Biogeosciences Discuss., 12, C5183–C5194, 2015
www.biogeosciences-discuss.net/12/C5183/2015/

Biogeosciences
Discussions

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: 11 September 2015

Dear Referee #1, On behalf of all the coauthors, I am really appreciated to your constructive comments on our work. I believe that the quality of our manuscript will be substantially improved after the proposed corrections. We have revised our manuscript according to the comments. Mainly we rearranged the main text and focused our discussion based on the results. 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 with all figures as a single PDF file. All the corrections will be emphasized in red in the manuscript.

Specific Comments:

Title: I would not refer to 'seasonal change of climatic factors' as climate is the longterm average of meteorological conditions at a given site. Response: The word "climate" will be replaced by "environmental"

pg. 10471, L. 10-11. Vapour pressure deficit (VPD) increases exponentially with increasing air temperatures and therefore warming is expected to have a larger influence in future VPD than reduced precipitation. Response: we had rewrite the sentence as "The direct effect of decreased rain events is the decreased soil water supply, which may further restrain transpiration especially for shallow root plants. However, since vapour pressure deficit (D) increases exponentially with increasing air temperatures and therefore warming is expected to have a larger influence in future D, transpiration will be enhanced in drier atmosphere in the absence of plant physiological regulation." (Pg. 1, L.2)

L. 13-17. As the authors point out, there are good examples of drought vulnerability in tropical forests, but also please check a recent review which deals with several stabilizing mechanisms of vegetation in response to extreme climate events (Lloret et al.,2012). (Pg. 2, L.26-31) Response: Thanks for your suggestion, the following sentence will be added to the main text after the plant vulnerability: "While as reviewed by Lloret et al., (2012), many empirical evidences support the existence of stabilizing processes minimizing and counteracting the effects of these extreme climate events, reinforcing community resilience." (Pg. 3, L. 6-8)

Lloret, F., Escudero A., Iriondo, J. Martinez-Vilalta, M., J., Valladares, F.: Extreme climatic events and vegetation: the role of stabilizing processes, Global Change Biol., 18, 797-805, doi: 10.1111/j.1365-2486.2011.02624.x, 2012

pg. 10472, L. 1-2. New paragraph? L. 23-24. What do you mean by 'deviated physiological response'? L. 26. Which 'abovementioned effects' are you referring to? L. 26-27. There is a link between the differential drought responses of different-sized trees (opening sentence of the paragraph) and the closing line on the different impact of water stress on tree growth of different-sized trees. Changes in hydraulic allometry are indeed mediated by sihfts in growth and biomass allocation. However, this link is not clearly explained in the paragraph, please clarify. Response: Yes, it would be better to divide the paragraph here (pg. 3, L. 21-22); Here when I said "deviated physiological response", we meant the different response to soil drought-induced water stress for small trees and large trees. We will change the sentence with "The different physiological response of juvenile and full-grown trees" (pg. 4, L. 11-12); abovementioned effects referred to "physiological response strategies of E. urophylla " (pg. 4, L. 24-25); The link will be presented as "As proposed by Cavender-Bares and Bazzaz (2000), juvenile trees are more affected by drought than mature trees, due both to their shallower rooting as well as their inability to fix C at low leaf water potential. They resist drought by closing stomata early in the day at the expense of C uptake. Mature trees avoid drought conditions by accessing deeper water reserves and adjusting WUE, sacrificing C gain only marginally." (pg. 4, L. 19-24).

pg. 10473, L. 'Reducing Gs to water vapor' please reword. L. 5-7. I cannot see the difference here between 'regulation of stomatal aperture' and 'stomata must react rapidly...'. L. 10. 'isohydric', not isotonic. L. 11-13. There are many studies dealing with seasonal changes in Gs responses to D. L. 14-18. The most frequent response is that trees reduce Gs before changes in hydraulic conductance (Martínez-Vilalta et al 2014). Response: this phrase was redundant and had been deleted; we meant that plants not only regulate the stomas on the aperture, but also on the sensitivity; yes, it should be "isohydric" (pg. 5, line 8); the sentence had been removed; the word "accompanying" had been replaced by "followed" (pg. 5, line 12).

