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**Geoscientific
Model Development
Discussions**

Interactive comment on "MAESPA: a model to study interactions between water limitation, environmental drivers and vegetation function at tree and stand levels, with an example application to [CO_2] $\times$ drought interactions" *by* R. A. Duursma and B.E. Medlyn

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First, we would like to point out that we were not given the opportunity to respond to the reviewer's comments in the review process for a different journal. We welcome the opportunity to address the reviewer's concerns. As we argue below, the major points raised by the reviewer are based on a misunderstanding of the value of the new model (MAESPA), as compared to the existing MAESTRA model. A few minor points are also

discussed.

Reviewer : "One comment I would like to add/emphasize from my original review (below) is that the authors should identify what the novel use of this model is over MAESTRA. In other words, MAESTRA can be used to study interactions between water limitation and CO2 in its current form. As a model description paper, the authors should describe the novel aspects of this model over the version of MAESTRA that already responds to water limitation and CO2 (please see details in review below)."

Response : This comment rests on either a misunderstanding of the capabilities of Maestra, or a misreading of our manuscript. Maestra does not simulate the water balance, as it does not calculate the soil water content based on transpiration, rainfall, and so on. As such, it is not able to perform simulations as we have shown in Figure 4. It is possible to 'prescribe' the soil water content in the existing Maestra implementation, combined with an ad hoc dependence of stomatal conductance on soil water availability. Therefore, it is incorrect that "MAESTRA can be used to study interactions between water limitation and CO2 in its current form".

Reviewer: (related to the above comments) : "They should have at least compared and contrasted their new model with some of that work to justify why they needed a multilayer soil moisture model and how their estimates improved over those of Reynolds et al. (2009) and/or others that accounted for soil moisture within MAESTRA."

Response: The study by Reynolds et al does not simulate the water balance, because Maestra is not able to simulate the water balance. They simply (as other similar studies) use soil moisture as a prescribed driving variable, which does not capture the feedback of water use on soil moisture availability. It is therefore not possible to do any of the simulations we present with the existing Maestra model (response to 'need for multilayer soil model'; see below).

Reviewer: (related to the above comments) : , "In the discussion the authors state "All currently available soil-plant-atmosphere models can only be applied to entire canopies, restricting their use to studying stand-level processes." The statement is simply not true and the paragraph is very misleading. As an example, MAESTRA with the current soil moisture function can be applied at the individual tree level and scaled up and several studies exist in which it was validated at the individual tree level and/or stand scale." AND "One could already perform very similar dry down and drought times atmospheric CO2" interaction "simulations" with the current form of MAESTRA that already includes a soil moisture stress function and the ability to change atmospheric CO2".

Response: Our statement in the Discussion is true, since Maestra does not calculate the water balance (in other words, Maestra is not a soil-plant-atmosphere model, because it only includes a very rudimentary soil component). We are not aware of any existing models that can be applied to single plants (possibly growing in pots) and stands of plants, and calculate the soil water balance and provide the feedback from soil moisture to plant processes.

Taken together, these separate comments from the reviewer indicate a misunderstanding of both the MAESPA model, and the MAESTRA model on which it is partly based.

Reviewer: "the most significant flaw of the manuscript (lack of adequate validation data)."

Response: It is true that we don't present a full validation of the model, this is planned for a future contribution. As the journal 'Geoscientific Model Development' welcomes manuscripts that describe models, we feel this is an appropriate contribution. Moreover, as we argue in our manuscript, a complex model such as MAESPA can be successfully used to improve our quantitative understanding of interactions between driving variables, and plant responses. We include an example on CO2 x drought in-

teractions, study the effects of plant acclimation, and conclude that we were able to further our understanding of a published experiment. This exercise in the manuscript goes much further than to simply say 'the model was validated', but this aspect of our manuscript was not commented on by the reviewer.

Reviewer: "Plus, the authors fit their model to a data set with no independent data for validation (of course the model will work well if you fit it to the data).".

Response: as explained in section 2.2.3, "Using the fitted parameter set for aCa plants, we simulated water use by the eCa plants, with the only difference that leaf area was increased as observed in the experiment." Therefore, we did NOT modify the parameters to fit the eCO2 treatment as well. If the model would give a wrong CO2 response, we would find a very bad fit to the eCO2 data, and as such, we did have an independent test (although we agree with the reviewer, more thorough comparisons would be needed).

