# Response to reviewers

July 25, 2014

We thank our two anonymous reviewers for their constructive comments. We made our best to address them. The outline of the paper has been modified. Our point by point response is below in blue.

## 1 Reviewer 1

We thank reviewer 1 for his/her constructive comments.

General Comments

This paper shows a detailed comparison of water isotopic composition simulations to measurements, for a specific site, and how such a comparison can help to diagnose the source of model biases. The paper is well structured, presents novel concepts and data, and reaches a fair amount of conclusions. I do have a list of specific ques- tions, comments and technical corrections, though. The paper is recommended for publication in ACP after these minor revisions have been taken into account.

Specific Comments

p. 4461, line 17: What is the temporal resolution of the model?

We added this information in section 2.2: "The time step for the resolution of the dynamical equations is 1 minute." and then "The physical package is described in detail by [Hourdin et al., 2006] and called every 30 minutes."

p. 4461, lines 20-22: "no distinction is made between transpiration, bare soil evapo- ration, or intercepted water by the canopy". We know non-fractionating transpiration has a different impact on dD compared to fractionating evaporation, and the impact can be significant on dD in the lower atmosphere, e.g. in the Tropics. Therefore, this rises the question of how important this lack of distinction is for the conclusions of this study. For example, one of the conclusions is that LMDZ could have a problem with amount of surface evaporation. How would this conclusion change if the surface evaporation would be more enriched by including transpiration? This same question could be asked in relation to the high bias in dD that seems related to the air mass origin: would this conclusion be sensitive to the inclusion of enriched transpiration effects? Could the authors elaborate? Such a discussion might be worthwhile to include in the Conclusions section. Also, are there perhaps other models that do take the distinction in fractionation between evaporation and transpiration into account? Could these be used for a sensitivity study to the effects of transpiration?

- We are now more precise in the description of the representation of land surface fluxes: section 2.2.2: "Land surface evaporation is calculated as a single flux, represents all components of evapo-transpiration. No distinction is made between transpiration, bare soil evaporation, or evaporation of water intercepted by the canopy. For water isotopes, we assume that transpiration is the dominant component of evapo-transpiration (e.g. [Williams et al., 2004, Jasechko et al., 2013]). This approximation is especially reasonable in Siberia ([Iijima et al., 2014]). No fractionation is associated with transpiration ([Washburn and Smith, 1934, Barnes and Allison, 19 Thus we neglect fractionation during evapo-transpiration, as in most GCMs (e.g. [Hoffmann et al., 1998]). "
- To estimate the impact of nelecting fractionationg evapo-transpiration, we looked at sensitivity tests using the LMDZ-ORCHIDEE model: "... we used a few additional simulations in which LMDZ was coupled with a more sophisticated, state-of-the art land surface scheme called ORCHIDEE (ORganizing Carbon and Hydrology in Dynamic EcosystEms, [Ducoudré et al., 1993, Krinner et al., 2005]) enabled with water isotopes ([Risi, 2009,

Risi et al., 2013]).". These simulations are not those used in the core of the paper because they are available for the year 2006 only.

- In ORCHIDEE, we can disable the isotopic fractionation during land surface evaporation to test its impact without changing anything in the climate and water cycle. We compare simulations with and without fractionating evaporation:
  - Results for the East-West and North-South gradients in $\delta D$ and d-excess are explained in section 3.3.3 entitled "Impact of the representation of fractionating evapo-transpiration".
  - Results for the daily variability in $\delta D$ are explained in section 5.2.3 entitled "Impact of the representation of the land surface".
  - We have added new tables to summarize the results:
    * table 1: list of the sensitivity tests with ORCHIDEE
    * table 3: Characteristics of the spatial distribution in q, $\delta D$ and in d-exces for the different tests
    * table 8: Characteristic of the daily $\delta D$ vs $ln(q)$ distribution in JJA

pp. 4461-4463: I miss a description of the time series covered in this study. Although the start date of certain measurement periods are described, no end date is mentioned. Please mention somewhere (in Sect. 2?) which exact period you consider for this study.

We added this information in section 2.4:

- for the vapor: "The instrument has provided continuous measurements of $\delta D$ and $\delta^{18}O$ since April 1st, 2012, and we use the data up to the end of 2012."
- for precipitation: "We use the data from the end of October 2012 to December 2012."

p. 4464, Eq. 1: does the tilde symbol in the last term mean "approximately proportional to"? Since the expression is only truely proportional to "ln(dD/1000 +1)". This should be explained, either in the equation (e.g. by using the direct proportionality symbol (\propto in LaTeX) and the term "ln(dD/1000 +1)"), followed by a sentence of how this can be approximated, or by explaining in the text what the tilde symbol () in Eq. 1 means and why it is used.

