

## Initial Response to Referee and Other Comments

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)

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Thank you to the referees, editor and Dr. Keeling for their time and thorough reviews. A combined response to these comments are below.

The most impactful comments by the reviewers in our opinion are related to the potential impact of mesophyll conductance upon the simulations. Please see our responses to the reviewer questions related to this topic. (Referee 1.4, 1.20, 1.23, 2.8).

Second in importance, we agree with the reviewer comments that a figure devoted to tracking carbon flow and distinguishing between the different model formulations would be useful. Please see the responses to reviewer questions related to this topic. (Referee 1.10, Ralph Keeling 2).

All suggested or proposed changes as described below are already implemented in a revised version of this manuscript. If given the opportunity, we plan to submit the revised manuscript to Biogeosciences.

On behalf of all authors,

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Response to comments of Referee 1:

Referee 1.1: 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.

Author: Thank You

Referee 1.2: 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  $V_{cmax}$  calibration description or try to focus on a few key outcomes.

Author: We propose to clarify this by using the 'pre-photosynthetic' vs. 'post-photosynthetic' terminology used later in the manuscript. We believe this would improve upon the 'alternative nitrogen formulation' description, but leaves the necessarily detailed description for the main body of the manuscript (too long for abstract).

Referee 1.3: The original source of the Ball Berry model (Ball et al. 1987) should be acknowledged. Further,  $h_s$  represents relative, and not specific humidity at the leaf surface. The definition of  $h_s$  is unnecessary.

Author: We intend to add this citation. We will also correct  $h_s$  to read: relative humidity. Thanks for catching this.

Referee 1.4:  $c_i$  is intercellular, not intracellular  $CO_2$  partial pressure, unless you consider mesophyll conductance, which seems not to be the case.

Author: Thanks for catching this terminology mistake. We will correct this, and we intend to add discussion that highlights that CLM ignores mesophyll conductance, and discuss the implications.

Referee 1.5: 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?

Author: We intend to change  $E_T$  to ecosystem transpiration in Table 1.

Referee 1.6: Table 1: please check the unit for  $iWUE$ , it shouldn't be  $gC\ gH_2O^{-1}$ . Add  $CO_2$  and  $O_2$  for the Michaelis Menten constants.

Author: The units for  $iWUE$  should be changed to  $\mu mol\ C\ mol\ H_2O^{-1}$  and we will add  $CO_2$  and  $O_2$  for the constants.

Referee 1.7: In Equation 14, I assume you mean "An" rather than "A". Please clarify.

Author: We mean "A" or gross assimilation as opposed to net assimilation "An" which includes a leaf respiration term. To clarify we intend to define "A" more clearly in the methods and include a definition in Table 1 as well.

Referee 1.8: Please provide latin names for the dominant species at the site.

Author: We will add the latin names.

Referee 1.9: 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.

Author: We intend to make this clear in the text. The values 4.4 and 22.6 represent the diffusional and enzymatic contributions to isotopic discrimination during photosynthesis.

Referee 1.10: 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 be comprehensible for all readers. I strongly encourage the authors to include the overview figure that they have shown in an earlier comment

Author: We intend to add a new figure (similar to what was proposed and submitted to the interactive comments session). It will track the flow from photosynthate to allocation of biomass. It will also pictorially, show the role of the nitrogen cycle in the downregulation of photosynthesis.

Referee 1.11: 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  $A_n \cdot (1 - f_{dreg})$ ?

Author: This will be better addressed by adding a new figure as proposed in the comment by Referee 1.10. Also we will add two new equations that define how potential GPP is calculated from  $A_n$ , and that potential GPP is downscaled with  $f_{dreg}$ . We left these out previously for conciseness.

Referee 1.12: How is it possible that a reduction in  $A_n$  caused by N-limitation does not feedback on  $g_s$ ? 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.

Author: ‘ $A_n$ ’ (leaf-level photosynthesis) does not undergo a reduction from N-limitation, only GPP (ecosystem photosynthesis). This will be much clearer with the addition of the new equations proposed within Referee 1.11. In general, the fact that N-limitation does not feedback on  $A_n$  and  $g_s$ , makes CLM a ‘partially’ coupled model. We intend to add discussion to this effect.

Referee 1.13: 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.

Author: We will make these changes.

Referee 1.14: 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.

Author: We intend to greatly simplify the explanation of the synthetic  $CO_2$  and  $\delta^{13}C$  time series in sections 2.3.1, 2.3.2 and move the details to the Methodological details section of the supplement. This will also serve to limit the length of the document, a concern for some reviewers.

