

Response to Referee #1 (J.Severinghaus)

We thank Referee #1 for the interesting comments and detailed explanations on various aspects, and address the questions raised below.

Comment 1

One important improvement can be made in the magnitude of isotopically relevant O₂ production, which of course also must match respiratory consumption. 11 Pmol O₂/yr seems much too small to me, given that many plants produce O₂ without engaging the carbon fixation apparatus, for example during times of stress. The plants are able to produce ATP by absorbing photons and creating O₂. Thus my guess is that by scaling O₂ production to carbon uptake we have badly underestimated the true O₂ production and destruction. Furthermore, even within the carbon fixation framework, I think we have underestimated the amount of photorespiration. Most plant physiology experiments are done under ideal, well-watered, nutrient-replete conditions. In the real world, drought stress is common, and plants often photorespire at these times because of closure of stomata to preclude water loss. In the oligotrophic subtropical surface oceans, nutrient limitation seems to drive picoplankton to produce O₂ without fixing carbon and nitrogen, and so O₂ production by the marine realm has also been underestimated. Based on the rapidity of the changes we see in the $\delta^{18}\text{O}_{\text{atm}}$ record in ice cores, a total O₂ production figure of 40 Pmol per yr seems more likely, with perhaps half of that from the terrestrial biosphere

It seems that our manuscript was not clear enough that photorespiration is well taken into account in our approach. Actually, we have taken into account dark respiration (soils and plants), Mehler reaction and photo-respiration. As a consequence, oxygen production is not scaled in a simple way to carbon gross primary production but we take into account the photorespiration in addition.

Photorespiration is calculated from the proportion of C₄ vs C₃ plants, temperature and CO₂ level (assumed constant in our study) as depicted in the biochemical model of photosynthesis from Farquhar et al. (1980) and already done in the studies of Hoffmann et al. (2004) and Landais et al. (2007) (FYI see attached Figure R1). Increasing photorespiration modifies $^{18}\epsilon_{\text{resp}}$, as photorespiration is associated with a high discrimination, and in turn affects $\delta^{18}\text{O}_{\text{terr}}$. With 60 % of photorespiration instead of 30%, the O₂ production would amount to 18.7 Pmol/yr.

We propose to explain more clearly that photorespiration has been taken into account in the revised version of the manuscript.

Second, the reviewer is concerned by the value of 11 Pmol O₂/yr for the LGM which seems too small. Present day carbon and oxygen production amount to 10.5 Pmol C/yr and 17.95 Pmol O₂/yr (taking into account photorespiration) in the ORCHIDEE model, respectively. This is in line with other estimates e.g.

Angert et al. (2003) or Welp et al. (2011), estimating 8 to 13 Pmol C/yr and 12.5 to 14.2 Pmol C/yr, respectively. ORCHIDEE carbon production seems to be consistent with observations, and the scaling of O₂ production to C uptake leads to a value in agreement with former studies.

For LGM and HS, estimates from the ORCHIDEE model are indeed lower than other estimates. It gives land carbon productions of 6.8 and 6.5 Pmol C/yr for LGM and HS, respectively, which translates as 11.8 and 11.4 Pmol O₂/yr for LGM and HS, respectively. This is up to a factor of 2 lower than LGM estimates from Joos (2004), Hoffmann (2004) or Bender (1994), ranging from 23 Pmol O₂/yr to 16.7 Pmol O₂/yr. The carbon production seems to be underestimated in ORCHIDEE model for glacial times, and the low oxygen production is a consequence of it. We will clearly point this out in a revised version.

Reviewer #1 also suggests that the scaling factor between carbon and oxygen (Keeling, 1988) of 1.07 used in our study may be underestimated. We can modify the scaling factor to reach literature oxygen production value. For instance, with a scaling of 1.47 (1.87), the O₂ production reaches 16.17 (20.56) Pmol/yr without modifying $\delta^{18}\text{O}_{\text{terr}}$. The scaling may have been underestimated, but must be strongly (too much) increased to reconcile O₂ production with previous studies. Therefore the scaling factor alone probably cannot explain the discrepancy, but we'll acknowledge its potential underestimation in the revised version.

