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Discussions

Interactive comment on "Simulating oxygen isotope ratios in tree ring cellulose using a dynamic global vegetation model" *by* S. G. Keel et al.

Anonymous Referee #2

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In this paper Keel and colleagues have implemented oxygen isotope signals (δ^{18}O) in soil and leaf water pools and wood cellulose in the global-scale land surface model LPX-Bern. This model is forced by monthly, gridded meteorological fields from the CRU for the period 1901-2010 and monthly, gridded isotope forcing provided by the coupled atmosphere-land surface model ECHAM5-JSBACH for the period 1960 to 2012. The LPX-Bern model is then ran at a daily timestep and tested against annually-resolved treering cellulose δ^{18}O dataset from field sites in Switzerland and 1960-2003 average tree-ring cellulose δ^{18}O data from sites across Europe. A sensitivity analysis of some relevant climate drivers or biophysical parameters is also performed.

In my opinion this paper is suitable for publication provided that the authors provide a bit more information on the model simulations and on how they draw some of the conclusions.

Regarding the model simulations I could not see any information on how the monthly atmospheric forcing was used to run the LPX-Bern surface model at a daily timestep. I was also surprised to see that daytime air relative humidity was assumed to be only 10

Regarding the way some conclusions are drawn, I was missing some steps in several places. For example, on page 18479 lines 13-15, the authors claim that the model reproduce treering δ^{18}O across Europe "within the uncertainty of soil water δ^{18}O", but no statistical test is presented to support this statement, no value on the "uncertainty of soil water δ^{18}O" is given, and the single-site example given to support the statement (Table 1) shows that the soil water δ^{18}O is actually relatively well captured by the model while the biggest difference arise in the leaf water (and cellulose) δ^{18}O signal. To me this is a clear indication that biases in relative humidity (and leaf temperature) are more likely responsible for the differences found between observed and modelled tree ring δ^{18}O at this site. This is actually confirmed by the authors later on (e.g. page 18481 lines 14 and 18 or page 18482 line 9). The conclusion drawn on page 18480 lines 27-29 seems also to lack some steps as it does not seem to follow logically from what is said just before.

I also found the the ordering of the figures is somehow confusing. I would not refer to figures in the material and methods if the figures are not commented there.

Other minor comments are given below:

Page 18464

Line 4 "not been made use of" could be reformulated.

Line 5 "could" has an ambiguous subject

Lines 7-10 "compare well" (twice) is a bit too vague

Page 18465

Line 4-5 It should be noted that this is a difficult task as cross-lab synthesis of δ^{18}O data can contain large biases. Do you have an idea of the bias on your particular dataset?

Line 20 Hyphen should be removed.

Page 18466

Line 13 The term "boundary conditions" can be misleading as it suggests that the LPX-Bern model is ran/used only on a limited domain, rather than at the global scale. Maybe use the term "isotopic forcing"? Also is it realistic to use CRU meteorological data together with ECHAM δ^{18}O data, e.g. if CRU and ECHAM5 precipitation do not coincide? I guest at a monthly timescale it is not of an issue but maybe at some locations during the dry season. . .

Page 18467

Line 1 This is very likely that evaporation or mixing would modify soil surface δ^{18}O. Is such assumption made in LPX-Bern or ECHAM5-JSBACH? It is not clear from the text. If it is the case it should be stated. If it is not the case I don't see the reason to write this sentence.

Line 10 This statement is incorrect because the evaporative enrichment (i.e. the Craig-Gordon d18O value) will also be higher (relative humidity effect).

Line 11 I would add "thought to be" 27‰ enriched. . . Also I would precise "bulk" leaf water.

Page 18468

Line 6-7 Those extra steps are not "related to leaf water" (as stated in see line 3). These are for models of δ^{18}O in treering cellulose.

Line 9-10 I am not sure it is the correct argument. If a large range of different species

is involved, I would rather go for species-specific parameters. . .

Page 18469

Line 2 Does it mean that you have multiple PFTs sharing the same soil and space, i.e., competing for light as well?

Line 5 Is this maximum transpiration equal to potential ET?

Line 9 Are gm and αm species-specific parameters?

Line 12 Why is the ratio of intercellular to ambiant CO_2 mixing ratio set to 0.8? It is very high no?

Line 17 I would reformulate "which is a Farquhar model".

Line 21 Does it mean the model is using a daily time step? It has not been stated so far.

Page 18470

Line 2 I guess λ is the same parameter that is set to 0.8 in the previous paragraph. Maybe introduce the symbol before?

Line 8 I think Farquhar and Lloyd (1993) is a better reference.

Line 19 Why using such a high value (32‰). This is the upper bound in the literature and it is quite controversial.

Page 18471

Line 6 Does this value of L comes from Faruhar and Lloyd (1993)? Why is it not PFT specific? Maybr cite Kahmen et al. (2009) here? Equation 6 Do you have a reference? It seems to come from Cuntz et al. (2007) but there is a typo there. Maybe use a different formulation that has no typo (e.g. Braud et al. 2005 Eq. C1 or Cuntz et al. 2007 Eq. A23) or mention original Eq. A22 but state there is a typo?

Line 13 Rather than “high transpiration rates” I would say “low relative humidity” or “high evaporative demand”.

Page 18472

Line 1 You cite Sternberg et al. (1986) above. Why about results from Sternberg and Vendramini (2001) (see their Figure 1)?

Line 8 “with a daily time step”: finally. . .

Page 18473

Line 7-12 This reduction in relative humidity between daytime and daily values seems rather low. How large is the reduction in the data from Meteoswiss? Also it is stated before that the output data from ECHAM5-JSBACH are at monthly time scale. How do you go to the daily time scale from then?

Line 16-17 How is the δ^{18}O of soil water in JSBACH used in the soil water scheme of LPX given that the soil vertical discretisation seems quite different? You would need to provide explanations for this important aspect. Also how is the soil evaporative enrichment treated in LPX?

Page 18474

Line 23-25 But Tair is already 4°C higher than the observed no? And what about the other terms that depend on leaf temperature, including the relative humidity term?

Page 18475 Line 11-13 Not very clear from this figure.

Page 18477

Line 2 The order of the figures is a bit strange. I would not introduce them in the material and method section if not commented there.

Page 18479

Line 10 30gC/m2 is extremely low. Are you sure of the units? Aso δ^{18}O of which pool

are we talking about here?

Line 11 I guess “these” refer to the humid sites + CAZ but should be stated a bit more clearly maybe.

Page 18480

Line 24 Not clear from figure. Maybe draw a line fo 2003?

Page 18485

Line 7 Could you be more quantitative?

Page 18486

Line 26 Need a reference.

Interactive comment on Biogeosciences Discuss., 12, 18463, 2015.