1 Reviewer 1

General : The paper lists and discusses the main controlling mechanisms of water stable iso- topes (d18O) as well as the second-order parameters D-excess and 17O-excess. In contrast to the conventional water stable isotope parameters (d18O and dD) that have been studied since long for temperature reconstructions of ice cores, the second-order effects as observed through D-excess and 17O-excess are less deeply studied so far. This is particularly true for 17O-excess, since it is a parameter that only recently got attention through its potential to yield information about the relative humidity conditions at the moisture source region. There is hope by combining these primary and secondary water stable isotope parameters to better constrain past climate variations both at the site of precipitation as well as at the source of the original moisture.

However, as the authors point out already in the introduction, the main driving mechanisms for the secondary processes are not well understood yet in particular those for the 17O-excess, this is in part due to the still very sparsely available data. In addition the mechanisms are complex and rely often on empirical parameters such as the su- persaturation. Related to this is the main concern of the reviewer : How valuable is such a model-data comparison when major driving mechanisms such as supersaturation as well as the surrounding relative humidity are not well understood. Both parameters are fitting parameters which are poorly constrained by measurements.

Being aware of these shortcomings, it is very ambitious to discuss Present-day (PD) to Last Glacial Maximum (LGM) conditions. The results depend strongly on the chosen value for supersaturation that may have changed between these two climate states.

The rather poor agreement of the model-data comparison highlights the importance of additional measurements of the second-order water stable isotope parameters. Both in-situ water vapour measurements as well as laboratory experiments are highly recommended in order to improve the understanding of the driving mechanisms of those parameters. In this regard the new laser-based techniques are extremely helpful for in-situ isotope measurements of water vapour. Yet, it is not possible at least so far to determine 17O-excess with this technology. In the present manuscript the reviewer misses a strong statement that such measurements are urgently required.

We agree and we added such a statement in section 5 (conclusion and perspectives) : p 25 l1-5 : "Determining the controling factors in nature with more confidence would however require much more data to more comprehensively evaluate GCM simulations of ^{17}O-excess. Continuous, in-situ water vapour measurements are needed in order to improve the understanding of the driving mechanisms of d-excess and ^{17}O-excess. In this regard the new laser-based techniques are extremely helpful for in-situ isotope measurements of water vapour. Yet, it is not possible, at least so far, to determine ^{17}O-excess with the required precision ($\leq$5 permeg) using this technology. Such measurements would be very helpful."

The paper is well structured and well written. However, it is only of interest to a specific community, mainly due to its dependences on empirical parameters that were incorporated as tuning factors in the model. Nevertheless, since it is the first approach of modelling 17O-excess using a General Circulation Model, I recommend publishing it with a focus for calling for new experiments mainly related to the limiting processes, i.e. supersaturation as well as re-evaporation and diffusive exchanges during rain falls.

Now we call for such experiments :

- regarding λ, in section 5 : p26 l 23-26 : "Measurements of vapor and precipitation along Antarctica transects would be very helpful to better constrain this function. New laboratory experiments focused on fractionation during ice formation in cold conditions would also be helful.".
  Also in abstract : p 2 l 21-22 : "sensitive to a parameter whose tuning would require more measurements and laboratory experiments."
  p 24 l 23 : "Estimating their magnitude calls for laboratory experiments."
- regarding ϕ : we add p 15 l 18-20 : "Simultaneous measurements of d-excess in both vapor and precipitation would be very helpful to ensure that tuning ϕ does not lead to error compensations."

Also, we stress that a value of the paper is also the description of the methodology to decompose isotopic variations into different components.

Detailed comments :

Abstract, third paragraph : The explanation of the balancing effects of distillation, transport and air mixing should be improved. It was rather difficult to follow unless further reading of the main text.

OK. We have now simplified and clarified it.

Intro, l 13 : It might be worthwhile to reference Sodemann's work as well in this regard

OK, we added [Sodemann et al., 2008] : p 3 l 12.

Intro, l 20 : Uemura et al., 2010 should be listed here as well as reference

OK : p 3 l 21.

P 5496, l 2 : Indeed stratospheric intrusions have not been discussed throughout the paper and they are also not mentioned in the conclusions to be at least part of the PD-LGM differences. It would be worthwhile to add a sentence on this issue.