We have removed the "approximately equal" part of equation 1 and the Rayleigh distillation is written only for $R_v$. The approximation is clarified a few sentences later: "Since $R_v$ remains close to unity, $\delta D$ can be approximated by: $\delta D \simeq ln\left(R_v\right) \cdot 1000$. "

p. 4466, lines 15-16: from Fig. 2 it is not obvious that "domain average values" have been subtracted from the satellite data. Which values have been subtracted, and shouldn't the values in Fig. 2 represent this by showing the variations around this domain average (like in Fig. 3, for example)? Also, mentioning the subtracted values could be worthwhile for readers who are interested in these possible biases of the satellite data.

Correct. We did not need to subtract the mean values on these plots. We added this clarification in the caption: "Note that $\delta D$ values without any subtraction are plotted here because LMDZ happens to show values similar to those of GOSAT and TES."

p. 4467, lines 10-16: earlier it was mentioned that the model makes no distinction between transpiration and evaporation. So is it not misleading to speak of evapo-transpiration here? In fact, could this lack of distinction not play a role in the underestimation of the latitudinal gradient (via gradients in the vegetation coverage perhaps)?

- Now we explain what we call "evapo-transpiration": section 2.2.2: "Land surface evaporation is calculated as a single flux, represents all components of evapo-transpiration. No distinction is made between transpiration, bare soil evaporation, or evaporation of water intercepted by the canopy."

- Yes, the lack of fractionating evaporation may play a role in the underestimation of the latitudinal gradient. Now we quantify this effect in section 3.3.3 and table 3. We show that the effect is very small: "To summarize, neglecting isotopic fractionation during bare soil evaporation does not appear to be a major caveat of our study."

p. 4467, line 23: with the word "this", it is implied that LMDZ captures the trend of a decreasing d-excess, followed by an increase, as shown by Masson-Delmotte et al. 2008. This seems a bit too optimistic, as LMDZ only shows a decreasing trend. This nuance should be added to the text.

We clarified our explanation: now section 3.1.3: "In spite of the noisiness in the data, a decreasing trend with latitude can be observed. This could be associated with the Rayleigh distillation, which first decreases d-excess until about -20°C and increases it below ([Masson-Delmotte et al., 2008]). In Siberia, only the decreasing trend can be seen because temperature are infrequently below -20°C. LMDZ captures the d-excess decrease with latitude, with a decrease from 14‰ at 35°N to 5‰ at 70°N (Fig. 4d)."

p. 4470, lines 1-2: "There is no relationship between the seasonality in dD and in q." This is a very strong statement that should be backed-up by either a figure or a reference. It seems very unlikely, looking at the rather strong correlations between dD and q shown for example in Figs. 6 and 7 and the discussions earlier in the paper. I guess that a relationship between the seasonalities in dD and in q is actually to be expected, but it is the shape (or variability) of this relationship that points to undetected physical processes.

We clarifiy what we mean: section 3.4: "There is no relationship between the amplitude in the seasonal cycle in $\delta D$ and that in $q$ among the different SWING2 models (Fig. 8c)."

p. 4470, lines 10-11: how was the spatio-temporal matching of the LMDZ model at the Kourovka site performed? Maybe this can be mentioned in 1 or 2 sentences?

We added in section 4.1: "For LMDZ, we use outputs from the closest grid point from Kourovka. Since LMDZ is nudged by reanalyses, it captures the daily variations in circulation, so that it is possible to make a day-to-day comparison."

p. 4472, lines 13-19: It would be useful to present the average values of d-excess of the observations and the model in Fig. 9b (this was well done in the previous paragraph, so why not repeat it here?). But even without these average values, it seems that the d-excess in precipitation observations (red dots in Fig. 9b) are higher than the model values. The model values actually seem consistent with the expected value of -3 per mil (corrected for altitude). Therefore, the concluding remark "The fact that the snow and vapor from observations and simulation have a similar d-excess is currently not well understood" does not seem to be justified?

We have rewritten this paragraph to take into account this comment: section 4.2: "The d-excess in observed precipitation is similar to the d-excess in surface water vapor: observed $d_p - d_v$ is 3‰ on average (Fig. 11b). LMDZ simulates $d_p - d_v$ values of -2‰ on average. If the snow was in equilibrium with the surface water vapor, $d_p - d_v$ would be about -5‰. The d-excess increases with altitude and the vertical gradient between the surface and 2 km is 1‰/km. Taking this effect into account, $d_p - d_v$ should be about -3‰. This theoretical estimate is very consistent with what is simulated by LMDZ. Why observed $d_p - d_v$ is 3‰ rather than -3‰ could be due to microphysical processes or post-condensational processes. However, the large spread of $d_p - d_v$ values prevent us from concluding for sure that observations are inconsistent with LMDZ and with the theoretical estimate."

p. 4473, lines 4-5: The statement: "In Sect. 3.2, we showed that LMDZ reproduces well, at least qualitatively, the seasonal and daily variations in q and water vapor dD at the surface." seems a bit too optimistic: Fig. 5 in Sect. 3.2 does NOT show q, and does NOT show daily variations (only monthly variations are shown). These are discussed in Sect. 4.1, though. Please correct these references, including the corresponding figures numbers within parentheses.

