Anonymous Referee #2

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In this paper Keel and colleagues have implemented oxygen isotope signals (_18O) 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 _18O dataset from field sites in Switzerland and 1960-2003 average tree-ring cellulose _18O 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.

Thank you

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 time step.

Text will be added in section 2.3 (page 18472, line 23 of the original MS): “Monthly climate data are linearly interpolated to daily values, except for precipitation where a stochastic weather generator is applied to compute daily precipitation following Gerten et al. (2004)”

I was also surprised to see that daytime air relative humidity was assumed to be only 10

Apparently, this sentence is incomplete. It will be noted in the text that relative humidity is downward corrected by an absolute value of 10%. Thus, we do not assume a relative humidity of 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 _18O across Europe “within the uncertainty of soil water _18O”, but no statistical test is presented to support this statement, no value on the “uncertainty of soil water _18O” is given, and the single-site example given to support the statement (Table 1) shows that the soil water _18O is actually relatively well captured by the model while the biggest difference arise in the leaf water (and cellulose) _18O 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 _18O 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).

It is not our intention to imply that uncertainty in soil water 18O data represent the largest uncertainty for simulated tree ring d18O values. To avoid confusion the statement will be changed to read: “We conclude that LPX-Bern is able to represent the magnitude and the spatial climatological pattern of δ18O in stem cellulose in Europe, generally within a few permil of available observations.

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.

This sentence on line 27-29 will be moved to the end of the previous paragraph (line 14 of the original MS)

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.

We will remove references to figures in the material & methods. The new order of figures has been described above.

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

Sentence on line 4/5 will be changed to: “So far stable oxygen isotope signatures of tree rings are not represented in dynamic global vegetation models (DGVMs). DGVMs integrate many hydrological and physiological processes and their application could improve proxy-model comparisons and the interpretation of oxygen isotope records.”

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

“well” on line 7 will be replaced by “within a few permil”. “well” on line 10 will be deleted

Page 18465

Line 4-5 It should be noted that this is a difficult task as cross-lab synthesis of _18O data can contain large biases. Do you have an idea of the bias on your particular dataset?

Most of the data used in our study are from the ISONET study for which an inter-laboratory comparison has been made: Boettger, T., et al. (2007). For d18O in cellulose the variation reached 0.58 permil for the means.

Line 20 Hyphen should be removed.

Will be done.

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”?

Term will be replaced

Also is it realistic to use CRU meteorological data together with ECHAM _18O 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. . .

We are not aware of any data product that provides an observation-based evolution of soil water d18O during the past 50 years. Thus, we have to rely on the ECHAM data. Haese et al. (2013) estimate that the root mean square error between precipitation *d*18O simulated by ECHAM5-JSBACH and the GNIP data is 1.78 ‰.

Page 18467
Line 1 This is very likely that evaporation or mixing would modify soil surface _18O. 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.

Sentence modified to read: “The d18O of surface soil water reflects the d18O signal of precipitation averaged over a certain amount of time and is further modified by evaporation of soil water leading to evaporative enrichment and potentially by mixing with ground water”

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

Statement modified to read: “This Péclet effect tends to reduce the signal of evaporative enrichment in bulk leaf water. The Péclet effect is large when transpiration rates are high.”

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

Will be done.

We will add an explanation for bulk leaf water (i.e. whole 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 _18O in treering cellulose.

Text changed to read: “but additional processes related to d18O signals in leaf water and stem cellulose are resolved at varying degrees of complexity”

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. . .

Text on line 10 amended by: “.. and as we lack detailed species-specific information, e.g. on water flow and the Péclet effect.”

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?

We will specify: ‘PFTs compete for water, but not for light.’

Line 5 Is this maximum transpiration equal to potential ET?

We will add a comment.

Line 9 Are gm and _m species-specific parameters?

We will make this clear by adding:

“which are equal for all plant functional types”.

Line 12 Why is the ratio of intercellular to ambiant CO2 mixing ratio set to 0.8? It is very high no?

See above
Line 17 I would reformulate “which is a Farquhar model”.

Photosynthesis is modeled following (Collatz et al., 1991, 1992), which is based on the formulation by (Farquhar et al., 1980; Farquhar and von Caemmerer, 1982) generalized for
global modeling purposes.

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

Text added on p. 18468 and line 23: “.. Wania et a., 2009), and features a daily time step for photosynthesis and evapotranspiration.”

Page 18470

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

Will be done.

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

We will replace this reference by Farquhar and Lloyd (1993)

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

We chose the value based on an experimental assessment by Cappa et al. 2003 and will add the reference.

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?

The formulation is indeed misleading and we will change it as follows:

"… the dilution of 18O-enriched leaf water… is effective over a path length L (Farquhar and Lloyd, 1993). To keep the model as simple as possible we set L to 0.03 m for all PFTs following (Kahmen et al. 2011), although L can vary largely between species (Kahmen et al. 2009)."

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?

We will add the reference Cuntz et al. (2007) and mention that there is a typo in equation A22 that we use.

Line 13 Rather than "high transpiration rates" I would say "low relative humidity" or "highevaporative demand".

We will change the wording to: "low relative humidity"

Page 18472

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

We could not find the publication by Sternberg and Vendramini (2001).

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?

See comment above

Line 16-17 How is the _18O 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?

We will add: 'In ECHAM5, there are no soil layers and the isotopic composition has no vertical gradient. Any water taken up by plants has the d18O of soil water. The soil layers in LPX do not affect the isotopic composition, but are exclusively used for quantitative assessment of water pools and fluxes.

In ECHAM5 the evaporative enrichment is affected by an equilibrium and a kinetic fractionation factor as described in more detail in Haese et al. (2013).

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?

Yes, Tair is already about 4 degree C higher. We could change this simulation and reduce the temperature by 3 degree to test equal air and leaf temperature instead. The text would need to be modified accordingly.

For this simple test we did not change any other terms and we will add: 'while all other terms remained unchanged.'

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

We will add: '(the modeled data shown as thin lines in the upper four panels)'

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.

We will change the order of figures as described above.

Page 18479

Line 10 30gC/m2 is extremely low. Are you sure of the units? Aso _18O of which pool

are we talking about here?

The above ground biomass at this site is indeed very low as this grid cell is dominated by herbaceous plants. This is why we exclude it. We make this more clear:

'extremely low above ground biomass since herbaceous plants dominate this grid cell'

We will add: 'stem cellulose d18O" to make clear which pool we refer to.

Line 11 I guess "these" refer to the humid sites + CAZ but should be stated a bit more

clearly maybe.

Will be changed

Page 18480

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

Line will be added to highlight year 2003.

Page 18485

Line 7 Could you be more quantitative?

We will add: ‘, e.g. the effect of soil water d18O varied around zero in the 1960s and is consistently positive in the 1990s (Fig. 9b).’

Page 18486

Line 26 Need a reference.

We will try to find an additional reference for global leaf water d18O.