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Interactive comment on "20th-century changes in carbon isotopes and water-use efficiency: Tree-ring based evaluation of the CLM4.5 and LPX-Bern models" *by* Kathrin M. Keller et al.

Anonymous Referee #1

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In the manuscript "20th – century changes in carbon isotopes and water-use efficiency: Tree-ring based evaluation of the CLM4.5 and LPX-Bern models" Keller et al. present the implementation of a carbon isotope scheme in two global models as well as their performance with respect to simulated spatial patterns and decadal trends. The model results are compared to two different datasets, tree-ring records and bulk leaf delta 13C data. This study is a valuable contribution to ongoing efforts on the implementation of carbon isotopes in global vegetation models. The overall approach as well as the results are presented in an adequate and clear manner. My main criticism is on the conclusions that are drawn from these results. Some parts of the discussion will be subject to revision. More detailed comments are listed below.

Abstract The abstract is a nice summary and contains all important aspects of the paper. However, I disagree with the last sentence. Suggesting "fundamental problems associated with the prescribed relationship between conductance and assimilation" is rather provocative and not supported by the results of this study. This relationship is strongly supported by observations (see e.g. Wong 1979 and also De Kauwe et al. 2013, Global Change Biology; papers cited in this study) and consequently used in most global models. I.e. it would indeed be a fundamental problem in our understanding of plant physiology. If this thought is brought up at such a prominent position in the paper, it needs to be better discussed and corroborated later in the manuscript, see below.

Introduction The introduction starts with a nice overview on the application of isotopes in the Earth System, which is a good motivation for this study. Following this part, the connection between carbon isotopes and plant physiological behavior is pointed out. The introduction closes with a very clear outline of the goals of this study.

Methods Page 4: the listing of all PFTs seems a bit unnecessary to me. It is enough to mention the classifiers (phenology, photosynthetic type, etc.). Alternatively, one could provide a table showing the different PFTs and their main attributes in the appendix, but I don't think this is necessary for this manuscript. I would appreciate some more information on the carbon and nitrogen pools mentioned on the same page, in brief. How do they communicate? On what time scales? There is an abrupt jump from leaf level photosynthesis (the models by Farquhar and Collatz) to the canopy level (GPP). Please add a short sentence explaining how photosynthesis is scaled to the canopy level. However, I think it would make more sense to explain this (p.4, lines 14-19) after equation 6, and not in the general description of the model. In addition, rather than describing the stomatal model in words on page 6 and showing the Equation in the discussion (Eq. 14), I would show the equation at this point. Please make sure that its original source is cited and that the notation is consistent: here you use "ca" for atmospheric CO2, later in Equation 14 "CO2" is used. Equation 6: Could this equation be double-checked? In my understanding the last term of this equation should be the overall resistance, i.e. 1/(1.6*gs) + 1/(1.4*gb), which differs from the term here. Please also check the equation on page 6 l.13, including the unit for conductance. The information on the LPX-Bern model is quite detailed and in some parts unnecessary. Again, the information on the PFTs can be shortened. E.g. for this paper it is not relevant what PFTs grow on peatland. Descriptive text elements such as "The CO2 flux from the atmosphere to the stomatal cavity is proportional to the CO2 difference between the atmosphere and the stomatal cavity (ca - ci)" are not needed and can be seen from the Equations (e.g. Eq.8) or are physical principles. The stomatal control as simulated in LPX is poorly described. It is stated that ci/ca is set to 0.8 for non-water stressed conditions. That reads as if ci/ca is constant whenever there is enough water, even under low light, high VPD etc. Further, it is not really clear how this optimization works. Is it an optimization in the sense of Cowan & Farquhar 1977? If yes, the original reference should be cited. If not, it would be good to either elaborate this aspect or cite another study at this point where it is explained in more detail. Are the two models forced with two different meteorological datasets? CRUNCEP and CRU TS3.23? Is there a reason for this? And could that affect the results in some way?

Just for clarification: when referring to delta 13C forcing (e.g. p.8, l.21) it would be clearer to write atmospheric delta 13C.

The sentence "An empirical convective boundary layer parameterization (Monteith, 1995) couples the carbon and water cycle" does not make sense to me. Please explain why the convective boundary layer couples the water and carbon cycle.

To me it would be more helpful to read how the leaf boundary layer is treated in the model, as it directly affects your calculations (see Eq. 6). In general, when describing the models I recommend putting more emphasis on the calculation of variables that are directly used for later calculations or referred to in the Results section (e.g. calculation of the leaf boundary layer, are there differences in how soil water stress affects gs or An?). This will certainly be of greater interest to the reader than a list of PFTs that

occurs in every land surface model in a similar form.

Page 8: not everyone is familiar with the discrimination model by Lloyd & Farquhar 1994. Please mention the key differences between the two models here (Lloyd & Farquhar 1994 and Farquhar 1989). The fact that the two formulations give similar trends is an interesting aspect but it is a little bit hidden in the Methods section. Lines 14 – 20 are better moved to the discussion and can be extended. I think it would be good to be more precise here: what processes are not considered in the discrimination formulation and what does that change or not change? For instance, why is the agreement with leaf delta 13C worse when the more complex model is used? Why does it not change the trend? Discussing such aspects may not be the focus of this study but it would be a valuable contribution to the discussion on how isotopes are (or should be) considered in global models.

