as discussed in this study. Overall, multiple approaches can be used when analyzing such a rich dataset. However, here we were interested in studying the multi-temporal correlation between these variables and this is only possible by using high frequency sampling. If one were to use daily averages or a constant time measurement (e.g. between 10 am and 16 pm) one may be missing small variations that could provide relevant information about temporal correlations that can only be explored using the high frequency sampling dataset that we present. We now include more text about this in the manuscript in a concluding section entitled "limitations and future research considerations and applications".

"b. Soil moisture is vague. Better to say volumetric water content."

Response: Yes, this is right and has been altered throughout.

"Only one probe is use and moved every month, despite a well-know high spatial variability. It means that temporal and spatial variability are confounded in this case. It is maybe not so important because it has a weak influence on soil respiration, but anyway, it is a weak point."

Response: Yes, we now acknowledge in the revised manuscript in the discussion that spatial variability of SWC is important, but again it is a comment focused on spatial variability not the temporal variability, which is the central point of the paper. We hope that our revised manuscript makes the emphasis clear that this manuscript is dealing with temporal NOT spatial variability within the available technology.

"c. To my knowledge, LI7500 is not a close path IRGA. Maybe the reference is wrong. Did you account for nocturnal storage of CO2 when calculating Reco, and therefore GPP?"

Response: Yes, this is not a closed path and we apologise for the mistake, we used a LI7000, which is a closed path analyser; this has now been corrected in the M&M section. As for the storage term: We did not account for this, and we now point this out in the revised manuscript. This is a common problem, for example canopy storage measurements are unavailable for many flux tower sites in the Amazon (e.g. Iwata et al., 2005). Estimates of storage from a single point measurement, (which would have been our only option) have been proven to introduce large errors (see Iwata et al., 2005). In fact, Thomas et al. (2010) recently published EC flux data from a nearby site to ours (Wytham Woods) without making any canopy storage measurements and also related these data to SR fluxes.

"d. Meshes are inserted at 45cm depth. What is the rooting depth? Is there any autotrophic source of CO2 below? Please provide arguments."

Response: The main fine rooting depth in this clay soil is basically down to about 40 cm and declines in an expected exponentially pattern, also this site is water logged a lot and the organic matter is concentrated in the top soil. Therefore, more than 95% of the fine root mass will be captured by this depth. The only other autotrophic source is from trunk roots, which do not add significantly to the SRa component. This information is now included in the M&M section.

"e. The calculation of Ra, Rr, Rh and Rm doesn't account for difference in soil water content and the decomposition of cut root. Many authors have considered important errors associated with this lack of consideration. The authors acknowledged that point in the discussion but it should be mentioned here also because the equations as they are presented are false. We may have expected that you have attempted to quantify

the uncertainties due to this short cut."

Response: To our knowledge, none of the so far few mycorrhizal flux component studies including mesh separation has applied such a moisture and decomposition correction and we are unclear as to why the presented calculations are false (no explanation is given). This 'uncorrected' mycorrhizal component flux calculation has been applied to several other studies (Heinemeyer, Moyano, Subke, Fenn etc.); however, a moisture correction could be done but the effect is most likely to be small. We acknowledge this issue now better in the discussion and further point out our achievement to minimize the moisture artifact by partial rain exclusion. Regarding the root decomposition effect, we did consider the effect of cut roots and removed most visible roots when installing the mesh bags/collars, thus reducing any potential residual decomposition effect (we now added this information to the M&M section); we also compared the decomposition effect indirectly by comparing deep collar vs. repeatedly cut treatments, which did show good agreement. Moreover, the fact that the measured flux proportions agree well with the expected (i.e. according to the literature) overall contributions of Ra vs. Rh and RS vs. Reco makes us confident that any such root litter and decomposition effects must have been small. However, we clarify this limitation and the resulting potential error in the final section (limitations and future research considerations and applications).

"f. Calculation and discussion - of Q10 are irrelevant when the determination coefficient is low (or when the range of temperature is too small - not given here). Table 5 is therefore unnecessary and the discussion on this point is only confirmatory."

