Biogeosciences Discuss., 6, S599–S605, 2009
www.biogeosciences-discuss.net/6/S599/2009/

Biogeosciences Discussions

Interactive comment on "A whole plant approach to evaluate the water use of mediterranean maquis species in a coastal dune ecosystem" *by* S. Mereu et al.

Anonymous Referee #4

Received and published: 28 March 2009

Review of ‘A whole plant approach to evaluate the water use of mediterranean maquis species in a coastal dune ecosystem’, by S. Mereu, E. Salvatori, L. Fusaro, G. Gerosa, B. Muys, and F. Manes; for Biogeosciences.

General comments: In this paper, Mereu et al. compare the water use of three different woody species coexisting in a coastal dune ecosystem characterized by very low soil water contents. The study period lasts from mid May to late July 2007. The authors confirm that the three studied species (Quercus ilex, Phillyrea latifolia and Arbutus unedo) have well contrasted drought responses: A. unedo showed a marked decline in gas exchange and water use parallel to the development of summer drought. Q. ilex,

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on the other hand, showed a slight decline in water use associated with lower midday leaf water potentials. Finally, P. latifolia showed almost constant transpiration rates in connection with an apparent increase in whole-plant hydraulic conductivity.

The results are largely descriptive and confirmatory of previously published studies. The main interest of the paper is the extreme character of the study system itself and the fact that many studies have already been conducted at the same site, providing an opportunity for integrating different aspects of the ecophysiology of the studied species. In my view, however, this opportunity has not been fully realized. I suggest the authors restructure the manuscript and focus it around one or two well defined hypotheses. In that respect, the previous paper by Alessio et al. (2004) on the water sources of the studied species at the same site, together with the previous ecophysiological studies on those species, provide a very good opportunity for hypothesizing specific responses for each species. Also, the paper would benefit greatly from a review by a native English speaker and by a careful revision by the authors to correct any remaining mistakes.

Specific comments: (1) p.1714, l.7; and thereafter: The manuscript contains many ambiguous statements that are not well supported with either data or arguments. For instance, what is meant here by ‘complexity of the response’ and by ‘complexity of the system’?

(2) p.1715, l.19-20 and thereafter: It is unclear to me why do you think that ‘these characteristics of Mediterranean dune ecosystems may prevent the possibility to determine the water use strategy of a species’, and what is added by your study in that regard. The connection with climate change should be either developed further or deleted.

(3) p.1716, l.11: I don’t think this equation is required. In any case, it is unclear what ‘g’ stands for in Eq.1, and the same symbol is repeated with a different meaning in Eq.3. Also, you should make clear at which level you are focusing the discussion: is it at the leaf level? at the whole-plant level? This is very relevant

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for the concepts being discussed. Finally, the effect of capacitance should be also introduced.

(4) The sentence ‘The implication of our findings for the quantification of the interactions between Mediterranean vegetation and the atmosphere will be finally discussed, in the frame of the ACCENT-VOCBAS campaign’ at the end of the Introduction is never substantiated.

(5) Overall, the Introduction is too general, and should be streamlined focusing on the specific hypotheses that the authors want to address in their study.

(6) p.1719, l.24-26: why were predawn leaf water potentials not measured? (see below) Please also specify whether the four leaves per species were sampled from different individuals.

(7) A general methodological question is why measurements were not continued after the end of July, as conditions would have been presumably (even) drier and might have highlighted different responses to those observed. It would also be useful to know how the meteorological conditions of 2007 compared to those of an ‘average’ year.

(8) p.1720, l.3-5 and thereafter: The fact that sap flow was not measured for P. latifolia remains an important limitation of the study. Why did you not use another technique, such as the heat balance method, allowing the measurement of small stems? Also, four stems per species is a low sample size provided the variability of sap flow. Finally, it is unclear how sapwood depth was estimated, and how the radial integration of sap flow was achieved.

(9) p.1722, l.19-24: the soil water contents (SWCs) reported in the study are extremely low. Were the TDR probes calibrated using soil from the study site? This is critical in this case as SWCs are used to estimate predawn leaf water potentials (see below) and whole-tree hydraulic conductance.

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(10) p.1724, l.23: you should justify why an exponential function is used instead of the more usual logarithmic fit.

(11) p.1724, l.24-26: it is unclear what changes in the environment on the 20th of June that justifies splitting the data there. Also, from Fig.7 it is not clear that the relative change before and after that date is different for the two species.

