

Interactive comment on “Improved understanding of drought controls on seasonal variation in Mediterranean forest canopy CO₂ and water fluxes through combined in situ measurements and ecosystem modelling” by T. Keenan et al.

Anonymous Referee #2

Received and published: 30 March 2009

General comments

This paper is on modelling the diurnal pattern and seasonality CO₂ and water fluxes in Mediterranean ecosystems, with a focus on modelling the effects of water stress. This is an interesting paper build on the discussion about stomatal and non-stomatal limitation to develop a modelling approach testing both effects at canopy level. One of the main result is that the authors were able to obtain a better fit using a strong non-stomatal limitation. There is a definition problem here that should be corrected because I'm afraid it could spread. Stomatal limitation refers to the decrease of photosynthesis

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due to a decrease in stomatal conductance, not to a change in the slope of the so called Ball-Berry model. I hope the authors understand the difference and can correct in the text (see specific comments below).

Another problem of bigger importance is that soil moisture is a critical variable for this paper and no direct measurement of soil moisture were used in this paper while probably available at most Fluxnet sites. At least, reconstruction should be clearly validated against measurements. It should be noticed that because calculation of the canopy fluxes in the model is decoupled to soil moisture, important feedback between the soil and the canopy are not represented in the model. Figures 5, 6 and 7 should not include evapotranspiration because latent heat is an input to the model: latent heat data were used to model soil water, which was used to model GPP and evapotranspiration. Thus the models were not really validated against evapotranspiration independently. It could be argued that correctly simulating fluxes in Mediterranean forest, both qualitatively and quantitatively, can be done only with a model that is fully validated not only on carbon, but also on water fluxes, because both fluxes are so tightly coupled. In addition, there are some confusion about soil moisture parameters such as RSWC and smax and smin being badly defined. (see comments below).

Separation and analysis of stomatal vs non-stomatal limitation based on eddy-flux measurements is unconvincing (Figure 4). It is already difficult to measure/calculate non-stomatal limitation correctly at leaf level. I doubt that meteorological conditions (radiation and temperature) are strict enough and hold for the whole canopy, including sun and shaded leaves. What about VPD effect? Calculation of C_i at canopy level is oversimplified and could introduce important bias in the calculation of non-stomatal limitation, and especially its seasonality. What about seasonal bias in the partitioning of NEE between GPP and Reco? Figure 4 and data analysis should be discussed thoroughly because it implies many simplification that may introduce important bias. In addition, your results should be discussed confronting leaf level data from the literature because your findings are contradictory to some earlier data, especially from Flexas et

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al. What could explain this discrepancy between leaf and canopy level processes?

The authors should clarify how they parameterized equations 4 to 7. There are multiple procedures to do that: from very empirical to statistical methods including validation on independent data and giving uncertainties about the parameters. The authors should be aware that it is always possible to obtain a better fit by including more parameters and fitting them to data without validation. Thus I'm not surprised that the non-stomatal limitation approach is better because it includes more parameters (W_{fac} for V_{cmax} and J_{max} , thus 6 new parameters). In addition, parameters value should be discussed, especially the q parameter which introduce some non linearity into the model. The authors should also give values of V_{cmax} and J_{max} used as well as any other important parameters of the models. A parameter list would be useful.

A additional comment concerns the need to improve citation to more appropriate references.

Specific comments

Page 2286 Line 26 : Allen (2001) not in the references list Page 2287 Line 1: Boyer (1982) about genotypic selection? This is clearly not the appropriate reference Page 2287 Line 3: Jump et al. (2006) about *Fagus sylvatica* ? This is clearly not the appropriate reference Page 2288 Line 21: I do not agree with this sentence. All models imply that stomatal aperture is affected by soil moisture, but the Tenhunen approach implies that the ratio of g_s to photosynthesis changes with soil moisture. Page 2289 Line 8: Warren (2008) is not the appropriate citation here, there are much earlier paper discussing that. Page 2289 Line 14: What about papers from Grassi, Galmés. . . Page 2290 Line 2: Please give a short justification of using these 2 models. Page 2290 Line 20: Wofsy et al. (1993) is not appropriate within this context. Page 2291 Line 4: Obviously, soil moisture is a critical variable for this paper. Direct measurement of soil moisture is done at all Fluxnet sites and should be used here. At least, reconstruction should be validated against measurements. Page 2291 Line 12: How can you assume

