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 #2

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 irritating. 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-know 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. 2) Similarly, the 4 collars (or 12) are located in virtual circle of 10 m radius close to the eddyflux 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. Wavelet coherence analysis can be kept assuming no correlation between spatial and temporal variability. 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. 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. 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). b. Soil moisture is vague. Better to say volumetric water content. 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 influ-

ence on soil respiration, but anyway, it is a weak point. c. To my knowledge, LI7500 is not a close path IRGA. Maybe the reference is wrong. Did you account for nocturnal storage of CO_2 when calculating Reco, and therefore GPP? d. Meshes are inserted at 45cm depth. What is the rooting depth? Is there any autotrophic source of CO_2 below? Please provide arguments. 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. 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. 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. 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 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 j. Figure 2 and 4 should be merged and GPP shown on a third panel. k. Figure 6: cumulated values (gC m-2 y-1) will be more appropriate than average values (μmol m-2 s-1)

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