(12) p.1725, l.7-12: the results on the radial sap flow patterns should be presented earlier. Also, if I am not mistaken in l.12 it should say ‘the radial pattern DID NOT change’.

(13) p.1725, l.21-27: were your estimates of LA/SA at the branch level as in Martínez-Vilalta et al. (2003)? Otherwise that could explain the observed discrepancy. Please clarify.

(14) p.1726, l.15-16: I do not see how this sentence follows from the previous discussion. Please reword or delete.

(15) p.1726-1728: I found this part of the Discussion confusing and anecdotic. In my view, one of the most intriguing results of this study is the fact that the studied species managed to keep leaf water potentials relatively high while there was basically no water in the soil (Fig.1), suggesting that they had access to deep water resources. However, a proper understanding of this result would require knowledge on the root distribution of the studies species, as well as detailed measurements of predawn leaf water potentials. In this regard, a figure showing the time patterns of the estimated soil water potential would be useful (alternatively, this information could be added into Fig.2).

(16) p.1728: As I have said before, the results of the Alessio et al. (2004) study may provide a good starting point to structure the paper around one or two relevant hypotheses regarding how the study species may respond to drought.

(17) p.1728: the apparent increase in whole-plant hydraulic conductance in P. latifolia

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is intriguing, but the data is not conclusive enough to reach solid conclusions, and the discussion on that point remains highly speculative. To begin with, the estimation of whole-plant hydraulic conductance in P. latifolia is not based on sap flow (as in the other two species) but on leaf-level gas exchange measurements. The authors do not say how many leaves were sampled and, at any rate, they should show that the estimates of whole-plant hydraulic conductance are similar for the other two species regardless of whether they are based on sap flow or leaf-level transpiration.

(18) p.1729, l.7: see comment (11) above.

(19) p.1730, l.13-15: the reference to climate change is far too general to be of interest.

(20) Fig.1: I am surprised that the VPD values are so low, never reaching values $>$1.6 kPa. Do they correspond to average daily values or average daytime values?

Technical corrections: (1) p.1714, l.13: Water potentials should be reported as negative values.

(2) p.1716, l.23: ‘hydrostatic’ instead of ‘idrostatic’.

(3) p.1717, l.7: ‘or A combination of’.

(4) p.1717, l.18: ‘resistant to severe drought’.

(5) p.1718, l.3: give coordinates of the study site.

(6) p.1718, l.23: As far as I know, LiCor instruments are manufactured in Lincoln (NE, USA), not the UK.

(7) p.1719, l.8: delete ‘Moreover’.

(8) p.1720, l.16-17: what is meant by ‘the single species’ in ‘From the total sap flow of each tree, the mean flow per unit leaf area of the single species…’ Please reword.

(9) p.1721, l.7 and thereafter: be consistent with symbols: Gb or gb?

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(10) p.1722, l.1: ‘DID NOT allow for…’

(11) p.1722: The description of statistical procedures is poor. Did you analyze the data as repeated measures? Also, delete ‘p$<$0.05’ in l.7.

(12) p.1723, l.6: please use the past tense when reporting results.

(13) p.1723, l.8: be consistent with nomenclature: SLA or LMA as in Table 1?

(14) p.1723, l.14-15: ‘WITH respect to…’

(15) p.1723, l.20: ‘gas exchange rates’.

(16) p.1723, l.22: delete ‘rather’.

(17) p.1724, l.1: ‘IN mid June’.

(18) p.1724, l.3-5: ‘P. latifolia showed the highest gas exchange rates at the end of the experimental period (21 July 2007) compared to the other two species’ instead of ‘P. latifolia showed, at the end of the experimental period (21 July 2007), the highest gas exchange rates with respect to the other two species’.

(19) p.1724, l.8-9: ‘raised continuously, but with decreasing slope for VPD $>$ 2.8 kPa’ instead of ‘rises continuously, even if the slope decreases at 2.8 kPa’.

(20) p.1724, l.9-10: explain better or delete.

(21) p.1724, l.17: ‘what WAS observed’.

(22) p.1724, l.19: ‘declined BY only’.

(23) p.1725, l.1: there is no Eq. 7. Also, I would say ‘small’ rather than ‘negligible’.

(24) p.1726, l.1: ‘Sanchez-Vilas’.

(25) Fig.8: ‘mmol’ instead of ‘mmoli’ in axis labels.

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