Referee 1.15: 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.

Author: Good idea. We intend to update this figure to show the average seasonal cycle in fluxes. We will move the original figure 2 to the supplement, not only to provide the length of the data record, but also to demonstrate transient behavior as revealed by the flux data (i.e. changes in productivity, or latent heat exchange by drier conditions etc.)

Referee 1.16: 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.

Author: The manuscript does not address the accuracy of modeled WUE, given we don't have observations of transpiration and therefore no observations of WUE, only observations of latent heat flux, which the model simulates quite well after calibration (see Figure 2).

Referee 1.17: 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?

Author: We intend to clarify that this occurs over the time frame of 1960-2000.

Referee 1.18: 3.2.1 you state that “. . .this trend imposed by iWUE can be neutralized by increasing  $c_a$ .” Firstly, what trend do you mean? The one in  $c_i/c_a$ ?

Author: We mean the established relationship between iWUE and discrimination, that is, as iWUE increases, discrimination weakens (Saurer et al. 2004; equation 17). We discuss this in the previous paragraph, and intend to edit the text to emphasize what relationship/trend we mean.

Referee 1.19: Secondly, I am struggling with the logic of this sentence, since the principal effect of rising  $c_a$  is stomatal closure, which increases iWUE. So how can  $c_a$  counteract this at the same time? Doesn't that depend on how strong stomata respond to  $c_a$ , 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  $g_s$  with  $c_a$  and a constant  $c_i/c_a$ . Please clarify this argument, in particular the role of  $c_a$  for iWUE.

Author: Equation 17 defines an inverse relationship between iWUE and  $c_i^*/c_a$  (full derivation of this relationship can be found in supplement). Equation 17 suggests that  $c_i^*/c_a$  (discrimination) should decrease as a result of iWUE increasing (constant  $c_a$ ). However, if  $c_a$  is also increasing at the same time this relationship between iWUE and discrimination can weaken. Because iWUE should respond to an increase in  $c_a$  through  $g_s$  (as you have commented), this implies a weak stomatal response to  $c_a$  in the model.

Referee 1.20: 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?

Author: We intend to add discussion of the implications of ignoring mesophyll conductance upon our discrimination results. In particular we intend to discuss the possibility that ignoring mesophyll conductance could have contributed to the overestimation of the simulated photosynthetic fractionation. We also intend to add discussion that explains one of the key reasons that iWUE and discrimination can vary independently is because the model that Seibt et al. 2008 uses that relates iWUE and  $\delta^{13}\text{C}$ , includes mesophyll conductance. They demonstrate that trends do emerge that are different from the linear model used by Saurer et al. (2004), consistent with our simulation results (increasing WUE and increasing discrimination). This finding is largely coincidental, considering that CLM does not include mesophyll conductance, however, it is still important to show the model is not necessarily in conflict with observed trends.

Referee 1.21: 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  $A_n$ , and thus reduce the model-observation mismatch. Note that this would also affect  $V_{cmax}$ .

Author: Thank you for bringing this to our attention. The idea that the stomatal slope is too high leading to a stomatal conductance that is too high is also consistent with our simulated mismatch in discrimination (i.e. it is too high). We will add the Lin et al. 2015 paper to bolster this argument.

Referee 1.22: 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.

Author: Referee 2 offers DeKauwe et al., (2013) that provides site based observations within the FACE experiment at Duke and Oak Ridge, that indicate that fully coupled  $A_n$ - $g_s$  models tend to perform better in terms of GPP and WUE response to increased  $\text{CO}_2$ . We intend to add discussion of how the partially-coupled version of CLM that we use in our manuscript are consistent with DeKauwe.

Referee 1.23: Conclusions: You state that the isotope measurements suggest a lower  $g_s$  than the flux tower measurements. I'm not sure if I agree with that, since you didn't derive  $g_s$  directly from the eddy covariance measurements, but rather used the Ball-Berry model with an uncalibrated stomatal slope to model  $g_s$ . So if your stomatal slope parameter is inappropriate for the vegetation at the site, then your  $g_s$  will be as well, but that can't be directly related to the eddy covariance data.

Author: After calibration of  $V_{cmax}$  the simulated fluxes matched the flux tower observations much better (Figure 2), which makes our calibrated set of parameters consistent with the eddy covariance flux tower data, and biomass observations. We think it is reasonable to suggest the stomatal slope is too high considering that other studies suggest the stomatal slope should be relatively low for coniferous evergreen species (Lin et al. 2015; Mao et al. 2016) (we intend to add the Lin et al. 2015), and that the

simulation is overestimating discrimination –consistent with a stomatal slope (stomatal conductance) that is too high.