Martínez-Vilalta J, Poyatos R, Aguadé D, Retana J, Mencuccini M. 2014. A new look at water transport regulation in plants. New Phytologist 204:105–15.

pg. 10475, L. 1-2. Not really an assumption of sap flow measurements per se, but one related to upscaling of point measurements of sap flow. L. 19-20. Delete sentence,

it is not needed here. Response: you are right, "sap flow measurements" should be replaced by "scaling up of sap flow density" (pg. 6, line 25); the sentence here had been deleted (pg. 7, line 15).

pg. 10476, L. 4-6. Please clarify whether you are referring to stand transpiration here. Provide details on how you did it: did you multiply the mean sap flux density per unit sapwood by the sapwood area: ground area ratio? Did you use s stratified scheme (by diameter classes)? L. 15. In the previous equation, ks should be whole-plant conductance per unit sapwood (not only sapwood conductance). Response: no, we were not referring to stand transpiration here, we aimed to acquire the AS and AL of the trees for sap flow measurement, however, since the DBH range of harvest trees couldn't cover those trees for sap flow measurement, we adopted those data in Zhou et al. study, and merged them with our harvest data together, and fitted the relationship of DBH versus AS and DBH versus AL. Then the sap flow measurements were scaled up to whole tree transpiration by multiplying the predicted AS from the fitted relationship above (pg. 7, line 29). Here we should say sorry because we miss the method of stand transpiration. We will add them as below: "Stand water use per ground area (E, mm) was estimated as the product of plot (20$\times$20 m) sapwood area (derived from the DBH versus AS relationship above for each tree in the plot) and hourly mean of Fd of the monitored trees (since no significant relationship between Fd and DBH was observed, p=0.45), and divided by the ground area of the plot. The total water use during the experimental period was summed by the hourly mean of E. However, because of the power down and equipment failure, there were missing data during the experiment. We fitted the relationship between Q0 and daily sum of E to fill the gap (pg. 8, line 5-12);" yes, there is some word missing here, we will correct them as "whole-plant conductance per unit sapwood" (pg. 8, line 18-19).

pg. 10478. L. 11, please see previous comment on ks and be consistent with equation (1). L. 17-18. It would be easier to refer to the seasonal periods always with the same name, wet vs dry, and not mix it with 'April' vs 'October'. Response: actually, the equation here should be removed since no $k\Psi$ was associated in the main text; yes, we will make it be clearer.

pg. 10479. L. 3-25. Please make this paragraph shorter, there are sentences that can be omitted; for example L. 11-12. L. 17-20. How was the boundary-line analysis conducted? Using a binning approach or using quantile regression? Response: the paragraph had already been shortened, see (pg. 11, line 23-28 and pg. 12, L. 1-9); a binning approach was used as below: The upper boundary line was derived by: (1) partitioning Fd data of each tree into 50 μmol m-2 s-1 intervals, (2) calculating the mean and standard deviation of Fd in each interval, (3) removing outliers ($P < 0.05$; Dixon's test), (4) selecting the data falling above the mean plus one standard deviation, and (5) averaging the selected data for each Q0 interval with $n \geq 5$ remaining Fd values. Excluding intervals with $n<5$ was done to prevent Q0 intervals with too little information from affecting the relationship. The mean Fd values of each tree of all Q0 intervals obtained in step (5) were Fitted in Figure S2. (pg. 11, line 8-14).

Pg 10480, L. 8-9. Please describe better the scaling procedure from Fd to Et in the methods, starting with defining Et there. See previous comment on page 10476, L. 4-6. L. 18. The method of calculation of ET-NOC needs to be described in the methods. Response: it had been defined in pg. 7, L. 29; ET-NOC was defined in pg. 7 L.29-30.

pg. 10481, L. 1-2. Hence, there are no differences in soil water availability between wet and dry periods (see discussion). L. 13-21. This paragraph should be placed in the methods section. The methodology for deriving boundary-line responses needs to be described. L. 23-24. There is no statistical model and test reported to claim that m differed significantly among light levels. L. 26-29. These lines belong to the methods section; for example, Qo is mentioned here and the equation defining it is provided later. L. 14-22. Part of this paragraph also should be in the methods section; please rearrange the text accordingly. The data analysis section should contain a description of the various analyses and why they were performed. L. 14-27. Same as previous comment. Response: the water potential here was the leaf water potential,

even though there was no differences in leaf water potential, the soil water availability was significant different between wet and dry season, see Figure 1. However, since the similar leaf water potential in dry and wet season indicated a less stressed soil water condition even in dry season for E. urophylla; this paragraph had been moved to methods section, and the boundary line analysis had also been specified in the data analysis (pg. 11, L. 8-14); yes the significant level had been added; Q0 actually had already been defined in the introduction section (pg. 7, L. 7), by the way, the lines had been moved to the methods part (pg. 10, L. 12-20); the text of this paragraph had been rearranged.