Comment 2

A second improvement can be made in explicitly treating the effect of poor exchange between wet soil air and atmosphere, as described by Angert et al. in several papers, and its connection to water saturation of the soil. Water saturated soils are known to be very poor at transmitting oxygen from the sites of respiration back to the atmosphere. For this reason actual soil respiration in wet soils has a much weaker effect on d18O_{atm} than would be estimated from the enzymatic fractionation itself. This effect can be understood via the following thought experiment: imagine a liter of air taken from the atmosphere in a flask, then the valve is closed. Then respiratory fractionation consumes all the oxygen in the flask. When the valve is reopened, only nitrogen and argon diffuse back out to the atmosphere. This flask has therefore made no contribution to d18O_{atm}. In other words, a back flux of isotopically enriched oxygen to the atmosphere, the residual left over after partial respiratory consumption, is necessary to make an effect on d18O_{atm}. If this back flux does not exist, no effect on the atmosphere occurs.

Therefore during strong monsoon intervals, some large fraction of respiratory isotope enrichment goes unrealized as a d18O_{atm} contributor due to the waterlogged soils in these climates. During a Heinrich event, these zones dry out and the soils become well aerated, greatly increasing the respiratory contribution to d18O_{atm}. In my opinion the authors have not adequately evaluated this "Angert" mechanism. I was surprised that the authors found a small effect of opposite sign to this hypothesis, in fact a reduction in the isotopic enrichment during Heinrich events from soil respiration. This is probably wrong and needs to be re-evaluated, since Heinrich events are times when waterlogged soils dry out and start having a stronger impact on d18O.

The authors greatly thank the reviewer for this very clear explanation. It is indeed important to consider the weakening of respired O_2 back-diffusion in waterlogged soils. In our model, the soil isotope discrimination is already taken into account. Indeed, we directly used Angert's results on soil water limited diffusion during respiration in using their values of respiration fractionation factors (taking into account limitation by diffusion of oxygen in water) for the different types of soils.

We have assigned fractionation factors for each soil using the soil type discrimination proposed by Angert et al. (2003). For this, we relate the Angert's soil types to the type of vegetation cover over the considered soil in ORCHIDEE model, hence indirectly to soil temperature (see Fig. 2 of Angert et al., 2003). As an example, we have assigned tropical soils (fractionation coefficient of -10.1‰) to soil covered by dominant PFT Tropical broadleaf evergreen trees and PFT Tropical broadleaf raingreen trees. Tropical soils (-10.1‰) discriminate significantly less than temperate (-17.8‰) or boreal soils (-22.4‰) following Angert et al. (2003). The global respiratory isotope fractionation for the control run calculates as -15.895‰ , much weaker than the common value (-18‰) used for terrestrial ecosystems. As soil respiration only occurs where vegetation exists, a shift of the latter modifies the spatial distribution of soils where dark respiration takes place. In our model, the change of vegetation cover from LGM to HS leads to a very slight weakening of soil respiration isotope fractionation using fractionation values of Angert et al. (2003), thus considering soil aeration. A figure of the simulated soil respiratory fractionation for LGM is attached (figure R2).

