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Biogeosciences Discussions

Interactive comment on "An observational constraint on stomatal function in forests: evaluating coupled carbon and water vapor exchange with carbon isotopes in the Community Land Model (CLM 4.5)" *by* Brett Raczka et al.

Anonymous Referee #2

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Raczka et al. present an interesting attempt to link carbon isotope data with the function of CLM4.5. I have listed a few thoughts below:

- I found the introduction very clear but I wonder if there is any other literature on how other models have used isotope data? I realise the authors suggest this is the first time it has been attempted in CLM and I realise this paper is primarily targeted at the CLM community, nevertheless I think my one concern would be the lack of literature in relation to other models and isotopes? Currently it is largely non-existent.

- Equation 1: I don't think you mean Respd = dark respiration. Rdark is not the same

as day respiration/respiration in the light. Suggest the use of Rday or Rd.

- Equation 4: I'm pretty sure that "Bt" should be applied to your slope term "m", rather than the minimum stomatal conductance, b? Can you please check you have this correct. Certainly if you do then this would be a striking departure from most other models.

- Line 23: "tree canopy" is this only true for trees, what happens with grasses in the model? If not, perhaps delete tree and leave just canopy.

- Equation 8 & 9: it would be helpful to the reader to explain where the numbers 4.4, 22.6 and 1000 come from, or what conversions they apply to.

- Century model (line 26/27) should have a reference.

- I'm not sure what the length of the paper was but the results/discussion text did feel very long? Similarly the conclusions runs to nearly two pages. This seems excessive to me. I'm fairly confident there are cuts that could be made to the text which would make it more digestible to the reader. I certainly found myself losing track during my reading and I think this is the key area which requires editing during revision.

- The authors note: "the overestimation of discrimination may suggest the stomatal slope in the Ball-Berry model (m=9 in Eq. 4) used for these simulations was too high." While it is may be true that the slope parameter is poorly informed by site data, the logic of this conclusion in itself may not be valid. Isotopic measurements *should* give lower slope values than those one would infer via leaf gas exchange data (i.e. the data used to inform the Ball-Berry model). This is because leaf gas exchange measures the resistance from the intercellular spaces (Ci), whereas isotopes measures the resistance from the chloroplast (Cc). I see no mention of this in the text and caution against the authors potential drawing the wrong conclusion from the model-data discrepancy.

- In discussing the "limited nitrogen formulation", the authors note: "In general, there were no categorical differences in behavior between these two classes of models dur-

ing CO2 manipulation experiments held at Duke forest and ORNL (Zaehle et al., 2014). CLM 4.0 was one of the few models in that study to consistently underestimate the NPP response to an increase of atmospheric CO2 due to nitrogen limitation, however this finding was attributed to a lower initial supply of nitrogen." This is not strictly true. As part of the same model-data inter-comparison of the models to the data at the two FACE sites, De Kauwe et al. (2013, Global Change Biology), found no support for the implementation whereby assimilation is limited by nitrogen availability, but not stomatal conductance. They concluded: "Stomatal conductance data from both sites were used to test modelled leaf-level responses. The simple stomatal conductance model (Eq. 1) fitted the data well (Fig. 6), supporting the assumption of coupling between assimilation and stomatal conductance. Importantly, at the ORNL site, N content of the foliage declined strongly over the course of the experiment (Norby et al., 2010), but neither the slope of the stomatal model, nor the response of A/gs to CO2, was altered by this decline (Fig. 6b). These data indicate that the coupling between stomatal conductance and assimilation is not affected by N-limitation (Fig. 6b). The data therefore tend to support coupled models over uncoupled, or partially coupled, models such as DAYCENT and CLM4." Furthermore, I would question if there is any evidence that plants follow the "limited nitrogen formulation"?

- Phrases like "The relative high simulation skill" or "CLM was able to accurately" need some quantification. There are a number of similar cases dotted around the manuscript.

- Figure 2. I realise that a strength of this paper is the long timeseries; however, showing $\sim$15 years of data like that isn't particularly instructive. It is hard to distinguish the model-obs differences. Perhaps average a day/week or monthly climatology across years would more clearly show differences. This figure could also be kept, perhaps one could go to the supplementary.

- Figure 8e. I find it hard to believe that there is no reduction in the soil moisture availability factor during the whole of the summer? This seems unlikely to me? Could

this please be checked?

- Figure 9. I would suggest the symbol sizes could be reduced, they seem a little large for the figure panels.

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