A related comment, "Overall, the authors present modifications with very little validation and no confidence it will work beyond their case study." This is somewhat inconsistent with the reviewer's notion that we have not added anything new, but simply combined a few existing models. The modifications we have made are (nearly) for the purpose of easier parameterization, while keeping the original biophysics of the model component intact. For example, SPA uses a different scheme to parameterize the soil water retention curve, we have chosen for a more flexible routine (the Campbell equations). Both methods can yield very similar parameterized equations, but differ only in how the user specifies the input.

Furthermore, we argue that it is important, not only to validate against data, but also to examine the behaviour of the model under pre-determined conditions to see if it is consistent with expectations. For example, Tuzet et al. (2003) did not validate their model, but rather explored its behaviour in response to a drying cycle. Our simulations serve a similar purpose – to illustrate that the model behaves as we expect given the

assumptions underpinning it. You could argue that this kind of exploration of model behaviour can actually give more confidence in a model than simple validation against data – because available data may only correspond to relatively small regions of model space.

Reviewer: - "In the introduction the additional complexity of incorporating the multi-layer rhizospheric soil moisture model is not justified, especially not when one considers that several other models already have similar functions (e.g. Baldocchi - CANVEG; Tuzet et al., 2003)."

Response: We agree that we should have included a better justification of this complication, and we will add this to the next version of the manuscript (there are two reasons: a multi-layer soil gives a more realistic response to wetting events that only saturate the top layer of the soil, and a multi-layer soil model allows vertical variation in soil texture and rooting density).

Neither the CANVEG model (as described by Lai et al 2000, JGeophysRes. See also statement by Katul et al. 2003, "because CANVEG does not consider the soil–root–xylem hydraulics") nor the Tuzet et al (2003) model have implemented a multi-layer soil, and neither model calculates the soil water balance (in both cases, soil moisture is used as a driving variable only).

Reviewer: "In addition, they make no mention of why SPA by itself (already an atmospheric process model coupled to a multi-layer rhizospheric soil moisture model) is inferior in comparison to the new MAESPA". - And "...what is deficient in the SPA atmospheric processes as compared to MAESTRA [?]."

Response: As explained in the third paragraph of the introduction, and again in the Discussion, the novelty of MAESPA is that it can be applied to single plants as well as canopies (and for canopies, including non-homogenous canopies). We did not point out explicitly that SPA does not do this (although it was implied), we will make this more explicit in the next version of the manuscript.

Reviewer: "The authors say on lines 41-46 of page 10 that MAESPA calculates the soil water balance for the entire stand at once. The stand in Figure 2 and 3 would use different amounts of water per tree even if they are all the same species because the tree sizes have different amounts of leaf transpiring surface area. This would create a heterogeneous soil moisture situation in both the vertical and horizontal direction. I don't see how MAESPA could handle this because it does not account for horizontal water flow. This is a critical flaw..."

Response: We do not agree this method is a 'critical flaw'. Our method simply averages the soil water uptake by trees in a prescribed area of the stand (or the entire stand, or only one of the trees at a time, given the input settings), to estimate the average soil water content in that area. We recognize there would be spatial variation in reality, but it is beyond the scope of MAESPA to estimate this variation, since there are rarely ever measurements taken at a fine spatial scale to validate a simulation like that. In essence, our method is no different from one-dimensional soil-plant atmosphere models, that aggregate the canopy into one 'homogenous' layer, and calculate the soil water balance based on this average canopy. We first calculate water use by single trees, and then average those to calculate the soil water balance (note that, due to many non-linearities, the two would not give the same results).

We do admitagree that this point deserves more clarification., and this will be addressed in the revised manuscript.

Reviewer: "Lastly, the authors minimize the amount of information needed to run the soil water balance by tucking those parameters in an appendix table. Since the ability to handle the soil water balance is a major thrust of this manuscript, the authors should discuss how practical it would be to obtain the variables listed in Appendix B and move that table to the main body of the manuscript."

Response: The appendix table was simply meant as a summary of variables that we think are the most crucial ones that users should focus on. We feel it leads away from

the purpose of the paper to explain how to measure these parameters, since that would require much space. A future contribution on the application of MAESPA will address some of these issues. We can however make the appendix into a regular table, as we agree that that would be helpful.

Interactive comment on Geosci. Model Dev. Discuss., 5, 459, 2012.