In order to further address the role of soil water saturation on respiration $\delta^{18}O$, we perform a sensitivity test by reducing soil respiration in waterlogged soils. To do so, we half the soil respiratory isotope fractionation in the tropics of the rainy hemisphere, NH for LGM, SH for HS. The soil respiratory isotope fractionation calculates to -13.44‰ , 2.45‰ weaker than in the LGM_ctrl control run, and close to the value estimated by Angert et al. (2003), -13.8‰ . The terrestrial fractionation factor, the terrestrial component of the Dole Effect, decreases from 23.41 in the LGM_control run to 22.41‰ in the modified one. The picture is even amplified for HS_exp run, where soil respiratory isotope fractionation weakens from -15.61 to -12.71‰ , leading to terrestrial fractionation factor depletion from 23.52 to 22.28‰ over HS. This sensitivity experiment indicates that in our simulation, soil respiration does not strengthen during a HS, once the climatic system has reached equilibrium. This may be different over a Heinrich Event, involving shorter timescales. Aerated soils in the NH do not compensate the diffusion limitations in water saturated soils of the SH, even with half of the soils shut down in the tropics of the most rainy (and productive) hemisphere. If the reviewer and/or editor feels that this sensitivity test is useful to discuss the robustness of the small effect of soil respiration on the LGM-HS anomaly, we propose to add its discussion in the revised version.

Comment 3

A third area that could be improved is the precipitation isotopic match between data and model. For example, the model produces 1.6‰ changes at DO events in Greenland, but the data show 4‰ changes. This is not really satisfactory, even though the authors say it is acceptable. While Greenland is not important for O₂ production, there are other somewhat more troubling mismatches between data and model in the low latitudes and the authors seem to wave away these issues. A more candid and realistic discussion of these model shortcomings would boost the effectiveness of the paper.

We agree that the comparison is not very good for Greenland but we also think that it is difficult to quantitatively compare the $\delta^{18}\text{O}$ change simulated by a freshwater input (the most efficient way to model a Heinrich event) and the $\delta^{18}\text{O}$ decrease between a Greenland interstadial and a Greenland stadial. Indeed, there are more and more evidence that the $\delta^{18}\text{O}$ decrease at the end of a Greenland interstadial is not due to the same freshwater discharge than the one associated with a Heinrich event. It can well be due to a threshold in the extent in sea-ice or an atmospheric heat transport. This indeed needs to be explained much more clearly in the new version and discuss potential implication for low latitudes uncertainties in the modeling approach.

We thus propose to make a clear case in the new version that the most efficient way to produce an Heinrich event with a model is to throw freshwater in the high latitudes of the Atlantic ocean but results from recent studies (Marcott et al., 2011; Guillevic et al., 2014; Rhodes et al., 2015; Alvares-Solas et al, 2013) suggest that this does not satisfactorily explain the observed sequences of events and especially the decoupling between Greenland and low latitudes. This is also the reason why we only focused on Heinrich Stadials and not D/O events.

Regarding low latitude model data mismatches, in addition to the modeling approach potential implications, we will also discuss the low latitude model-data anomalies in more details in the revised version to emphasize the model shortcomings. Specifically, in addition to the orography, another reason can be invoked to explain the poor match between model and data in Northern India (Timta Cave): Figure 9 (lower panel) of Kageyama et al. (2009) depicts precipitation change between HS (LGMc) and LGM (LGMb). Since the largest Indian monsoon signal is simulated over the ocean, and not over land (Kageyama et al., 2009) in the control simulation, no changes in monsoonal activity takes place over India over HS (-0.5 to -2 mm/day is only simulated over the ocean). In Northern India, hence Timta cave site, the model does not simulate any significant rainfall change between the 2 periods. A more intense weakening of the Indian monsoon over land in the HS run, hence less rainfall, would have helped reconciling model and data at Timta Cave, since precipitation $\delta^{18}\text{O}$ would have been enriched through the amount effect. It will be clearly stated in the revised version that at Timta Cave and Cave of the Bells, our model fails to capture the calcite $\delta^{18}\text{O}$ anomaly recorded in speleothems. However, the modelled precipitation $\delta^{18}\text{O}$ anomaly is quantitatively consistent with observations for most of the compared sites (Table 3 and Figure 4). The 2 sites where model and data completely disagree (Timta Cave, India and Cave of the Bells, North America) are located in altitude and they do not correspond to the regions where most of the oxygen is produced.

Comment 4

Why aren't the time series of the model output shown, for direct comparison with the ice core $d18O_{atm}$ data? Just curious. It would strengthen the paper.

