Biogeosciences Discuss., 12, C6421–C6428, 2015
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Biogeosciences Discussions
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Interactive comment on "Water use strategies of a young *Eucalyptus urophylla* forest in response to seasonal change of climatic factors in South China" *by* Z. Z. Zhang et al.

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Received and published: 12 October 2015

Dear Referee #2, On behalf of all the coauthors, I am really appreciated to your intensive comments and advices on our work. I am totally agreed with some of your opinions for the research. I believed that your comments will be great helpful for us to improve our work in this paper and in the future. We have revised our manuscript according to your comments. Please refer to the following response for details. The page and line numbers mentioned in the response refer to the latest revision of our manuscript (based on the revised version that sent to referee 1#) with all figures as a single PDF file. All the corrections will be emphasized in red in the manuscript.

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Interactive Discussion
Discussion Paper

Response to “General comments”:

Firstly, I want to clarify that dry and wet season is characterized by the rain distribution and soil water condition for the typical subtropical monsoon climate in South China (see Yan et al., 2006). We were referred to both of the soil water condition and evaporative demand in our paper. In our research, we found that the soil didn’t exert limitation on transpiration, which was promoted by transpiration demands. However, we will address it more clearly in the paper (pg.2, L. 26-27). Secondly, we admitted that there is a short of the relative soil water content in our study. However, even we have the relative soil water content, no one can tell that the plant was water limited by the soil water scarcity consider of the variety of plant types. What we can do is to quantify the responses of plants to different soil water condition. As to the plant water status, it is also lack of unified criteria to tell people whether the plant is water stressed or not. The leaf water potential is a good parameter to indicate the water condition of a plant, and what we can do is just to tell people how it will react to adapt to the surrounding environment. Thirdly, your advice of the combination of transpiration and water balances is a good idea, will take it into consideration in the future. However, it is actually not our topic in this study, we were not going to talk about it too much. As to the short duration of the experiment, I was no more agreed with you. There may be significant individual differences to confuse our result. However, I want to argue that it is unnecessary to worry about it since our experiment was conducted in a planted forest where the environmental heterogeneity is negligible. And the annual variation is also negligible within a short period (like a decade). We can tell that trees with different ages can be treated as the substitute of a same tree in different growing status. You also mentioned that older trees have lived and adapted to seasonal changes during years that younger trees did not “experiment”, I was also agreed with you. Actually, we are indeed intending to capture the result of this “different experiences” in our study.

Yan, J., Wang, Y., Zhou, G., & Zhang, D. (2006). Estimates of soil respiration and net primary production of three forests at different succession stages in South China.

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Global Change Biology, 12(5), 810-821.

Response to “Specific comments”:

Q and -m are not defined in the abstract. Response: yes, it had been fixed (pg.3, L3, 14). Introduction. Ok in general, but the objectives should be better defined. For example: “will the soil drought alter the water use of E. urophylla?” (P10474L1). This is a rather naive question; I would say that the soil drought will alter the water use of every plant, if severe enough. More interesting would be to know at what threshold of relative soil water content the transpiration of this species begins to decrease with the typical environmental demand of the “dry season”, or the rate of decrease in transpiration for a further decrease in water content. I also missed some more reviewing effort about the water use and water relations of the species or some other similar ones within the genus. But I must say I am not a specialist of Eucalyptus. P10470L16: reference Gs (not references Gs) P10473L10: isohydric, not isotonic. Also in Title of Section 4.4. Response: yes, the objectives had been rewritten (pg.6, L.1-3). As to the water use and water relations for this specie, we did treat it as an exception with other species. So we thought it was unnecessary to talk too much about it in this paper. All the other spelling mistakes had been corrected.

M&M I miss some soil data (upper and lower limit, depth) and some more climate data related to transpiration (for example reference evapotranspiration and its seasonal evolution). D is also a key variable for the discussion of results, and a plot of it its average daily value (or the average value during April and October) would be helpful. Response: we only have the soil water content of 30cm depth and we didn’t measure the soil depth of the plot. D in dry and wet season was 1.07 ± 0.39 kPa and 0.32 ± 0.28 kPa respectively, which had been presented in pg.12, L.8-9.

P10475L1: pure thermal dissipation probes (like Granier types) are raising increasingly serious concerns. For example - as this is relevant to this paper – they give different responses depending on trunk mass. In any case some serious critical discussion

about TDP and its errors is required and should be added, as the whole work is based on it. See e.g recent works of Sappala et al. Response: yes, it is really an interesting question and worth of further discussion. However, we were not aimed to discuss the potential errors of this method, and too many discussions will lead to digression. What we can to was tried to fix the FQs during the sap flow measurements and its scaling up, such as the radial variation of sap flux on the trunk.

P10475L1 to 4: the variability of the sap flux within a tree is one of the main problems at the time of using sap flow to measure transpiration but it is so rarely mentioned in sap-flow papers that when it is addressed - like here - it is worth a standing ovation. Bravo! Response: thanks for your affirmation.

P10475L26: the whole trees were scanned with the LI 3000 or a subsample? If a subsample was used then the scaling-up procedure should be mentioned. Response: yes, five small subsamples of each tree were scanned and weighted (fresh weight), the ratio of leaf area/ fresh weight was estimated. Then the whole tree leaves were then collected for the estimation of AL (pg.7, L24-26).

P10475L28: maybe I missed something, but here the authors mention some core sampling was made? Response: I'm sorry about the missing. We take cores with a growth increment on trees around the stand (n=27). The sapwood depth was visually distinguished from heartwood by changed color at the boundary of the two parts on the cores before the sapwood area was estimated (AS) (pg.7, L. 26-27). P10476L13 (Eq. 1): I cannot find the definition of some of the variables. EL is defined only in the next page, etc. P10476L20: used a Scholander bomb? Response: EL had been defined in equation 2(pg.9, L. 17). We used a portable plants pressure chamber (PMS 1000, Corvallis, OR, USA) to measure the leaf water potential (pg.9, L. 3-4).