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that soil evaporation was negligible? Why the authors didn't use sap flow measurements, which is probably better than latent heat flux for this particular purpose. Page 2293 Line 2-15: Separation of stomatal vs non-stomatal limitation based on eddy-flux measurements is unconvincing. What about seasonal bias in the partitioning of NEE between GPP and Reco? Calculation of C_i is oversimplified. It is already difficult to measure/calculate it correctly at leaf level. I doubt that meteorological conditions are strict enough (radiation and temperature). What about VPD? Page 2296 Line 7: The first hypothesis is badly explained. Stomatal, conductance should always decrease with water stress. I guess the authors modified the ratio between g_s and photosynthesis. Same for photosynthesis, I guess the authors reduced photosynthetic capacity or mesophyll conductance, not photosynthesis alone. Page 2297 Line 4: How did you parameterized these functions? There are multiple procedure to do that: from very empirical to statistical methods giving uncertainties about the parameters. Page 2297 Line 7: Please be more specific about which parameters were calibrated ? Page 2297 Line 7: What are the exact meaning of s_{max} and s_{min} ? Are they related to field capacity and wilting point? Are they related to relative water content or relative water extractable? Page 2297 Line 7: What is the meaning of q ? Why do you expect the relationship to be non linear ? Is there any problem with overfitting ? Page 2298 Line 9: This approach is not appropriate because (1) the reconstruction of soil moisture the 4 sites was not properly validated, (2) I would argue that correctly simulating fluxes in Mediterranean forest, both qualitatively and quantitatively, can be done with a model that is validated not only on carbon, but also on water fluxes, because both fluxes are so tightly coupled. Page 2298 Line 24: Please give the date for the golden days periods at each site. Page 2299 Line 11: Please give your definition of RSWC. Is it the same definition as for example Granier et al. (1999)? Page 2300 Line 1 : The authors probably should contact PI's to get the data in order to validate your model. Page 2300 Line 5: The authors are wrong with the definition. Changes in the slope is related to change in water use efficiency, not stomatal limitation. Page 2300 Line 12: This statement lacks of support. I do not understand the reference to Figures 2a and 2b. I do not see any

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figures showing the seasonality of the slope. Page 2300 Line 17: This statement lacks of support (figure, table, ...). Page 2300 Line 21: How did you asses water stress ? I do not understand your reference to water stress? Did you use data of leaf water potential not shown in the paper ? Page 2300 Line 26: In Figure 4, proportional soil water is similar to RSWC of Figure 2. Why there are no value <0.2 ? How did you calculate the relative change in A_n ? Page 2300 Line 26: What are the units of parameters in Table 2, especially s_{max} and s_{min} ? The values of s_{max} and s_{min} look dubious. How these value are related to maximum soil water holding capacity or soil water at field capacity? Page 2301: Figures 5, 6 and 7 should not include evapotranspiration because latent heat is an input to the mode. In fact, latent heat data were used to model soil water which was used to model GPP and evapotranspiration. I don't think the models were validated on photosynthesis and evapotranspiration. Page 2301 Line 22: Figure 5 and 6 do not show transpiration data, but evapotranspiration data (latent heat). Page 2303 Line 20-25: Where is it possible to see that? The quality of the graph does not allow the reader to see if the results support this statement. Page 2303 Line 20-25: Concerning the overestimation of evapotranspiration and its after effect on water stress, how the authors tested for this effect? In reality, there should be a feedback between evapotranspiration (or transpiration) and soil water content so that trees conserve soil water, especially when water is scarce. Obviously this feedback, which is probably a important feature of Mediterranean ecosystems, is no represented in your modelling approach because soil water simulation is decoupled to canopy fluxes. Page 2306 Line 8: Loretto and Centritto (2008) is probably not the appropriate reference. Pahe 2306 Line 12: Flexas et al (2008) reported very fast changes in mesophyll conductance. Page 2308 Line 1: Figure 4 and data analysis should be discussed thoroughly because it implies many simplification that may introduce important bias.

Interactive comment on Biogeosciences Discuss., 6, 2285, 2009.