We agree that it would be nice to have time series for direct comparison with data. Unfortunately, we think that this would not make sense because the model has been run at equilibrium, i.e. one experiment for the Heinrich and one experiment for the LGM. LGM and HS experiment have been run 1000 and 800 years, respectively, so that we can reach an equilibrium, and we use the averages of years 200-250 yrs and last 100 years, respectively. As a consequence, showing time-series would only show the time needed to reach an equilibrium state without any link to the reality of climatic sequences.

Comment 5

It would be useful to show a zonally-averaged $d18O$ of terrestrial precipitation curve versus latitude from the model, both pre- and post-Heinrich, so the viewer can see how the $d18O$ changes. This $d18O$ zonal average should be a weighted average, weighted by terrestrial photosynthetic production of O_2 , so that it is most relevant to the question at hand. Of course, the weighting will change as the rainfall changes and the photosynthetic production changes. A second plot should show the latitudinal variation of O_2 production, so that the reader can see that it is concentrated in the tropics. On a third plot, the total terrestrial rainfall amount both pre- and post- should also be shown. This way it is clear to the reader that the total rainfall shifts south during a Heinrich event, and at the same time becomes isotopically heavier. It is also an interesting question, whether the total amount of rainfall on land becomes less during a HS. The total amount of rainfall on both ocean and land is tightly constrained to equal the amount of evaporation, but no such constraint applies to the land fraction. So it is quite possible that more rain fell over the ocean at HS, compared with D/O Stadial conditions.

We thank the reviewer for his suggestion. In our study, we consider a system at equilibrium for 2 periods, LGM and HS. We have 2 sets of monthly resolved data averaged over LGM and HS. We thus provide here 2 figures (one with the full latitudinal profile (R3), the other zooming on the intertropical zone (R4), where most of the production occurs) for the control (LGM_ctrl) and HS (HS_exp) simulations. In addition to the southward shift of the tropical rain belt, they clearly show how rainfall amount and $\delta^{18}O_p$ are anticorrelated as expected on most of the latitudinal profile. During a HS, $\delta^{18}O_p$ is enriched in the NH down to 14 °S. The figure zooming on the intertropical band reveals a particular pattern between the equator and 14°S, where oxygen production is most enhanced at HS, as precipitation are more abundant but also heavier in $\delta^{18}O$.

We'll present the figure zooming on the intertropical band (R4) in the revised version of the manuscript.

Total amount of terrestrial rainfall decreases by around 0.02 mm/day during a HS, while total amount of rainfall is similar (1.85 mm/day) between the 2 time periods.

Comment 6

The maps of isotopic composition of rainfall aren't very useful. It would be better if the color scale was adjusted so that the most relevant range of values are more finely graduated. As it stands most of the interesting parts of the tropics are all one color.

6. The colorscale has been adjusted to better see tropical variations. Note that $\delta^{18}\text{O}_p$ difference between the two periods is also displayed in Fig. 4a (whose colorscale was unfortunately slightly offset, see figure R5). Figure 3 has been modified to follow recommendations of reviewer #2 (see comment 1 and attached figure R6 and R6bis in reply to reviewer #2).

Comment 7

I was surprised to see no reference to the controversial David Battisti hypothesis. This idea is that rainfall amount does not change at cave sites like Hulu Cave, but rather the $d18\text{O}$ of precipitation changes without change in rainfall amount. What does your model say about this hypothesis?

We suppose (please correct if I'm wrong) you refer to the hypothesis described in Pausata et al. (2011)? Battisti is a coauthor and they propose that change in rainfall amount in Indian rather than in South East Asia explains changes observed in calcite $d18\text{O}$ in Chinese stalagmites (in South-East Asia). In our model, as shown in Table 3, the modelled increase in $\delta^{18}\text{O}_p$ quantitatively agrees with data $\delta^{18}\text{O}_c$ increase during Heinrich Stadials in most of the compared sites. -0.17 and -0.13 mm/day are simulated at Hulu and Songjia Cave during HS. Rainfall amount drops in East-Asia and North-West India, mostly over the ocean (see comment 3) but increases in South-East India, as shown in the attached figure. However, $\delta^{18}\text{O}_p$ is enriched over the whole India (with an abrupt change south of Timta Cave) and South Asia.