pg. 10482. L. 7. Does Gs,ref-max correspond with parameter a in Eq. 5? L. 18-20. Please simplify your sentences, here and in other instances within the text: 'Gs,ref was significantly higher in the wet season (88.6) compared to the dry season (68.8, $p<0.01$)' L. 21. Here you define the ratio dry/wet for Gs,ref-max, but previously you use the terms 'ratio of Etnoc between wet and dry seasons (pg. 10480, L. 25-26). Please be consistent and use always the same ratio. Paragraph starting on line 23: Again, the paragraph is a mix of the explanation of an analyis (methods) and results. It is difficult to understand: please specify which are the variables in the boundary line analysis, don't refer to the analysis 'above' (L. 24); which 'slope' (L. 27)?; what do you mean by 'improved' or 'suppressed'? Please use simple clear terminology (i.e. positively or negatively correlated). Response: yes, it referred to parameter a in Eq. 4; it had been simplified (pg. 14, L. 21); the ratio of Etnoc between wet and dry seasons had been removed since it seems to be irrelevant with our topic; we had rearranged this paragraph and cleared the confusing words (pg. 13, L. 21-29).

pg. 10483, L. 10. Has 'Hp' been defined in the methods? Also, the following sentence can be omitted, because it's wrong: conductance is ks, not 1/h. L. 15-22. It is not clear how ks was estimated (and it is not clearly explained in the methods either). I understand that it was calculated from Eq. 1, but is there a reason to not calculate it using the measured Fd and the water potential difference? This seems easier to me.

Response: it has been removed since it was weak relevant with our result, and the following sentence had also been removed; actually Fd and the water potential difference will result in whole tree hydraulic conductance, not hydraulic conductivity, and equation 1 is transformed from the hydraulic conductance, since D varied significantly among different days, so we defined D=1kPa to compared the difference of ks between wet and dry season.

pg. 10484, L. 9-11. The problem is that there is no really 'drought' here. Predawn water potentials were the same as in the wet period and SWC was still high. Tree transpiration may be little affected by reductions in SWC before a threshold is reached, there is plenty of evidence for this. In this study, tree transpiration is largely controlled by evaporative demand, not by SWC. You don't even need to invoke access to deep water in the soil to explain your results (SWC in the upper soil is already high). It is unclear to me what is being discussed in the paragraph, mixing the effects of rooting depth with aerodynamic coupling. Response: we have some different opinions about the result. Even predawn water potentials were the same, what we can tell is that the plants didn't suffered a water stress in dry season, and the decreased soil water content within the ground layer (0-30cm) in dry season didn't influence the water status of E. urophylla. We wanted to present why this situation happened for E. urophylla in this paragraph. As to the aerodynamic coupling relation, we want to explain that D controlled the variation of Et from wet to dry season is reasonable. pg. 10485, L. 6-17. What is the message of this paragraph? Soil evaporation is an important component in this low-LAI forests? There is no specific hypotheses or measurements on this. L. 23-25. Stem capacitance has not been measured in this study, so this claim is not appropriate here. In fact, the entire paragraph does not discuss a relevant result arising from the study and could be highly shortened, or even omitted. Response: yes, it seems irrelevant with our topics, and had been removed; we don't think this paragraph was irrelevant with our topic since night time transpiration contributed to one aspect of water use strategy. However, the "stem capacitance" was removed.

pg. 10486, L. 12-13. There are many different views on the mechanisms of stomatal closure, and the direct response to leaf water potential is only one part of the story. L. 17-21. The difference in 234.4.% (GS constant) vs 159.5% (Gs decreasing in response to increasing VPD) is kind of obvious. I may be missing something, though; what is the novel result here? L. 21.24. I can't see the link between these lines and your results... L. 25-28. This should go to results. Response: we have corrected the sentence here as "One of the views on the mechanisms of stomatal closure"(pg. 17, L. 27); what we wanted to quantify the stomatal regulation of transpiration for E. urophylla; yes this part seems to be irrelevant with our result, and had been removed; those lines had been moved to the results.