However, we agree with the reviewer that because the stomatal slope parameterization was not taken directly from leaf-gas exchange measurements at the site, therein lies a possibility that calibrating the stomatal slope value to match isotopic discrimination could be in fact compensate for other parametric or structural errors within CLM. We plan to discuss this possibility, and that we may be able to correct for bias in discrimination by including a representation of mesophyll conductance in CLM.

Referee 1.24: Figure 1: what do the lines prior to 1850 represent? Is it necessary to show them here?

Author: We changed the limits for the first column to start at 1850.

Referee 1.25: Figure 8: in Panel A it says fractionation in the heading but discrimination in the caption. Please stick to one.

Author: We will change this heading.

Referee 1.26: 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 over-dimensioned - P. 21, line 19: “through”, not “though”

Author: We will make these changes.

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Response to comments of Referee 2:

Referee 2.1: I found the introduction very clear but I wonder if there is any other literature on how other models have used isotope data? I realize 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?

Author: As this referee points out later in the review: “... [suggests] cuts that could be made to the text which would make it more digestible.”; here is an instance where we felt we needed to be concise. We do make references to previous isotope literature as was relevant to our work, for example Mao et al. (2016) and Aranibar et al. (2008). Nevertheless, to address the reviewer’s concern we will add a sentence that describes other isotope enabled land surface models.

Referee 2.2: Equation 1: I don’t think you mean  $Respd$  = dark respiration.  $R_{dark}$  is not the same as day respiration/respiration in the light. Suggest the use of  $R_{day}$  or  $R_d$ .

Author: Correct. The CLM literature (Oleson et al. 2013) uses the term ‘ $R_d$ ’ to describe this maintenance respiration term. For this manuscript, we have purposely named this term ‘ $Respd$ ’ to prevent confusion with the isotope community convention of using ‘ $R$ ’ to describe the ratio of  $^{13}C$  and  $^{12}C$  isotopes. To

address the reviewer's concern we will refer to this term as just 'leaf respiration' in the text and Table (1).

Referee 2.3: 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?

Author: This is correct as we have defined in Equation (4). See Oleson et al. (2013).

Referee 2.4: 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.

Author: We will replace "tree" with "vegetation" to avoid confusion. In this manuscript CLM simulates the Niwot Ridge vegetation as a temperate evergreen needleleaf forest as already stated. Grasses are not considered in this manuscript, but CLM is capable of simulating grasses.

Referee 2.5: 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.

Author: We intend to edit the text to make a clearer linkage between the numbers and the fractionation mechanism they represent.

Referee 2.6: Century model (line 26/27) should have a reference.

Author: We will add a reference (Parton 1988).

Referee 2.7: 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.

Author: We found this suggestion difficult to address given its generality. However, where Reviewer 1 made specific suggestions of cuts within the Methods (2.3.1, 2.3.2) we intend to cut roughly 20 lines of text from the Methods section. We also intend to cut ~ 10 lines of text within the conclusions. The revised manuscript will remain the same length (26 pages) even with significant discussion added.

Referee 2.8: 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 ( $C_i$ ), whereas isotopes measures the resistance from the chloroplast ( $C_c$ ). I see no mention of this in the text and caution against the authors potential drawing the wrong conclusion from the model-data discrepancy.

Author: We intend to address mesophyll conductance specifically as discussed above. First, our finding that the stomatal slope parameter value is likely too large is a reasonable conclusion for 2 reasons: 1) A lower stomatal slope value is consistent with both model results (Mao et al. 2016) and leaf-gas exchange measurements (Lin et al. 2015). Discussion of the Lin et al paper will be added. Second, a lower

stomatal slope value will lead to a lower stomatal conductance which will help reduce the overestimation of the modeled isotopic discrimination (Figure 7).

With that being said, we will add in the discussion the possibility that this result may, in part, come from the simplified approach of CLM 4.5, that does not specifically include mesophyll conductance and assumes intercellular CO<sub>2</sub> = intracellular CO<sub>2</sub>. Therefore, we will add this caveat, that the need to reduce the stomatal slope, may be the result of missing mechanisms governing mesophyll conductance within CLM. Given the potential importance to the paper, we will include this possibility in the abstract.

Referee 2.9: In discussing the "limited nitrogen formulation", the authors note: "In general, there were no categorical differences in behavior between these two classes of models during CO<sub>2</sub> 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 CO<sub>2</sub> 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  $A/g_s$  to CO<sub>2</sub>, 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"?