As in Pausata et al's (2011) study, a freshwater pulse was applied to the control simulation with LGM background climate. The enrichment in $\delta^{18}\text{O}_p$ observed in Chinese caves is reproduced by the model (as in Pausata et al.'s (2011) study) but the latter fails to capture the enrichment in Timta cave, as in Lewis et al. (2012) study, where the freshwater pulse was applied to a present-day simulation. Results of our simulation do not discard a possible role of the Indian monsoon in the oxygen isotopic enrichment of Chinese stalagmites, but local amount effect can at least partly explain the increase in $\delta^{18}\text{O}$ over South-East Asia.

More detailed comments:

Page 2282, line 24 “effusion processes” This is incorrect. Harmon Craig and Michael Bender erroneously used this term long ago and it has unfortunately stuck. Effusion is a different process, having to do with Graham’s Law of Effusion, in which a gas is fractionated by passing through orifices smaller than the mean free path length, which is about 1 micron at atmospheric pressure. Because the light isotope has a higher velocity than the heavy isotope, in inverse proportion to the square root of the masses, the light isotope is enriched by a factor of the square root of the ratio of the masses. This process was used, for example, to enrich uranium isotopes during the second world war. Current understanding of the bubble close-off fractionation in glacial firn is that permeation of the gases through the ice lattice surrounding an overpressurized bubble is responsible for the fractionation (Severinghaus and Battle, 2006; Huber et al., 2006). Instead of transport through orifices of 1 micron, the permeation mostly occurs by breaking of bonds in the ice lattice, for gases like O₂ and N₂. So it should be called “a permeation process” or just “permeation through the ice lattice”, or ‘ice permeation fractionation’.

Thank you once again for a very clear explanation. In Page 2282, line 24 “effusion processes” will be replaced by “permeation through the ice lattice” as suggested.

Page 2283, line 1. Rather than “infer..” this should probably read “have thus been explored as possible constraints on biospheric productivity”, since no truly successful inference has been made yet.

Will be replaced by “have thus been explored as possible constraints on biospheric productivity”

Page 2283, line 13. Should be “d18O_{atm} variations actually reflect in large part “because it is the variability that is dominated by meteoric water. The absolute value of d18O_{atm} (+24 per mil on the VSMOW scale) is dominated by respiratory fractionation, in contrast.

The reviewer is right, and the sentence will be corrected accordingly: d18O_{atm} variations actually reflect in large part the isotopic composition of the meteoric water.

Page 2283, line 14. “latter” not “later”

Will be corrected.

Page 2284, line 16. “Millennial-scale climate variability is perhaps best known from the Greenland ice cores, where it is manifested in the stable water isotopes of ice. During the last glacial period, these cores show 25 Dansgaard-Oeschger (DO) events.”

Thank you for clarifying the writing. The sentence will be replaced.

Page 2284, line 4. This is not really an accurate history, to say that “the supposed key role of the ocean stems in part from the presence of ice rafted debris”. The original papers on Heinrich events (by Bond, for example) stated very clearly that the Heinrich events came out into an ocean that was already in a cold, stadial state. Subsequent analyses have confirmed this timing relationship. So it was always known, right from the beginning of the discussions on Heinrich events, that Stadials were NOT caused by Heinrich events. Rather, the thinking at that time was that the stadials and DO events were a manifestation of a bistable nonlinear system in which the coupled ocean-atmosphere circulation could jump between two quasi-stable modes (Broecker and Denton, 1989). Today’s thinking on this has not changed very much, and such bistability can indeed be found in a wide range of ocean circulation models. [Many IPCC-class coupled ocean-atmosphere general circulation models cannot reproduce this bistability, which is widely seen in simpler models, but this fact is viewed by most workers in the field as a model defect rather than as an indication of how the real ocean circulation operates. The model defect is likely due to the computational limitations inherent in representing the non-geostrophic (frictional) hydraulics of the bottom hugging overflow currents on the Greenland-Scotland ridge. In the real world, these currents allow dense water to sink much deeper (to 3000 m) than they can in the model, and so the model produces a too-shallow AMOC that is also not subject to hysteresis, bifurcation, and bistability]