pg. 10487, L. 5-16. Here is an interesting result, which the authors could discuss further. Is this behaviour (i.e. not complete stomatal closure under low leaf water potentials) general among Eucalpytus? Can the authors provide more references and values of residual Gs in other Eucalyptus species? Another question, how low are the values of water potential (-1.6 Mpa) compared to absolute values of minimum water potentials recorded for the species? L. 26-27. Any explanation as to why ks decreases more than Gs? Where in the plant is this decline in ks expected to occur (leaves, stem, roots)? This is much more relevant than the discussion on WUE..(see next comment). L. 27 and following. It is merely speculative to discuss about WUE because you did not measure assimilation and the reader has no way of knowing what these trees were doing in terms of WUE. Response: Two more examples of residual Gs was provided as: "Another example presented by Mielke et al. (2000) showed that GS of Eucalyptus grandis maintained ∼40% of the maximum when ψL<-2.45 MPa until the minimum ψL was reached (-2.8 MPa). GS was also found to stabilized at ∼37.5% of the maxmim after ψL<-2.37 MPa until the minimum predawn ψL (-3.37 MPa) when three allopatric Eucalyptus (E. camaldulensis Dehnh, E. leucoxylon F. Muell and E. platypus Hook) were investigated together (White et al., 2000)" (pg. 18, L. 14-20); even the minimum ψL of -4.8 MPa was observed, t a value of ∼65 mmol m-2 s-1 still occurred in their study(pg. 18, L. 20-21). In addition, most of the text in this paragraph had been rewritten; as to the reason why ks decreases more than Gs, we propose that the anisohydric behavior is responsible. We added this part as: "Cavitation avoidance was a likely physiological function associated with stomatal regulation during water stress in our experiments as we discussed above. However, the possibility still exists that the striking relations that were observed between cavitation and stomatal function were only correlations and that the main physiological trait involved in the regulation was elsewhere, such as in the leaf mesophyll itself since stomatal closure was also correlated to bulk leaf turgor (Cochard et al. 2002). Evidently, differences in the behavior of isohydric and anisohydric plants are due to differences in the sensitivity of their respective guard cells to a critical ψL threshold (Sade, et al., 2012), which may contributed to the unsynchronized response of decreased ks and approaching stabilized GS of E. urophylla." (pg. 19, L. 13-25); Since we didn't measured the ks of root, trunk, or shoots, however, we inferred that the hydraulic conductivity of shoots may be responsible, since many results had proved the branch dieback in periods of extended drought across a variety of species (Kursar et al. 2009; Urli et al. 2013; Choat 2013). However, it also can't rule out other possibility, such as roots, since Domec et al. (2010) reported that embolism in roots explained the loss of whole-tree hydraulic conductance and therefore indirectly constituted a hydraulic signal involved in stomatal conductance reduction for Liquidambar styraciflua and Cornus florida (pg. 19, L. 26-29 and pg. 20, L. 1-5); You are right about WUE, almost all the relevant discussion had been removed.

Mielke, M. S., Oliva, M. A., de Barros, N. F., Penchel, R. M., Martinez, C. A., da Fonseca, S., & de Almeida, A. C. (2000). Leaf gas exchange in a clonal eucalypt plantation as related to soil moisture, leaf water potential and microclimate variables. Trees, 14(5), 263-270. White, D. A., Turner, N. C., & Galbraith, J. H. (2000). Leaf water relations and stomatal behavior of four allopatric Eucalyptus species planted in Mediterranean southwestern Australia. Tree Physiology, 20(17), 1157-1165. Sade, N., Gebremedhin, A., & Moshelion, M. (2012). Risk-taking plants: anisohydric behavior as a stress-resistance trait. Plant signaling & behavior, 7(7), 767-770. Cochard, H., Bréda, N., Granier, A.: Whole tree hydraulic conductance and water loss regulation

in Quercus during drought: evidence for stomatal control of embolism? Ann. Sci. Forest., 53,197–206, doi: 10.1051/forest:19960203,1996. Kursar T.A., Engelbrecht B.M.J., Burke A., Tyree M.T., El Omari B. & GiraldoJ.P. (2009) Tolerance to low leaf water status of tropical tree seedlings is related to drought performance and distribution. Functional Ecology 23, 93–102. Urli M., Porte A., Cochard H., Guengant Y., Burlett R. & Delzon S. (2013) Xylem embolism threshold for catastrophic hydraulic failure in angiosperm trees. Tree Physiology 33, 672–683. Choat B., Jansen S., Brodribb T.J., et al. (2012) Global convergence in the vulnerability of forests to drought. Nature 491, 752–755.