- We added a sentence in introduction : p4 l1-5 : ". In Central Antarctica it may also be affected by stratospheric intrusions (Winkler et al., 2012a). Indeed, the ^{17}O-excess in the stratospheric vapor is so high (of the order of 3000 permeg (Franz and Roeckmann, 2005, Zhan et al 2006) relatively to that in the tropospheric vapor (a few tens of permeg maximum) that even a small flux of stratospheric vapor into the troposphere may affect significantly the tropospheric^{17}O-excess."
- We recall that this process is not taken into account by LMDZ : p 6 l 15-20 : "We do not consider the effect of methane oxydation on the stratospheric water isotopic composition ...but the role of these intrusions in the spatial and seasonal distribution of ^{17}O-excess and in its LGM-to-present variation is unclear. "
- We added a sentence on the PD-LGM differences : p 17 l 25-27 : "Note that stratospheric intrusions may contribute to the LGM-PD difference in ^{17}O-excess (Winkler et al 2013), but their effects is neglected in LMDZ."

P 5496, l 19 : . . .its difficulty to simulate some aspects of . . .., be more precise here,
what kind of aspects are not well captured by GCM models.

We added : p4 l 21-22 : "For example, simulated daily d-excess variations in the water vapor or in the precipitation are too flat ([Risi et al., 2010, Steen-Larsen et al., 2013]) and simulated d-excess variations at the paleoclimatic scale are always of opposite site compared to$\delta^{18}O$ even when observed variations are of the same sign ([Werner et al., 2001, Noone, 2008])."

P 5497, l 4-10 : this might be skipped.

This is a matter of taste, I like reading the outline in other people's papers. I have kept it but shortened it : p 5 l 7-11.

P 5497, l 18 : a statistical could scheme, it would be worthwhile to do some sensitivity tests with this approach regarding implications on water stable isotopes including second-order parameters. Has this been done ?

Unfortunately, we are not able at present to run any additional simulations with ^{17}O-excess, due to technical problems associated with the transition from vectorial to parallel machines.

However before the machine transition, sensitivity tests had already been done to the width of the sub-grid scale distribution of water vapor in the statistical cloud scheme used for large-scale condensation ([Risi et al., 2012]), though ^{17}O-excess was not included in these simulations. The sensitivity of $\delta^{18}O$ and D-excess to this parameter is limited to the upper troposphere where most of the large-scale condensation takes place. In the low-level vapor or in the precipitation, $\delta^{18}O$ and D-excess are not very sensitive to this parameter. Even for an extreme variation of the width parameter by a factor of 10, variations are less than 1‰ for $\delta^{18}O$ and for d-excess, except in central Antarctica where variations are up to 10 ‰ for $\delta^{18}O$ and 5‰ for d-excess.

We have also precised the description of the physical package, because the statistical cloud scheme of [Bony and Emanuel, 2001] is used to diagnose convective cloud fraction from a radiative point of view but not to diagnose condensation, so it has no direct impact on water isotopes.

We now write : p 5 l 19-23 : "... a statistical cloud scheme (Bony and Emanuel, 2001) which diagnoses convective cloud fraction from a radiative point of view. Precipitation can be created either by the convective scheme or by a large-scale condensation scheme. The large-scale condensation scheme is also based on a statistical cloud

scheme (Letreut and Li, 1991). The impact of tuning parameters in this statistical cloud scheme on water isotopic compositions are limited to the upper troposphere (Risi et al., 2012)."

P 5498, l 5 : A reference is required for your assumption that over land all evapo-transpiration occurs as non-fractionating transpiration.

Now we write : "We make the simplifying assumption that over land, all evapo-transpiration occurs as transpiration (e.g. [Hoffmann et al., 1998]), which is non-fractionating ([Washburn and Smith, 1934, Barnes and Allison, 1988]).

P 5498, l 20 : What is the difference, be more explicit.

We have replaced "different" by "more detailed" and we added the justification : p 6 l 26-29 : "at each time step, the model takes into account the evolution of the compositions of both the rain and the surrounding vapor as the rain drops reevaporate [Bony et al., 2008], whereas most GCMs take into account the evolution of the composition in the rain only."

It is interesting to note that water amount of the total column of a falling rain drop (from the cloud to the surface) is by far larger than the water in the rain drop. Therefore, exchange of the water molecules are expected during the fall. However, this has implications on the condensation temperature, which temperature has to be used ? It might be significantly different than the cloud temperature where the first condensation occurs.