Once again, we thank the reviewer for this clear explanation. We agree the sentence was not accurate and will remove the part mentioning the supposed key role of the ocean. The new sentence will read as “The presence of ice rafted debris (IRD, Ruddiman, 1977; Heinrich, 1988) in marine sediments from the North Atlantic region during the largest GS document episodes...”

Page 2285, line 9. “Even if IRD can be recorded” this doesn’t make sense – please fix. Perhaps you meant to say something else? “Even though IRD is present in each GS, not all GS contain a Heinrich event.”?

This was indeed meant to say what reviewer #1 said: “Even though IRD is present in each GS, not all GS contain a Heinrich event.” The sentence will be replaced.

Page 2285, line 12 should be Barker, not Baker

We will address this issue in the revised version.

Page 2286, line 4. It is implied here that temperature records follow the Greenland signal, in speleothems. This is not correct, in that speleothems mainly record a rainfall signal, not a temperature signal.

We agree with the reviewer that the sentence was confusing and will be replaced by: “Abrupt climate variation associated with the Greenland signal is found down to low latitudes in numerous terrestrial and marine archives (e.g. Clement and Peterson, 2008). Its climatic impact is recorded in a large part of the North

Atlantic region, both in marine cores (e.g. Bond et al., 1993; Broecker, 2000) and in speleothems (Fleitmann et al., 2009).

Page 2286, line 8. The “ITCZ” is a term that is reserved by the atmospheric science community for situations over the ocean. Over land, it is advised to NOT use this term, because the dynamics of the rising air motion is quite different. Therefore many of us in the paleo community are now using the term “tropical rain belts” (e.g. Rhodes et al., Science 2015) for terrestrial air convergence zones with high rainfall. You can also say “through a shift in the ITCZ and its terrestrial equivalent, the tropical rain belt”.

In the revised version, the term “tropical rain belt” will be used if it relates to terrestrial precipitation. Therefore the sentence will be corrected as: “...monsoon intensity through a shift in the ITCZ and its terrestrial equivalent, the tropical rain belt...”

Page 2286, line 15. Redundant use of “onset”

The corrected sentence will read as: “... using Rasmussen et al. (2013) definitions of onset of GS.”

Page 2286, line 17. Spelling – should be “inflection” not inflexion

Will be corrected accordingly.

Page 2286, line 22. “... should provide added value”

“strong” will be removed from the sentence as suggested by the reviewer #1

Page 2287, line 4. “repartition” is not widely understood in English – perhaps use “...water, vegetation redistribution, and productivity...”

We will replace repartition by distribution, so the sentence in the revised version will read as: “We combine climatic parameters (temperature and humidity), isotopic composition of meteoric water, vegetation distribution and productivity simulated by different models with monthly mean temporal resolution.

Page 2287, line 12. Again, please do not use ITCZ. Instead, “shifts in the tropical rain belt” is more accurate, because $d18O_{atm}$ is only affected by terrestrial precipitation, not marine precipitation

The sentence will be corrected according to the reviewer #1 suggestion.

Page 2287, line 14. “build up of atmospheric oxygen ...” this means to most readers an increase in oxygen concentration. Instead you should say “The isotopic content of atmospheric oxygen is controlled by numerous processes, so we must consider...”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2287, line 19 spelling – photosynthetically

Spelling mistake is corrected in the revised version.