Author: Thank you for bringing De Kauwe et al. 2013 to our attention. First, we may be talking about two different sub-groupings of models: in our manuscript we are comparing pre-photosynthetic (foliar nitrogen) and post-photosynthetic nitrogen limitation models. Almost all of these models regardless of pre/post photosynthetic sub-grouping contain stomatal-photosynthetic coupling though Ball-Berry type assumptions in the stomatal conductance model.

It is true that CLM4 and CLM4.5 in the default model (post-photosynthetic model formulation) is only 'partially' coupled in terms of photosynthetic-stomatal conductance, however the unlimited nitrogen formulation (pre-photosynthetic) in our manuscript is 'fully' coupled ( $A_n$  is consistent and solved simultaneously with  $g_s$ ). Therefore, our simulations were consistent with De Kauwe et al. 2013 in that fully-coupled models matched the observations the best. We will add this to the discussion.

This progressive de-coupling between  $A_n$  and  $g_s$  for our default CLM 4.5 version also explains the difference in transient behavior between  $g_s$  and  $A_n$  and  $iWUE$  as shown in Figure (6). We will add this to discussion.

This referee makes another comment that seems to be referring to a 3<sup>rd</sup> sub-grouping of model – models that do not consider nitrogen limitation at all – similar to the simple stomatal-assimilation model in De Kauwe. What role does nitrogen limitation play (if any) in assimilation and stomatal behavior? We are not sure how much our manuscript can inform this question. Clearly, the default version of CLM 4.5

(post-photosynthetic formulation) is strongly influenced by the nitrogen cycle, whereas our pre-photosynthetic formulation is less strictly linked to the nitrogen cycle ( $V_{cmax}$  was calibrated to match eddy covariance flux observations, not according to nitrogen constraints). However, even for the pre-photosynthetic formulation it is plausible that leaf nitrogen content plays a role in the  $V_{cmax}$  value. Therefore within the limitations of this manuscript we don't think we can comment on the significance of nitrogen limitation on ecosystem behavior, but only that if nitrogen limitation is implemented, it should occur pre-photosynthetically.

Referee 2.10: Figure 2. I realize that a strength of this paper is the long time series; however, showing ~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.

Author: We intend to edit figure 2 to provide a seasonally averaged flux behavior across all years, to better illustrate model-observation differences, and calibrated/uncalibrated model differences. We intend to move the original figure 2 to the supplement, not only to provide the length of the data record, but also to demonstrate any transient behavior as revealed by the flux data (i.e. changes in productivity, or latent heat exchange by drier conditions etc.)

Referee 2.11: 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?

Author: We thought the same thing. The modeled soil moisture tends to compare favorably to the observed soil moisture as measured at multiple depths both in terms of the magnitude and seasonal trends. In general, the modeled soil moisture tends to simulate slightly wetter conditions as we already point out in the discussion, but per communication with site PI's, the accuracy of the soil moisture sensors is questionable. Therefore we did not attempt to calibrate the hydrology model to best match the soil moisture sensors. We hypothesize that the modeled soil moisture is too wet at depth, thereby leading to little change in BTRAN (soil moisture stress parameter). We chose to limit this discussion in the text to keep the manuscript length in check, a concern for this reviewer.

Referee 2.12: Figure 9. I would suggest the symbol sizes could be reduced, they seem a little large for the figure panels.

Author: We will reduce the symbol size.

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Response to comments by Ralph Keeling:

Ralph Keeling 1: Overall, very nice paper, which I found quite educational. I'm passing on here a few comments that I jotted down as I read through the paper.

Author: Thank you.

Ralph Keeling 2: Perhaps it would be possible (?) to add a figure which diagrams the carbon flows from the atmosphere, through stomata to substrate formation for each of the three formulations. I'm imagining that the diagram would have arrows for each of these quantities:  $A_n$ , GPP,  $CF_{available\_alloc}$ ,  $CF_{alloc}$ ,  $CF_{GPPpot}$ , etc. Or maybe one figure would suffice, assuming the knobs to switch between formulations is clear enough.

Author: We intend to add a new figure (Figure 1), similar to what we proposed and posted in the discussion forum, which explicitly tracks the carbon flows through the 2 main nitrogen sub-models used in this study, and illustrates how the nitrogen limitation model interacts with these carbon flows from substrate to biomass. We also intend to add two new equations which explicitly show the linkage between  $A_n$  and  $CF_{GPPpot}$  and between  $CF_{GPPpot}$  and GPP. This new figure, and the new equations, combined with the existing Table 2 should provide a better overview of the sub-models that complements the text to enhance reader understanding.