pg. 10488, L. 11-20. Unclear the link between your results and WUE (and difficult to assess without actual measurements). How did you estimate PLC? Is it for stems, roots or leaves? Differences in PLC would be relevant for patterns in ks across tree sizes; why do you insist discussing WUE? L. 21-end of paragraph. This paragraph mixes discussion on the effects of tree size, growth rate and natural vs restored habitats on drought responses. The logig in lines 25 to L.2 in the following page is difficult to follow. Please keep to one clear message per paragraph. Response: the PLC was estimated as 100 $\times$ (1- ks/km), where km is the max ks, which had been put in methods section (pg.8, L. 20-21). It is actually the whole tree PLC; we had rewrite this paragraph (pg.20, L.15-29 and pg.21, L. 1-13).

pg. 10489, L. 4-14. Again, this discussion on WUE is not relevant here. The discussion should be focused on your results, guided by the hypotheses of the study. L. 15. Isohydry not isotonicity. L. 16-29. Most of this paragraph should be moved to the methods section. Response: we don't think this paragraph should be moved to the methods section, since the methods had already been mentioned, what we do here was the repeat of the analysis, and the only differences exist in the D ranges. We were discussing the reason for the variation of –m for E. urophylla. However, we think it would be better in the result section (pg. 14, L. 10-23).

pg. 10490, L. 3. You mean -m, not GS,ref. L. 5-8. Please clarify this sentence: why an analysis of -m at different light levels indicated differences in D?? L. 11-13. Do you think that the decreasing pattern of -m with radiation is actually mediated by changes in D? Because the D range will also change across light levels, and the D range has a known impact on the fitting of Gs vs D responses. L. 24 and following. The interactions between Gs, D and radiation might be more complex than what is mentioned here. For example, check this paper:

Ewers B, Oren R, KIM HS, Bohrer G, LAI CT. 2007. Effects of hydraulic architecture and spatial variation in light on mean stomatal conductance of tree branches and crowns. Plant, cell & environment 30:483–96. Response: yes, there was something wrong in this sentence, it should be removed; the discussion in this page was a little confusing, so we rewrote most of them (pg. 21, L.15-21).

pg. 10491, L. 12-14. Where does this result come from? L. 14-5. As mentioned here, the high SWC (and high pre-dawn leaf water potentials) probably preclude the interpretation of any relevant drought response in this study, L. 21. In short, transpiration was largely controlled by evaporative demand, with a weak effect of SWC. Response: these paragraph had been removed; the sentenced had been simplified (pg. 22, L. 15-16). Pg. 10492, L. 1-4. Neither hydraulic failure nor WUE were measured in this study. L. 5-8. What about Gs? It's also more drought-sensitive as tree size increases (Fig. 5). Response: both of them had been removed; actually, Fig. 5 presented a higher GS for larger trees, not sensitivity. It had been showed in pg. 22, L. 27-28.

Figures

Fig. 1. Please add values of VPD to see the changes in evaporative demand; the main periods of analysis (wet vs dry) can be highlighted in the figure. Fig. 3. Use dry vs wet, not October vs April. Are symbols trees or light levels? A legend for the symbols is missing. Fig. 4. Are symbols different trees? Legend is missing for both symbols and lines. Fig. 6. Please define 'normalised architecture', 'standardised architecture' and Hp in the methods section. Fig. 7. These analyses are not described in the methods.

Fig. 8. How are PLC curves obtained? Response: Figure 1: it had been added in the figure (pg. 33 L. 1-5); Figure 3:it had been corrected, different symbols represent the different light levels of the 15 trees (pg. 35, L. 1-6); Figure 4: Different symbols in the figure represented the 15 tree individuals. Light and dark lines responded to the least square fit in dry and wet season respectively (Figure 36, L. 1-8); Figure 6: this figure had been removed since it seems irrelevant with our topic; Figure 7: it had been describe in the result section, see pg. 15, L. 10-16; Figure 8: it had been addressed in pg. 8, L. 20-21.

S2: It is not clear whether all data or a subset of the dataset is represented here. I would expect more scattered points in the space below the curves. S3. According to the legend in Fig S3a, different symbols depict light levels, but the caption says that symbols represent trees. Please clarify. For S3b, different symbols are individual trees right? Please also consider using different symbols to represent different thigs (open vs closed), otherwise it can be a little confusing. Response: Figure S2: all the data during the period when the sap flow measurement was monitored were included, the boundary line analysis only depicted the upper boundary of the data; Figure S3: it represents different light level; yes, it represents different individuals in 3b, and you are right, we will correct it in the figure.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C5183/2015/bgd-12-C5183-2015-supplement.zip

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Interactive comment on Biogeosciences Discuss., 12, 10469, 2015.