Page 2287, line 19 to this list you should add a fourth category, soil aeration, because this strongly affects the effective respiratory fractionation – see papers by Alon Angert. In water-logged tropical soils in monsoon regions, the respiratory contribution to $\delta^{18}\text{O}_{\text{atm}}$ is perhaps only half of what it is in temperate soils. The reason is that the backflux of isotopically enriched oxygen to the atmosphere is hampered by the poor diffusivity of oxygen in liquid water. Therefore the effective respiratory fractionation in monsoon regions may be quite reduced, adding to the depleted isotope signature in $\delta^{18}\text{O}_{\text{atm}}$ from the monsoon meteoric water.

We thank reviewer #1 for this comment. Soil aeration is taken into account in our study, as explained in comment 2 of the present review. More than soil aeration alone, we would rather add “respiratory processes” as soil respiration, but also mitochondrial respiration, photorespiration and Mehler respiration are considered.

The corrected sentence will read as: “... is calculated, (iii) the worldwide vegetation cover and Gross Primary Productivity, defining the photosynthetically and respiratory active areas that contribute to $\delta^{18}\text{O}_{\text{atm}}$, as well as (iv) respiratory processes.

Page 2287, line 22. “Assuming a steady state, $\delta^{18}\text{O}_{\text{atm}}$ can thus be...”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2288, line 7. “... because the CO_2 level remains relatively stable (Bender..)” or other appropriate reference to the strat-trop CO_2 isoflux.

The relevant references will be added, i.e. Bender 1994 and Ahn and Brook, 2015.

We assume a constant CO_2 level between LGM and HS in our study. Ahn and Brook’s (2014) study show that variations over HS are small (less than 20 ppm). Effect of isotopic exchange between CO_2 and O_2 in the stratosphere on $\delta^{18}\text{O}_{\text{atm}}$ is expected to be proportional to CO_2 mixing ratio. Following Bender’s (1994) calculation, which estimates a $\delta^{18}\text{O}_{\text{atm}}$ depletion of 0.4 ‰ for a CO_2 concentration of 353 ppm, we can conclude that 20 ppm difference between LGM and HS can modify $\delta^{18}\text{O}_{\text{atm}}$ by ± 0.023 ‰.

Page 2288, line 9. “...influence, in this first approach, for the...”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2288, line 11. “...variations are largely driven by changes in the..,” [these authors did not propose that precipitation $\delta^{18}\text{O}$ was the sole control]

We agree with reviewer #1 that other controls were proposed by these authors. The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2288, line 12. "... low-latitude hydrological cycle.."

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2288, line 22. "...as the leaf water."

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2289, line 3. "... global production-weighted average isotopic composition of leaf water..."

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2289, line 13. "...is the temperature-dependent liquid-vapor equilibrium isotope effect (Majoube, 1971)"

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2290, line 3. "...leading $\delta^{18}\text{O}$ kinetic to values as low as 19‰ when using the Merlivat..."

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2290, line 5. "because higher values led to too high a global value for $\delta^{18}\text{O}_{\text{atm}}$."

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2291, line 5. "... qualitatively agrees with paleoarchive reconstructions..."

The sentence will be corrected (paleoarchive for paleoarchives)) according to the reviewer #1 suggestion in the revised version.

Page 2291, line 10. "... followed the Lloyd and Farquhar..."

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2291, line 26 again, repartition is not a word widely recognized in English

“...the d18Op repartition...” will be replaced by “...the d18Op distribution...” in the revised version.

Page 2293, line 3. “leaf, not leave

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2304, line 12. “... more important than..”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2307, line 8. “low latitude water cycle...”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2307, line 11. “..Rhodes et al.’s (2015) recent study suggests that ... in the WAIS Divide ice core...”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2307, line 14. “Guillevic et al.’s (2014) ice core multi-proxy approach...”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

Page 2307, line 18. “... is a valuable tool..”

The sentence will be corrected according to the reviewer #1 suggestion in the revised version.

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