Biogeosciences Discuss., 12, C6230–C6235, 2015
www.biogeosciences-discuss.net/12/C6230/2015/

Biogeosciences
Discussions
Open Access

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.

Anonymous Referee #2

Received and published: 5 October 2015

Water use strategies of a young Eucalyptus urophylla forest in response to seasonal change of climatic factors in South China

In this paper the authors measure the transpiration of a tropical Eucalyptus species with TDP ("Granier") probes. They then compare the transpiration and the canopy conductance of two periods, a "wet" one (with low VPD and plenty of water in the soil) and a "dry" one (with higher VPD and less –probably still non limiting - water in the soil). They discuss the difference in canopy conductance between the two periods, and how these differences are related to the tree size (or age).

General comments I sense some confusion throughout the paper induced by the use of

"dry" and "wet season": dry season means less water in the soil AND more evaporative demand, but the authors should be more precise about which one – or both - of these conditions are they referring to for each response they describe or make hypothesis about. See for example P10473 L25.

One of the concerns I have about this paper is that the water status of the trees is not really known. This is a serious limitation to any discussion of results. In my opinion, the soil water contents registered during the "dry" season are unlikely to be limiting the transpiration significantly (if at all), given that the soil is clay-loam and the vapour pressure deficit D is relatively low – in absolute terms (I may well be wrong as I'm not very familiar to this species). In any case, the upper and lower limit of the soil are not given, so we cannot devise the relative soil water content. In general, the leaf water potential data recorded do not suggest water-stressed conditions, unless in a very isohydric species, in which case the leaf potential means absolutely nothing). Neither the soil depth is given (or I missed it), which is quite important to reckon a rough water balance. In a site with 2000 mm of rainfall a year and a deep soil I would bet that no tree will ever have its transpiration limited by water scarcity; if the soil is 60 cm deep that would be another story. Whether or not water stress is present is important, and a precise quantification of it is paramount here, as one of the objectives of the paper is ". . . determining how the young planted Eucalyptus forest . . . will function under enhanced drought stress. . ." Another weak point is the lack of any independent assessment of transpiration beyond the Granier probes; no water balance was calculated, although they measure the soil water content. A little more confidence on the transpiration fluxes would have been very useful, as the transpiration measurements - much lower than expected - are the backbone of the work: if they are wrong no analysis or conclusion holds. Although the number of probes is high, the duration of the experiment (1 year) is relatively short (IMO) to draw general conclusions on the E. urophilla response to changes in water content and evaporative demand. This is particularly true if the Authors intention is drawing conclusions about the adaptation to drought as a function of tree age. In fact, older trees have lived and adapted to seasonal changes during years that younger trees did not "experiment", so the living history of trees of different ages is necessarily different, and this difference is depending on the weather variability between years. Conclusions about adaptative capacity at the tree level should be derived using longer experiments, following the same trees during their growing up.

Specific comments Q and -m are not defined in the abstract.

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

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

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.

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!

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

P10475L28: maybe I missed something, but here the authors mention some core sampling was made?

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?

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 pressure deficit.

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.

P10480L15: Not clear.

P10481L1-10: These leaf water potentials suggest no water stress.

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.

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 model idea.

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.

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.

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.

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.

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.

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.

Section 4.3: the discussions about water use efficiency should be backed up by some measurements. Without measurements of CO_2 assimilation, the whole WUE discussion is necessarily speculative.

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