We don't completely understand the point. Condensation and rain-vapor exchanges are treated as independant processes. They take place in different drafts (condensation in updrafts and rain-vapor exchanges in downdrafts). So rain-vapor exchanges don't affect condensation temperature. Now we clarify that theses processes take place in different drafts. In addition, maybe the misunderstanding arises from the fact that in simple models, many simplifying assumptions are necessary, whereas in GCMs, processes are represented more explicitely. Therefore, we added some more details on the condensation and reevaporation processes :

- We added details on condensation : p 6 l 22-24 : "In convective updrafts, condensation is assumed to be a closed process (i.e. vapor-condensate equilibrium) for the liquid phase (above -40°C) and an open process (i.e. Rayleigh distillation) for the ice phase (below 0°C)."
- We added details on the rain-vapor exchanges and on the temperature used : p 7 l 10-13 : "To calculate fractionation coefficients, the temperature at each level in the environment of the rain drops is used, i.e. in the unsaturated downdraft for convective precipitation or in the large-scale environment for large-scale precipitation."
- We recall that processes are vertically resolved : p 6 l 26 : "at each time step and at each level, "

P 5498, l 23 : . . ..with hddft being the relative. . .

OK : p 6 l 25.

P 5500 : It would be useful to compare results with the different forcing datasets, what implications would you expect for the results ? Percent range or much stronger ? It would be worthwhile to comment on this issue.

Sensitivity tests were performed using the IPSL forcing conditions, but without ^{17}O-excess. As explained above, unfortunately, we canot run any other simulations in the near future due to technical problems dring the supercomputer transitions. We added : p 8 l 17-19 : "As a consequence, evaporative recycling over high latitude oceans is too strong and $\delta^{18}O$ is unrealistically enriched at LGM (though d-excess is in slightly better agreement with observations) (Risi et al., 2010b). We are aware of the caveats of the CLIMAP"

When using the IPSL forcing conditions, the higher SST in high latitude ocean during the LGM leads to stronger evaporative recycling there, which leads to lower d-excess. Now we write in section 3.4 : p 17 l 21-23 : "When using the LGM SST forcing based on the IPSL climate simulation, the decrease of d-excess from LGM to PD is 1 stronger but has a similar shape (Risi et al., 2010b)."

P 5500, l 20 : How was this regridding done ? Just a linear approach ?

Now we write : p 9 l 1-2 : "regridded on the LMDZ grid by attributing to each LMDZ grid the average of all measurements falling into this grid."

P 5500, l 25ff : Have you also used a delay factor ? It might be possible that there Is an time offset.

Taking into account a delay would be useful to evaluate the results at the monthly scale. It is true that the seasonal cycle of river composition is delayed by a few months relatively to the seasonaly cycle in the precipitation (e.g. [Risi, 2009]). However, in addition to the delay, there is also a strong dampening of the seasonal cycle. This is why we compare with the annual mean-precipitation.

The delay is not the only limitation of our simple comparison. This is why we refrained from doing something too complicated. Now we write : p 9 l 10-20 :

"We compare observed composition in river water to the simulated annual-mean composition in the precipitation. In reality, river water composition integrates precipitation water over the previous months and over the entire watershed ([Kendall and Coplen, 2001]). It is additionally affected by evaporative enrichment ([Gibson et al., 2005, Risi, 2009]) and by temporal variations in drainage and runoff ([Dutton et al., 2005]). Coupling LMDZ with the land surface model ORCHIDEE ([Krinner et al., 2005]), equipped with a routing scheme ([Polcher, 2003]) and enabled with water isotopes ([Risi, 2009]), would be necessary to rigorously compare the model to river observations. This is beyond the scope of this paper, and this is why here we simply assume that river water is representative of the annual-mean precipitation. This assumption is justified by the fact that the isotopic seasonality in river water is usually strongly dampened relatively to that in the precipitation ([Kendall and Coplen, 2001])."

P 5501, l 13ff : Until the calibration issue is not fully understood, it should be handled with care, even for small ïAd'18O variations.

^{17}O-excess measurements have been performed in different laboratories (HUJI, LSCE) on same samples. These measurements are compared in Winkler et al. (2012) and Landais et al. (2012). Results on seasonal cycles and last deglaciation are very similar. This inter-laboratory calibration is done so that ^{17}O-excess of SMOW = ^{17}O-excess of SLAP = 0 permeg as measured at HUJI. This is also the calibration that has been proposed by [Schoenemann et al., 2013] so that all published ^{17}O-excess data are coherently calibrated with respect to the values of two international standards, exactly as done for δD and $\delta^{18}O$.