Ralph Keeling 3: ....where is the carbon that is fixed but not allocated ending up? Is it respired? If so, does this respiration return back from the stomata or return through some other pathway?

Author: For the limited nitrogen sub-model (post-photosynthetic limitation), the carbon that is fixed but not allocated, is removed from the system (does not show up as a respired flux). This is arguably a weakness in this version of the model: the downscaled assimilated flux is not consistent with the carboxylation rate ( $A_n$ ) and stomatal conductance ( $g_s$ ) that created the pre-downscaled flux, and is why this version of CLM is considered to be 'partially' coupled, which we intend to add to the discussion. The unlimited nitrogen sub-model (pre-photosynthetic limitation) is not subject to this apparent inconsistency and is 'fully' coupled. We intend to add a new Figure (1), similar to what was posted in the discussion forum, that shows a valve for this downscaling, and no respired flux. We also intend to add clarification that states that this excess carbon is lost to the system, and does not show up as a respired flux.

Ralph Keeling 4: Page 6, line 28: I'm missing how  $A_n$  is related to terms in Eq. (6). It would very much help to include an algebraic expression for this.

Author: We will add a new equation that relates  $A_n$  to potential GPP term  $CF_{GPPpot}$ .

Ralph Keeling 5: Page 6, lines 29-30. From the wording it sounds like maintenance respiration is partly double counted.

Author: The  $CF_{GPP,mr}$  term comes directly from the carbon pool from photosynthesis (photosynthate). When there is no photosynthesis the model calls on a storage carbon pool to meet this demand:  $CF_{GPP,xs}$ . The maintenance respiration is coming from the photosynthate pool, and when photosynthate is low or zero, is supplemented by the maintenance respiration storage pool. We will clarify this.

Ralph Keeling 6: Page 7, line 11. What does the subscript  $psn$  signify? Perhaps could be omitted?

Author:  $P_{sn}$  stands for photosynthetic fractionation. This is implied in the context of the manuscript so it will be removed throughout.

Ralph Keeling 7: Page 7, line 15: This formula suggests that  $A_n$  is not equal to the flux through stomata. So what is  $A_n$  equal to? Is it the same as potential photosynthesis? If so, needs stating. See earlier comment also.

Author:  $A_n$ , as defined in equation (1) is the (potential) leaf-level net assimilation rate which is used to calculate potential photosynthesis ( $CF_{GPP_{pot}}$ ). We will specify that  $A_n$  is the leaf-level net carbon assimilation. We also add a new equation that connects  $A_n$  with  $CF_{GPP_{pot}}$  based on a previous comment making it clear that  $A_n$  is the assimilation rate that is used to calculate potential GPP.

Ralph Keeling 8: Page 7, lines 22-23: This sentence is a bit ambiguous. Are both given in Eq. 9, or just one. If not both, then how is nitrogen limitation incorporated? Reading below, I see this is probably related to control of  $V_{cmax}$ . If so, this need stating more clearly earlier.

Author: Both formulations follow Equation (9). We intend to make this clearer in the text: "The unlimited nitrogen formulation also follows equation (9), however the vegetation is allowed to have unlimited access to nitrogen."

We intend to add discussion in the next paragraph of the manuscript that gives a thorough explanation how we used  $V_{cmax}$  in order to take into account for nitrogen limitation, even when the nitrogen downregulation factor is not used.

Ralph Keeling 9: Page 8, line 22. I'm missing an expression for how  $\delta_{GPP}$  is calculated from  $\alpha_{psn}$ . (Okay, I know enough to work this out for myself, but I'm not sure you should assume all readers would).

Author: The fractionation factor  $\alpha$  is defined in the beginning of section 2.1.2, stating that  $\alpha = R_a / R_{GPP}$ . This relates  $\alpha$  to  $R_{GPP}$ . One can then use equation (10) to get from  $R_{GPP}$  to  $\delta_{GPP}$ .

Ralph Keeling 10: Page 8, line 28. It would seem important to clarify what is meant here by GPP. Which of these is it:  $A_n$ ,  $CF_{available\_alloc}$ ,  $CF_{GPP\_pot}$ , etc. ?

Author: This GPP is the final downscaled GPP or actual GPP (ecosystem photosynthesis). We will add a new equation that defines how GPP is downscaled through  $f_{dreg}$ , and this downscaled, or 'actual' GPP is what is used in the definition for WUE.