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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 #1

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The manuscript by Raczka et al. provides an extensive description of how carbon isotope discrimination is represented in the CLM land surface model and how it can be used as a constraint for evaluating model performance. The model evaluation conducted in this paper provides valuable insights into the representation of physiological processes in process-based models, including potential improvements with regard to model structure and parameterization. This information will be useful to the wider land surface modeling community. Further improvements to the manuscript should include a few clarifications, in particular on how N limitation is implemented in the model. I have listed detailed comments here:

- The abstract could explicitly mention that three different N-limitation formulations were tested in the model. This information is worth to mention, but somewhat hidden in the abstract. E.g. it would make sense to shortly explain what the "alternative nitrogen limitation" formulation in line 29 actually means. To compensate for the additional number of words one could shorten the Vcmax calibration description or try to focus on a few key outcomes.

- The original source of the Ball Berry model (Ball et al. 1987) should be acknowledged. Further, hs represents relative, and not specific humidity at the leaf surface. The definition of hs is unnecessary.

- ci is intercellular, not intracellular CO2 partial pressure, unless you consider mesophyll conductance, which seems not to be the case.

- Equation 13: what does ET represent? In Table 1 it is listed as leaf transpiration, but that clearly doesn't make sense here. But I wonder if it is ecosystem transpiration or evapotranspiration?

- Table 1: please check the unit for iWUE, it shouldn't be gC gH2O-1. Add CO2 and O2 for the Michaelis Menten constants.

- In Equation 14, I assume you mean "An" rather than "A". Please clarify.

- Please provide latin names for the dominant species at the site.

- Equation 8: please state where the 4.4 and 22.6 come from and which of those represents fractionation due to diffusion and Rubisco. I am not sure if this is clear to all readers.

- 2.1.2: The comparison of different versions of how nitrogen limitation is implemented in the model and its implications is a very interesting aspect covered by the manuscript. Unfortunately, the three different formulations tested (unlimited N, limited N, no downregulation discrimination) are described in a rather confuse way, and I doubt that it will comprehensible for all readers. I strongly encourage the authors to include the overview figure that they have shown in an earlier comment and I recommend a better explanation of Equation 7. How is N-limitation determined? This is mentioned in the Figure caption, but one could also include this in the manuscript as well. Further, the terms “potential” and “actual photosynthesis” are mentioned on page 7, line 17f, but they haven’t been defined before, and they aren’t common terms either. In the standard (= limited nitrogen) version, is photosynthesis first calculated without N-limitation, then N-limitation calculated according to Equation 7, and then the actual photosynthesis calculated by An*(1-fdreg)? But then, how is it possible that a reduction in An caused by N-limitation does not feedback on gs? This should be the case considering Eq. 4. The approach becomes clearer after reading section 3.3, but it would be helpful to explain it better at this point.

- 2.2 State here that NEE and other fluxes are observations based on the eddy covariance method. Please clarify here that the NEE partitioning was conducted using two different methods, and briefly mention their approach.

- P.10 line 27ff: that’s a very detailed description which seems unnecessary to me. One could shorten this part or omit completely.

- Same is true for the last sentence in 2.3 and the first sentences of 2.4., where many technical and CLM-specific details are mentioned that one may consider to omit, as they are of lesser interest to the wider community.

- 3.1.1 what caused the overestimation of LAI, GPP etc. in the uncalibrated simulation? I presume the default value of Vcmax was too high?

- Figure 2: I think it would make more sense to show a mean annual course of the three variables rather than the complete time series. The way it is now makes it hard to see by how much GPP, ER, and LE differ from the observations on average. An interesting aspect is the underestimation of WUE. Is this more related to evaporation or transpiration? In the latter case this would be strongly related to the stomatal slope parameter “m” in the Ball-Berry model (see later comment), but could have other reasons as well.

One could shortly comment on this, up to the authors.

- P.15 line 15: the fact that stomatal conductance responds to atmospheric CO_2 is long known. I suggest citing an earlier study that showed this.

- P.16 line 13: Please make sure that the iWUE trend reported in the studies cited here refer to the same time period. Over which timespan did the 15-20% increase in iWUE occur according to these studies?

- 3.2.1 you state that "…this trend imposed by iWUE can be neutralized by increasing ca." Firstly, what trend do you mean? The one in ci/ca? Secondly, I am struggling with the logic of this sentence, since the principal effect of rising ca is stomatal closure, which increases iWUE. So how can ca counteract this at the same time? Doesn't that depend on how strong stomata respond to ca, as you have mentioned at the beginning of the section? This on the other hand is strongly controlled by the stomatal model used. The Ball-Berry model predicts a proportional decrease of gs with ca and a constant ci/ca. Please clarify this argument, in particular the role of ca for iWUE. I'm also wondering why the effect of mesophyll conductance is not discussed at this point, even though its importance is underlined in one of the studies you have cited (Seibt et al. 2008)? What would change if it was explicitly considered?

- 3.2.2 The idea that the stomatal slope may be too high for the site is interesting. Indeed a recent compilation of this parameter (Lin et al. 2015, Nature climate change) showed significantly lower values for coniferous evergreen forests than for other vegetation types (note that the study uses a slightly different model, and that the slopes cannot be compared 1:1, but they should vary in the same manner). One could cite this reference and point out that there is a biological explanation for why the slope should be lower for coniferous vegetation compared to other vegetation types. One could further explicitly mention that a lower stomatal slope would also give a lower stomatal conductance for a given An, and thus reduce the model-observation mismatch. Note that this would also affect Vcmax.

- Section 3.3 is very interesting, but I wonder if there is some more information on why one approach should be preferred over the other? Here you show that the limited N formulation is inferior to the others, which is nice, but is there also some biological evidence for this? What I mean is that the one reference you cite here (Zaehle et al., 2014) could be backed up by other (non-modeling) studies.

- Conclusions: You state that the isotope measurements suggest a lower gs than the flux tower measurements. I'm not sure if I agree with that, since you didn't derive gs directly from the eddy covariance measurements, but rather used the Ball-Berry model with an uncalibrated stomatal slope to model gs. So if your stomatal slope parameter is inappropriate for the vegetation at the site, then your gs will be as well, but that can't be directly related to the eddy covariance data.

- Figure 1: what do the lines prior to 1850 represent? Is it necessary to show them here?

- Figure 8: in Panel A it says fractionation in the heading but discrimination in the caption. Please stick to one. Why didn't you show iWUE here?

- Figure 9: could be helpful to add sub-headings on top of each row indicating the N-formulation used.

Technical corrections: - I suggest mentioning the FLUXNET ID of the site (US-NR1) - P.9 line 16: Max Planck Institute for Biogeochemistry - P9, line 18: remove brackets - P.5 line 13: remove brackets - Omit sentences like "the source code was modified..." - The horizontal lines of the error bars seem a bit overdimensioned - P. 21, line 19: "through", not "though"

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