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Biogeosciences Discussions

Interactive comment on "Improved understanding of drought controls on seasonal variation in Mediterranean forest canopy CO_2 and water fluxes through combined in situ measurements and ecosystem modelling" *by* T. Keenan et al.

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The paper by Keenan et al. (bgd-2009-3) has been assessed by two reviewers and both raise a number of critical concerns and recommend that major revisions will be necessary before the paper becomes acceptable for publication in Biogeosciences. A major issue is the lack of validation for the soil water content simulations, on which both reviewers comment, and I would like to reinforce their point – this is a no-go. The authors will have to show how well their water balance model is able to simulate the seasonal course of soil water content at these sites and discuss the uncertainties

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Interactive Discussion

Discussion Paper

introduced by not using measured, but simulated soil water content. Other reviewer comments relate to a lack of clarity regarding model parameterisation, model description, the motivation for using two models and a critical assessment of the EC data quality.

I mostly agree with the reviewers comments and in addition have several additional concerns regarding the paper, as listed below. It thus appears to me that fundamental changes will be necessary in order to make the paper acceptable for publication in Biogeosciences. Should the authors decide to submit a revision, it should be accompanied by a detailed point-by-point reply to the reviewers and my comments – given the considerable nature of changes to the paper which are called for I plan to have any revised paper be checked by the same reviewers again.

Editor major comments: My major concern relates to how Gc is calculated (Eq. 1) and how the parameterisation of Gc (Eq. 2) is transferred into the models. First, in Eq. 1 the units do not make sense and I do not really understand Eq. 1 because using Fick's law (which the authors seem to do) in my view Gc could be calculated simply as:

Gc = LH * P /(VPD * lambda)

LH . . . latent heat flux as measured by eddy covariance (J/(m2s)) P . . . atmospheric pressure (kPa) VPD . . . vapour pressure deficit (kPa) Lambda . . . latent heat of vaporisation (J/mol; 44100 J/mol @20degC) Gc . . . bulk conductance to water vapour (mol/(m2s))

Second, I do not understand why the authors mix the aerodynamic, quasi-laminar boundary layer and surface (stomatal) conductance into what they refer to as a bulk conductance. This is unnecessary as the surface (stomatal conductance) could be separated from the aerodynamic and quasi-laminar boundary layer conductance, giving a sort of big-leaf equivalent to leaf-scale stomatal conductance. This bears a conceptual problem, as the aerodynamic and quasi-boundary layer conductances are not under plant control, unlike the stomatal conductance. If the controls (wind speed, friction velocity, atmospheric stability) on the aerodynamic and quasi-boundary layer conductance change with drought, this may bias the bulk conductance independent of stomatal control. In this context I am questioning the usefulness of the bulk conductance for parameterising the models, which very much likely (although this is not entirely clear to me from the paper) scale up ‘pure’ stomatal conductance to the canopy level, e.g. by accounting for sunlit and shaded fractions of the leaf area. If this is so, there is a mismatch in scale between what is derived from measurements and used to develop the parameterisation, and model structure. Third by using the VPD the authors assume the evaporating surface to be at air temperature, which is unlikely to be the case, in particular during conditions of low evapotranspiration. This problem could be overcome by using the Penman-Monteith combination equation for deriving Gc – in this case usually the aerodynamic and quasi boundary layer conductances are separated. Fourth, the authors should assess and discuss the effects of any energy imbalance and thus a potential under/overestimation of LE on their conductance calculations, in particular if the energy imbalance changes with drought conditions, which might be the case – I have a paper in press at AFM on this issue which I would be happy to share with the authors.

Editor minor comments: (1) p. 2292, l. 8: the BB model uses Cs which is the CO_2 mole fraction at the leaf surface, that is within the leaf boundary layer (2) p. 2292, l. 14: “conductance to water vapour” (3) p. 2292, l. 23: with 3 free parameters, Eq. 2 is non-linear (4) p. 2293, l. 2-9: how sensitive are the results to your data screening procedure ? (5) p. 2293, l. 12: you have measurements of NEE and estimates of GPP, but not of net photosynthesis, which would be GPP minus autotrophic respiration (6) p. 2293, l. 20: what does “close to” mean ? I think it will be extremely important to critically check data with regard to rainfall events as these may cause relatively short-lived respiration pulses (7) p. 2300, l. 15-16: shouldn’t this be Fig. 3a and 3b ? (8) Fig. 3: how can you physiologically justify negative intercepts ? (9) Fig. 5 legend: “20 wet and dry” golden days ?

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Interactive Comment