In the paper, we re-write the paragraph to try to clarify the difference between what cannot be trusted and what can be trusted : p 10 l 3-13. We insisted on being caution : p 10 l 8-9 : "This calls for caution when interpreting ^{17}O-excess results."

P 5502, l 6 : You exclude sublimation as potential post-condensational effect why ? The reviewer cannot follow the authors argumentation that for solid precipitation no post-depositional effects occur.

Maybe the misunderstanding comes from the distinction between post-condensational and post-depositional effects. Post-condensation effects occur from the condensate to the surface precipitation. Post-depositional effects are from the surface precipitation to the snow or ice. In this paper, we discuss only post-condensational effects.

- Regarding fractionation during sublimation, we now write : "The diffusion of water molecules in ice is too low to allow for isotopic exchanges during the fall of snow ([Jouzel, 1986])." : p 10 l 24-25.
- We acknowledge the existence of post-depotional effect, which we neglect : we now write : For snow, we neglect post-depositional effects ([Taylor and Renshaw, 2001, Gurney and Lawrence, 2004, Ekaykin et al., 2009, Lee et al., 2010]) : p 9 l 4-5.
- We show that the neglect of post-depositional processes for seasonal time scale in shallow cores is reasonable : p 9 l 6-8 "This is a reasonable assumption since seasonal cycles of $\delta^{18}O$, d-excess and ^{17}O-excess measured in shallow cores compare well with those measured directly in the precipitation (Landais et al., 2012b).". In addition, unpublished results at Dome C show similar results.

P 5502, l 15 : delete constant

OK

P 5503, l1 : Assuming that all processes add up linearly. . .. . .this is a strong assumption that requires information, either a reference or a sensitivity study with two different model settings.

The decomposition is mathematically exact since the first term is calculated as a residual. What might not be exact however is the physical meaning of the first term, which may be contaminated by some non-linear combinations of the other terms. We quantified an upper bound of this contamination in the supplementary material.

In the paper, we add : p 11 l 25-28 : "Note that the assumption that all processes add up linearly is valid for $\delta^{18}O$ and for ^{17}O-excess, but may lead to uncertainties of up to 1‰ for d-excess in very cold regions. In the remaining of the paper, we will focus on d-excess variations larger than this uncertainty."

P 5504, l 28f : Underestimation of rhs and SST, could this underestimation be caused by an under representation of air mixing effects ? This relates to the mismatch of the water vapour results which are in contrast to the precipitation results (disregarding the underestimation of the changes by the model). It would be helpful to give information about vertical gradients of water stable isotope parameters for future measurement campaigns what to expect or where such experiments should be performed.

- We agree that information about vertical gradients of water stable isotopes would be helpful. We added a figure on that (now fig 2).
- The hypothesis that the underestimation of the sensitivity to rhs and SST may be due to mixing effects is investigated a bit further in the text, as the "Third" hypothesis. Sensitivity tests to boundary layer parameterization did not help. We rewrote this paragraph : p 13-14 l 25-3 : "Third, there could be problems in the boundary layer parameterization. If the boundary layer mixing is too strong, then the evaporative signal in the near-surface vapor may be dampened by advection of free-tropospheric air. Simulated latitudinal gradients of d-excess and of ^{17}O- excess in the free troposphere are smoother (or even reverted in the case of mid and upper tropospheric d-excess) than near the surface (Fig. 2). A weaker vertical mixing might thus improve the results. To test this hyothesis, the mixing length scale used in the boundary layer parameterization is halved (Fig. 1 purple). However, this does little to improve d-excess or ^{17}O-excess. "
- We also add : p14 l 11-12 : "Measurements of horizontal and vertical gradients in water vapor composition during cruises and aircraft campaigns would be useful to elucidate this problem."
- When discussiong the effect of convective-scale subsidence, we now refer ti fig 2 and stress that vertical profiles of d-excess and ^{17}O-excess are still very uncertain : p 22 l 14-15 : "For d-excess, the vertical gradient in the tropics remains an open question. LMDZ simulates a decrease with altitude (Fig. 2a)".

P 5507, 3.3. It is puzzling that seasonalities agree well between data and model but the latitudinal gradients are underestimated by the model. Why is this ? Has it to do with the tuning parameter phi ?

The latitudinal gradient in precipitation d-excess is well simulated. The problem is for the latitudinal gradient in the vapor.

