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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

Anonymous Referee #1

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Full disclosure, I have reviewed this manuscript once before when it was submitted to a different journal. I agreed to review it a second time because in the previous review, the other anonymous reviewer and I independently agreed on the most significant flaw of the manuscript (lack of adequate validation data). 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). After reading the current form of the manuscript, I have found that the authors did not change anything at all. Since I have already taken a lot

of time to read the manuscript several times and comment on it, I have included my original review below because after reading the manuscript and my review again, I still believe that each of the suggested changes are valid. 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).

Original Review This manuscript presents an interesting revision of MAESTRA and SPA that allows the atmospheric processes of MAESTRA to be coupled with the rhizospheric soil moisture processes of SPA. Above and belowground processes are therefore both accounted for at multiple layers. In addition, a rain interception, additional stomatal conductance sub model, and soil precipitation function are brought in from SPA and other sources.

Major points I see several major deficiencies that should be addressed before this manuscript could become acceptable for publication in Ecological Modelling. First, I must note that the improvement of combining the two models is mainly on the programming side. Therefore, I don't see much scientific contribution from the current results (I explain in more detail below).

The manuscript does not really present any mechanistic advances compared to the previous components of each independent model, either aboveground (MAESTRA) or belowground (SPA) portions. For example, a sub model for stomatal conductance is introduced that could be construed as more mechanistic, but it is already published elsewhere and also used in a similar whole plant model (Tuzet et al., 2003 PCE). In fact, each new sub model addition in this manuscript (multi-layer soil moisture, rain interception, stomatal conductance model, and soil precipitation function) that might justify naming the model MAESPA is already described and published elsewhere. Second,

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the authors fail to set up why they undertook such an improvement (we have to assume it is an improvement). 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). Along these same lines, they fail to mention other works that accounted for soil moisture within MAESTRA with a soil moisture feedback on stomatal conductance (e.g. Reynolds et al. 2009). The authors list this reference and other publications that deal with soil moisture within MAESTRA on their MAESTRA model web site bibliography, so they know the work exists and it seems very relevant to this manuscript. 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. 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. Moreover, SPA has been validated at various scales and it would be useful to know what is deficient in the SPA atmospheric processes as compared to MAESTRA.

Most importantly, there is a severe lack of validation data in this manuscript. The only attempt at validation is presented in Fig. 6 and does not show appropriate statistics (e.g. Root Mean Square Error and R2) to compare measurements to model estimates. The authors choose instead to jump right to simulations rather than build confidence in the new predictive power of MAESPA. Overall, the authors present modifications with very little validation and no confidence it will work beyond their case study. For instance, the authors state that MAESPA can simulate carbon and water fluxes well when compared to field measurements. However, the validation data is severely lacking or nonexistent (there is not any for carbon). To justify the new sub models a revised manuscript should focus on the validation of MAESPA against measurements at various scales as opposed to describing the soil moisture model (the components of the model have already been published, so why describe them again). Specifically, addi-

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tional measured versus simulated examination of the effects of drought stress would be essential to supporting the integration of the two models. Plus, without this validation it is not known if bringing all four of these items together (multi-layer rhizospheric soil moisture model, rain interception, new stomatal conductance model, and soil precipitation function) into what was MAESTRA might make the estimates inaccurate due to unforeseen interactions among the newly introduced sub model components.

Other points 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, especially knowing that the authors say on the bottom of page 22 and top of 23 "Canopy transpiration is estimated based on transpiration by the sample trees, and it is therefore vital that these sample trees represent the canopy in terms of water use."

Eq. 9 is not defined very well.

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.

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.

Lastly, the authors minimize the amount of information needed to run the soil water

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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.

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