P10477L4 Please, check if As and AL are defined in the first instance, I cannot find it out. P10477L7 (eq. 2): please control the dimensional correctness of equation 2. P10478L19: too many detail. P10479L17: D should be correctly defined as vapour

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pressure deficit. Response: As and AL had been defined in pg.7, L.23, 29. D had been defined in pg. 3, Line 1.

Results The measured sap flux seems very low to me. The ET would be less than 2 mm/day in the "dry season" and 1.3 mm/day in the wet season. It is difficult to put these number in the right context without the value of annual ET0. P10480L8: I sense incongruence here. The annual stand ET of 462 mm means an average daily ET of 462/365=1.27 mm/day. Now, in October, the peak transpiration period, the average ET was 5.7 kg/day tree. An hectare loses in October (P10480L11) 5.7(kg/day)*1375 (trees/ha) =7838 kg / ha day. 7838 /10000 (m2 /ha) = 0.7838 kg/m2 day or mm/day. There is something that is not working here, may be I'm missing something, but it might deserve a further look by the authors. Response: The small sapwood depth may be responsible for the low ET for this specie. About the incongruence, we didn't use the ground area as the dividend, because the low LAI, not all the land was covered by the crown, so we used the canopy area (AC, projected area of the tree crown) as the dividend to calculate the stand transpiration to minimize the error caused by soil evaporation.

P10480L15: Not clear. Response: I am sorry, I did understand what is not clear.

P10481L1-10: These leaf water potentials suggest no water stress. Response: as I have discussed above, you can't tell it is water stressed or not, we just can tell what happened for the plant under such conditions.

P10482L14-18: the number of trees with DBH >11 are too scant to affirm constant value. The significance of Fig 5, in particular of the curve fitted, for DBH >9cm is very feeble. This is true for other conclusions where DBH >9cm. Response: yes, the statement here was arbitrary. It had been corrected (pg.15, L.6).

Section 3.3: this part is quite obscure, I suggest the Authors to better define (in intro) what is exactly they are looking for and describe (in M&M) the methodology they decided to apply and why, step by step, instead of much line fitting without a clear-cut

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model idea. Response: yes, the referee #1 also point out the same problem, we had rearranged this part in the revised MS (see pg.13, 14, 15).

P10483L24: comparing this forest with other forests in different climates (and different evaporative demands) is scarcely useful. Having provided the ET0 of the area would improve the discussion. Response: yes, your advice is great helpful. However, since the experiment had finished, more calculation on ET0 is impossible for us. We will do that in the future. We had simplified this paragraph to remove the unnecessary discussion (pg.16, L. 9-17).

P10484L5-8: maybe because the ET0 was higher in Venezuela? We'll never know if it was this or the different LAI without relating the transpiration to a reference value of evapotranspiration. L9: for the same reason: the effect of VPD. Response: this is a possibility, since the water use of the E. urophylla is not our object in this study, we were not going to discuss too much about it. And the reference value of evapotranspiration is also impossible since the experiment had finished.

L18-19 and after: Please rephrase. The enhanced transpiration is already explained by the higher evaporative demand, no need to imagine a deeper rooting system (besides, authors present no data to affirm that the roots depth increases in the dry season).Without data of rooting depth all this part of the discussion is speculative. Response: yes, you are right, this part had been removed (pg. 16, 18-28, pg. 17, 1-9).

P10485L11: the soil has been defined heavy (clay) loam in M&M: this is not a low water retention soil. As we do not know the soil depth, we cannot do any water balance. Do the authors ever suspected that the Granier probes are underestimating the flux (and thus the transpiration?) Is it possible to exclude that? This is a very important point that should be made clear beyond any possible doubt: It would explain the transpiration lower than expected, the soil water depletion higher than expected etc. Response: you suspect is reasonable. But I think the mostly possible reason that may lead the underestimate of ET will the judgment of sapwood area. Firstly, the boundary of sapwood

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and heart wood is actually not very clear, especially for trees in tropic area and small trees. The average DBH in our stand was 10.1 cm during the experiment. Secondly, the implicit assumption is that heartwood will not transpire water in the plant, however, it might be invalid for small trees in our study. Lastly, with lower DBH and LAI, it is reasonable to have a low ET in our study.

P10485L11-12: the presence of litter on the ground will decrease (and never increase) direct soil evaporation. Is often done artificially in horticulture and it is called mulching. Response: yes, thanks for your explanation. This part is irrelevant with our topic, we had removed them.

P10485L18: nocturnal sap flow is partially due to water replenishment and partially to nighttime transpiration. In my knowledge, none has still found a satisfactory way to separate them. With the high stem water potentials measured by the authors, I would say that water replenishment was marginal here. Are the authors confident in the precision of the Granier method for measuring extremely low velocities like those measured at night? I'm not. Response: yes, the night time sap flow was small compared with daily ET. What we wanted do was to present a strategy for plants to maximized the gas exchange even it is negligible. As to your suspect, it is also exist in our research. However, since we compered the difference of same trees with a same measure system, we are confident that the difference of night time sap flow between wet and dry season was truly existed.

Section 4.3: the discussions about water use efficiency should be backed up by some measurements. Without measurements of CO2 assimilation, the whole WUE discussion is necessarily speculative. Response: Thanks, the referee #1 has the same opinion with you. We had removed most of the content about WUE.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C6421/2015/bgd-12-C6421-2015-supplement.pdf

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