Response: We agree with the general comments/concerns on the usefulness of the calculation of apparent Q10 values and we acknowledged this in our discussion section. However, we do not think the calculations are wrong in themselves (and our intention was not to fully understand the effect of temperature) rather the discussion needed to point out this limitation more clearly and not to over-interpret the data. Therefore, we have removed most discussion about these values but advocate to keep the Table 5 as it contains some relevant information (particularly as shown by the WCA comments by

Rev#1 on the seasonal changes in temperature responses of soil respiration), which otherwise would be hidden from the reader; overall, we feel it is important for the wider readership to see the apparent temperature relations and their seasonal changes (or none) clearly presented in a commonly applied form (and therefore a confirmation is not bad but an added value, particularly when future meta-analyses or topical reviews are done), to enable understanding the further analysis and to look for interannual variation, which otherwise would leave some fundamental questions when interpreting the data: If we just leave it out the readership will indubitably wonder, so what about any temperature effect? We particularly choose to only show the seasonal temperature responses, which should be unaffected by short term (i.e. diurnal) 'hysteresis loop' dynamics. Table 5 therefore provides basic but informative results (as pointed out by the reviewer) also needed as background in relation to the WCA (see following point below). Different approaches such as time frequency decomposition would give a more confident approach but cannot be part of this manuscript; we now make this clearer in the section 'limitations and future research considerations and applications'.

"g. Removing the influence of soil temperature using daily exponential equation (Q10) is only relevant if the R2 of the fit is high. If not, it will introduce biases. And even when the R2 is high, temperature might be correlated with other factors that directly affect soil respiration, and this effect will not be any more visible. This point should be taken into consideration."

Response: We appreciate this comment, which is valid but also debatable. For instance, we decide to de-trend the time series from soil temperature in a uniform way. We treated all time series the same way and therefore the interpretation can be done under the assumption of de-trending the entire time series under a similar protocol. On the other hand, if we are selective when removing the temperature effect we need to include a threshold that would also be biased and would shift results depending on our selection, maybe much more than an overall applied correction. Furthermore, if de-trending is not done uniformly one can include artificial oscillations/periodicities (e.g.

one day de-trend using a soil temperature depth of 2 cm and another day using 5 cm), which would be difficult to interpret. It is true that soil temperature may be correlated with other variables but these can be explained by temperature, which is likely a key driver. Thus, the way we interpret the results is that whatever variation is left (i.e., de-trended time series) needs an explanation by other variables/drivers such as GPP (which in turn is also de-trended with the same protocol as the soil flux to avoid fluctuations represented by temperature). This unexplained variation is at the heart of the WCA analysis and supports the application/development of other theories such as the 'overflow tap'. We now pick up on this issue in the revised hypotheses and discussion sections. We recognize that different approaches can be done but we kept consistency with a recently published protocol by Vargas et al. (in press, New Phytologist).

"h. I agree that wavelet coherence analysis is a nice tool to study the coupling of canopy and soil processes, but it is not the only one. Several groups have done 13C labelling experiment in coniferous and broadleaved forests allowing a tight characterization a this coupling and this should be mentioned in the discussion."

Response: This is correct and this technique is far from being the only or the best. It is just a technique that exploits the resources of continuous measurements to extract temporal patterns. Other techniques can be applied and we already acknowledged a few recent studies, those measuring isotopes directly provide experimental evidence, but here we took advantage of the indirect time series approach and explored this over a much longer time scale. We now refer to a few more recent references in the discussion section (4.4.) and clarify the possibility of using other approaches in the revised discussion. We also added a reference (Vargas et al., 2011, in press) explaining that this technique can reconcile the observations from flux measurements and isotope experiments that link canopy and soil processes.

"i. The overture on priming effect at the end of the discussion as testable hypothesis is acceptable but it should not appear in last part of the last sentence of the abstract because it is not supported by the data."

Response: We agree, and the reference to the priming aspect in the abstract has now been removed.

"j. Figure 2 and 4 should be merged and GPP shown on a third panel as part of Fig. 3."

Response: We now include the Fig. 2 panel in the previous Fig. 4 (now corresponding to Fig. 3c), but we feel it is important to link GPP graphically to the SR and canopy fluxes in the same overall panel; this adds vital information to making the link between canopy and soil C fluxes and also in relation to the climate over the entire period, whereas Fig. 2 focuses on the SR component fluxes over a shorter period. However, we would like to point out that the current Figs. have been reduced in size and could be enlarged, to reflect the available age width above the Fig. captions. We trust this will improve the clarity of the Figs.

"k. Figure 6: cumulated values (gC m-2 y-1) will be more appropriate than average values (μmol m-2 s-1)."

Response: We do feel that for a practical comparison to other literature the average values are more useful (also see below Ref#3 comments). However, we did already provide a cumulative value in the abstract and results section for comparison to other literature in that format, i.e. average SR = 740$\pm$43 gC m$-$2 yr$-$1.

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