1. Generally, LMDZ simulates the precip right and the vapor wrong. This is indeed related to ϕ. The tuning of ϕ affects strongly the dexcess of precipitation, especially the latitudinal gradient and the seasonality in the tropics and sub/tropics. Since the tuning of ϕ was performed to optimize the d-excess in precipitation, it's expected that d-excess in precipitation is well simulated. This good simulation might reflect some error compensation. In contrast, the dexcess in the vapor is little sensitive to ϕ. We tried to explain that in the previous section : p 15 l 15-18 : "It could also be that the correct values of precipitation d-excess arises from a compensation of errors. In particular, the parameter ϕ controlling kinetic fractionation during rain re-evaporation was tuned to optimize precipitation d-excess".
2. Specifically, LMDZ simulates the precip seasonality right. We add : p 16 l 14-16 : "This may be because d-excess seasonality in these regions arises from processes other than changes in evaporative conditions"

P 5509, l. 26f : The reviewer could not verify the sentence that the sum of all these contributions make the total signal (black line). The sum of the 4 contributions does not match the black line in all figures 7-9. Please explain it.

There was a mistake in the red curve when substracting the zonal mean. Now it is corrected and the sum is the black curve.

P 5515, l 5-25 : This is still very speculative. To sum up large effects of different signs without having good confidence in their magnitude is weak. Again this calls for laboratory experiments.

- We added : section 4.4, p 24 l22-23 : "Summing up large effects of different signs without having good confidence in their magnitude leads to strong uncertainty. Estimating their magnitude calls for laboratory experiments."
- Regarding the call for experiments, we also write :
  - abstract, p 2 l 20-22 : "The effect of supersaturation is however very sensitive to a parameter whose tuning would require more measurements and laboratory experiments."
  - section5, p 26 l 24-26 : "New laboratory experiments focused on fractionation during ice formation in cold conditions would also be helful."

P 5516, l 0-15 : Stratospheric intrusions should not be ruled out to add the at least part of the measured signals in 17O-excess.

We added : p 25 l 11-12 : "In addition, stratospheric intrusions cannot be ruled out to explain at least part of the measured signals in ^{17}O-excess (Winkler et al 2013)"

P 5519 : Acknowledgements should be bold and increased in size.

This is not our choice. We use the copernicus.cls and we followed the template provided by Copernicus. It looks like they want the acknowledgements this way. I checked that in published Copernicus papers, all acknoledgements are this way.

P 5533 : The term mixing as mentioned in table 5 should be defined in the main text. What do the authors mean with mixing ?

- We added in the caption : "The physical meaning of these processes are detailled in Sect. 4. Mixing refers to mixing of water vapors of different origins."
- As often as possible, we replace "mixing" by "mixing between vapors of different origins"
- In the main text, in sect 4 p 23 l 2-6 : "mixing between vapor of different origins (hereafter shortened as "mixing"). In particular, mixing includes evaporative recycling along trajectories, i.e. mixing between vapor undergoing distillation during its poleward transport and newly evaporated vapor from the ocean surface.".
- early in the text, we explain the consequences of mixing for ^{17}O-excess : section 1, p 3 l 27-29 : "for example, mixing vapors of same 17 O-excess and different 18 O leads to lower 17 O-excess in the mixture (Risi et al 2010c)".

P 5538 : Fig. 5 : I found only 5 bins not 6.

OK

Fig. 7-9 : Single effects do not add up to give the black line. Check this.

Now we have checked this and corrected fig 7 (now 8). For figs 8-9 (now 9-10) it was all right.

2 Reviewer 2

We thank reviewer 2 for his/her comments.

General :
The paper attempts to examine the question of controls on 17O-excess and D-excess using the LMDZ atmospheric model. The results are of interest to the CPD community and the paper generally well structured. The main finding appears to be that the LMDZ model is not ideally suited to answering the question, which unfortunately leads to ambiguous results. Despite this negative result, the paper is of interest and merits publication in CP after some revisions.

Specific comments :
The title, as is, is somewhat misleading. It would be more accurate to title the paper

"Can we determine what controls the spatio-temporal distribution of d-excess and 17O-excess in precipitation using the LMDZ general circulation model ?"

OK.

Sub-sectioning and writing is quite poor in places, and requires improvement. Some instances are picked out below. However it would be worthwhile authors re-examining all sections.

We have kept the main idea of the outline. But we have tried to reorganize section 4 on controls. Now each subsection represents a color on figures 8-10.

Integer values under ten are usually written using works rather than numbers e.g. "two terms" rather than "2 terms".

OK

P5494,L26 GCM abbreviation before definition

OK

P5495,L 14-15 Make clear the different between real processes and GCM representations of processes

OK : p 3 l 13 : "In models, its representation is very sensitive..."