Results Overall, this section is nicely written and clearly structured. Model results are compared to a study by Carvalhais et al. 2014. It would be good to provide a bit more detail here. Do you mean aboveground and belowground vegetation carbon? How was vegetation carbon estimated in the study by Carvalhais et al. 2014? Section 3.3: Results and Discussion are mixed here. It would be better to focus on the Results and discuss uncertainties in section 4. Just a thought: Why not taking PFT-specific model output? One could only take the corresponding PFT of the simulations that matches the PFT of the measured species. Up to the authors. The authors suggest that there is a stronger downregulation of stomatal conductance by water stress in LPX than in CLM4.5 in some regions. Here, it would be helpful to provide some possible explanations. Is it because water stress in LPX is stronger due to the climate forcing, the way soil moisture is simulated, or due to a stronger stomatal response to water stress?

Discussion This section contains many interesting thoughts, but its structure is not very clear. If it was divided in several subsections as it is the case for the results section, it would be easier to find certain aspects the reader is interested in. p. 17, l.14-18: This paragraph can be expanded. As mentioned before, the differences between the discrimination model used here, and a more complex one (e.g. Lloyd & Farquhar, 1994), as well as possible implications for the simulated absolute values of discrimination and its trends can be discussed in more detail. p. 18: The question that the reader will have is: why does CLM4.5 simulate such a strong trend in iWUE? The authors provide two possible explanations: 1) the downregulation of photosynthesis by nitrogen, and 2) an inadequate relationship between simulated stomatal conductance and assimilation. The first one is described well and is supported by other recent studies. In this context it would be helpful to know how the fdreg factor in Equation 6 changes over time, and whether it affects the relationship between An and gs. Concerning the second explanation, I don't understand what the key message should be. Is it the general form of the Ball-Berry model and the prescribed relationship between gs and An? In this case it should be mentioned that this model or similar models are used in most land surface models (see e.g. Sato et al. 2015, JGR Biogeosciences). If the reason for the strong iWUE trend is due to an inadequate relationship between gs and An, we should see a similar behavior in other land surface models. A comparison with other models is missing here. It is then argued that the trend may partly be attributed to changes in relative humidity, but no data are shown that would support this statement. What does the CRUNCEP climate forcing dataset suggest? Is there a trend in relative humidity that could explain the strong trend to some extent? Do areas that show a decrease in discrimination also show a decrease in relative humidity? The role of relative humidity (and possibly other climate variables) is an interesting aspect to discuss at this point, but it should be supported by data and discussed in context of the factorial simulations that were made. It also mentioned that the value of the stomatal slope parameter m might be too high. It would be good to provide some more information, here or in the method section. What is the value of m? Is it constant across PFTs? I agree that m is probably too high for coniferous forests, but not necessarily for other vegetation types. If the value of m is to be discussed here, the authors should at least cite Lin et al. 2015, Nature Climate Change, who looked more generally at patterns of m across

PFTs. They used a slightly different model, but that shouldn't affect the patterns of m, see also Miner et al. 2016 Plant, Cell & Environment. Changing m would certainly affect the absolute values of iWUE and discrimination, but would it make a difference to the simulated percentage trend in iWUE as shown in Figure 7? If the value of m is taken as a reason for the overestimated trend in iWUE by CLM4.5 this needs to be shown somehow. In my opinion, a change in m would primarily change the spatial patterns of the simulated discrimination. The formulation implemented in the LPX is better able to capture the observed iWUE trend. But is that really because of the optimization? I would argue that also the Ball-Berry model (Eq. 14) predicts a constant ci/ca and thus a trend in iWUE that is proportional to ca, provided that rH and m do not change over time. In my eyes this is indicative of changes in rH, or more likely, problems with the nitrogen downregulation, as discussed earlier. Why not testing this? The CLM4.5 model could be run with a version that does not include the nitrogen downregulation. The comparison of this alternative version with the version used in this study could be used to answer the question whether the problem lies in the nitrogen downregulation or in the stomatal conductance scheme (Eq. 14). If the alternative model version still shows a stronger iWUE trend than expected, this would be a stronger indication that an optimization based approach indeed works better. Maybe new global runs are not necessary, and a simple analysis based on Eq. 6 would suffice. However, without testing this, the statement (p.20, l.27f) remains speculative and should not be mentioned in the conclusion of the paper. In general, this part of the discussion needs to be revised according to the comments above.

p. 19: The behavior of iWUE and ci/ca as reconstructed from the tree-ring measurements and modeled by LPX-Bern is compared to other studies. The nice thing on this paragraph is that it is very comprehensive. But it could be clearer with respect to the method used in the cited studies. Rather than just listing the studies you could sort them by method, i.e. mention other isotope-based studies first, then other methods. At the moment studies using the same methods (e.g. FACE) are mentioned in different parts of this section (Ainsworth & Long, 2005 and De Kauwe et al. 2013) which seems a bit fuzzy. I think this aspect is important as different methods are associated with different uncertainties (which, however, do not have to be discussed here). With respect to the eddy covariance records it may be interesting to mention that a recent study (Knauer et al. 2016, New Phytologist) found that large-scale carbon and water fluxes are not in agreement with a constant ci, but rather with a constant ci/ca.

p. 20: The effects of land use change and representation issues between the datasets/model simulations are adequately addressed. It may be helpful for the reader to mention the Figures again where the described aspects can be seen.

Figures In some figures (e.g. Fig.2), the color code and the associated numbers are very small and hard to read. It would be ok to have fewer color classes as they are hard to distinguish. Fig. 2: For the difference maps, please state what is subtracted from what, at least in the legend. Fig. 5: representing the differences in mean delta 13C as barplots is not appropriate here. I recommend to remove the bars and show the error bars only, also in Fig. 6. From Fig. 5 onwards: Some of the points on the map are hard to see. It would be helpful if their representation could be changed.

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