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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.

T. Keenan

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Reply to Anonymous Referee #2

Many thanks for the time taken to assess this manuscript. We agree with the points which you raised, and believe they have led to a good improvement in the manuscript. Below we respond to your main questions.

1. 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 due to a

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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).

Authors: The referee's point is well taken, and this will be corrected in the text in the revised manuscript.

2. 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.

Authors: Please see response to similar comment from Referee #1.

3. 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).

Authors: We apologize that this has been poorly explained in the manuscript. As the referee states, the feedback between the soil and the canopy is essential to properly understand forest carbon and water fluxes in the Mediterranean. We have modified the text to explain that the reconstructed soil water content was only used for the calculation of the model parameters, and parameter responses to changes in soil water content. Evapotranspiration is used as a prognostic variable, forcing soil water content in the

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simulations presented in Fig. 5, where the different approaches to introducing water stress responses in the Farquhar-Ball-Berry model are assessed. We feel that it is valid to present evapotranspiration in this figure, because, given that the canopy is uncoupled from the soil, it is free to under- or over-estimate evapotranspiration, but this does not cause a feedback or compounding of errors for the next time-step. Latent heat is not an input for the simulations shown in Figs. 6 and 7, - these are 'free' simulations and a full bi-directional feedback between the canopy and the soil is considered. We have clarified the experimental design in the manuscript.

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

Authors: See response to the Editor comment regarding the calculation of canopy conductance and the subsequent C_i concentration calculation. We understand your concerns regarding Fig. 4, and have included discussion of the points raised.

5. 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

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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.

Authors: The parameterization of equations 4-7 will be clarified in the text. We understand your concern regarding the effect of the inclusion of more parameters, but disagree that in this case the inclusion of more parameters '(W_{fac} for V_{cmax} and J_{max} , thus 6 new parameters)' will influence the effectiveness of one approach compared to the other. The W_{fac} for V_{cmax} and J_{max} is the same parameter and acts on either V_{cmax} or J_{max} depending on which is being used to calculate photosynthesis for a given hour (photosynthesis is calculated on either a carbon (V_{cmax}) or light limited (J_{max}) basis). Therefore there is no change in the number of processes treated, or parameters used. This will be clarified with further discussion regarding parameter values in the revised manuscript.

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

Authors: O.K., Thank you for pointing out this omission. We will include parameter values or relevant references in the revised manuscript, and improve citation as suggested.

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