L17 reference for 17O-excess definition

OK. We added Landais et al 2006.

P5496,L1-2 Rephrase 'less conventional'

OK, we simplified the sentence : p 4 l 1-2 : "affected by stratospheric intrusions"

L15 'irreplaceable'-> 'invaluable' ?

OK

P5501,L4 Is Vostok data corrected for flow ?

Yes. We added : p 9 l 24-25 : "In Vostok the flow is taken into account in the age scale, though this has little impact on the last glacial-interglacial transition.".

P5504,L20-25 Can this be condensed ?

Yes, we removed one sentence.

P5505,L22-25 Seems rather weak, rewrite ?

What do you mean by "weak" ? We would need more information about what is wrong to know how to rewrite this sentence. In the meanwhile, we have complemented this sentence by a call for measurements of horizontal and vertical gradients, to try to end on a more constructive/positive note : p 14 l 11-12 : "Measurements of horizontal and vertical gradients in water vapor composition during cruises and aircraft campaigns would be useful to elucidate this problem."

P5508,L1-2 Difficult to understand this sentence.

Now : p 16 l 12-13 : “It is surprising that although LMDZ underestimates the d-excess sensitivity to evaporative conditions (Sect. 3.1), LMDZ is able to capture the observed d-excess seasonality”

L5 Spell out lambda as supersaturation.

OK

L12 Spell out the parameter.
OK

L18-20 Make the meaning of this sentence clearer.

Now : p 17 l 2-5 : “However, even with $\phi = 0$ (maximum kinetic fractionation during reevaporation), ^{17}O-excess is only 3 permeg lower during the dry season than during the wet season. Therefore, processes other than reevaporation may be at play in this region, and LMDZ does not capture them. “

L21 ”Observed” 4 permil ?

OK

L22 ”can” -> ”may” ?

OK

P5509 I like this decomposition.

Thank you.

P5511,L6 ”not affected as much”

OK

L18-26 A bit confusing fractionation coefficient effect ?

Yes, we added that.

Generally ordering in these sections would probably be clearer if it was consistently d18O, then d-excess, then 17O-excess.

Yes, now we have done that in all subsections of section 4, except when d-excess and ^{17}O-excess behave exactly the same, in which case we put both in the same paragraph.

P5512,L11-12 Why is this the case reference or hypothesis ?

We have clarified the rationale : p 21 l 17-19 : “However, we have shown in Sect. 3.1 that LMDZ underestimates the slope of ^{17}O-excess as a˜function of RH_s. Therefore, in nature the role for evaporative conditions might be stronger.”

L19 ”Distillation (red)” confusingly marked otherwise on fig.

Now : p 20 l 27 : ““other processes” (red curve, detailed in section 4.3)”

P5514,L17 ”This decrease is” ?

Now : section 4.3.2, p 23 l 9-10 : “In reality and in the simulations, this latitudinal gradient is dampened...”

L18 "trajectories"

OK

P5515,L1-3 Would be clearer if effects described, rather than line colours.

Now : p 23 l 10 : "distillation and mixing (red curve)". Also p 23 l 10-11.

L23 "Risi et al (2010) chose lambda to optimise..."

OK

P5516,L5-16 Why is this in the supersaturation section. Should be a separate section/subsection.

We agree, it's related to the distillation processes, so we moved it to section 4.3.2.

L19 Rephrase "very well"

We replaced by "correctly"

L24 "Exhibits caveats" ??

Now : "has difficulties to simulate"

L27 "factors"

OK

P5517,L17,18 Rephrase "push towards"

Now : p 26 l 15-16 :"At LGM in polar region, distillation and mixing effects tend to increase d-excess and ^{17}O-excess values, while supersaturation effects tend to decrease them."

P5518,L5 Rephrase "fractionating evaporation"

Now : p 27 l 4 : "fractionation during bare soil evaporation (e.g. [Gat and Matsui, 1991])"

L8-10 Rewrite last sentence.

Now we write : p 27 l 7-10 : "Applying this methodology to other GCMs will help extract robust features among models. If in the future, some GCMs are able to better simulate d-excess and ^{17}O-excess, applying this methodology to these GCMs will help understand what controls d-excess and ^{17}O-excess with more confidence."

Fig7-9 Confusing that legend/lines/decomposition varies between figures.

We have uniformized the legends and colors. We have removed the blue curve for consistency between all figures.

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