- Now we have added the evaluation of precipitable water on figures 2, 3, 4 and 5, and in the text in sections 3.1.1, 3.1.2 and 3.2.

- We have clarified where the seasonal and daily evaluation was performed (now section 5.1: "We showed that LMDZ reproduces well, at least qualitatively, the seasonal (sect. 3.3, Fig. 6) and daily (sect. 4.1, Fig. 9) variations in $q$ and water vapor $\delta D$ at the surface."

p. 4476, Section 5.2.1: The values in Table 3 are probably derived from a theoretical study using LMDZ? This might not be entirely clear to all readers and should therefore be mentioned (readers might wonder where the "true" values for temperature and relative humidity come from).

This data comes from meteorological data. We have added this information in section 2.3.2: "Basic meteorological measurements were performed on a meteorological station in the nearby town of Yekaterimburg. We use the temperature measurements to better interpret our $q$ measurements."

p 4478, lines 10-11: "When LMDZ has the largest enrichment bias in dD, LMDZ has also the largest moist bias in q" and also p.

4480, lines 3-4: "LMDZ exhibits the strongest dry bias on days when it simulates the strongest enriched bias in dD". Strictly speaking, these statements are not true. Looking at the JJA values in Fig. 12b, the strongest enriched bias of 50 per mil corresponds to biases in ln(q) of about 0.0 (so the smallest). Similarly, biases of -0.8 in ln(q) (the largest negative bias) correspond to biases in dD of 0 (the smallest). The statements are only true when using the "delta" terminology, but not in the absolute sense of biases. So those two sentences need to be rephrased, using the terms delta-delta-D and delta-ln(q) from Fig. 12b. For example, it is true that the largest values of delta-ln(q) correspond to the largest values of delta-delta-D (eventhough the largest values of delta-ln(q) correspond to the lowest absolute biases in humidity).

We agree. We have changed the wording now at all occurences.

Technical Corrections

*** Abstract:

The abbreviations LMDZ, GCM, TES, GOSAT, GNIP, SNIP and SWING2 need to be defined separately in the abstract.

Done. For SWING2 models, we have added table 1 that expends all the model names.

p. 4458, line 5: in-situ -> in situ

*** Sect. 1. Introduction:

p. 4459, line 1: there's a word (probably "Europe") missing after "Central and East- ern..."

p. 4460, line 18: the abbreviation "LMDZ" needs to be introduced.

*** Sect. 2. Data and methods:

p. 4461, line 2: subscript "standart" -> standard

p. 4461, line 6: represent -> represents

All these items have been corrected

p. 4461, line 18: by (Hourdin et al., 2006). -> by Hourdin et al. (2006).

OK. We have a few bibliography formatting problems. They will be solved during copy-editing.

p. 4462, line 3: it is not clear why there is suddenly a "4" behind LMDZ.

Now: "We use the fourth version of LMDZ (called LMDZ4) "

p. 4462, line 4: "From the other hand" -> "On the other hand"

p. 4463, line 26: the abbreviation "GOSAT" needs to be introduced.

p. 4464, line 3: the abbreviation "TES" needs to be introduced.

p. 4465, line 11: "...given q (blue)." -> ...given q (blue and pink). (in stead of pink, I think magenta is actually a better description of the color).

p. 4465, line 12: add commas before and after the word "however"

p. 4465, line 17: sensitive TO evaporation
p. 4465, line 18: "on Fig. 1" -> "in Fig. 1"
p. 4465, line 24: "overestimate" -> "misrepresent" (since it could be both an overestimate or an underestimate)
*** Sect. 3. Model evaluation of spatial and seasonal variations:
p. 4466, line 22: "features are decreasing d-excess trend" -> "features are showing a decreasing d-excess trend"
p. 4467, line 1: add a comma after "evaluation"
p. 4467, line 9: "to conclude." -> "to draw conclusions."
p. 4467, line 9: "underestimate" -> "underestimation"
p. 4467, lines 28-29: "Simulated d-excess is less noisy than in observations" -> "THE simulated d-excess is less noisy than in THE observations"
p. 4468, lines 20-23: "underestimate" -> "underestimation" (4 times)
p. 4469, line 3: "over-estimate" -> "over-estimation"
Fig. 6: the labels c) and b) in the figures seem to be switched ( c) should be b) and b) should be c) )"

All these items have been corrected

*** Sect. 4. Evaluation over Kourovka:
p. 4470, line 10: this is the first time the term "LMDZ-iso" is used (as compared to "LMDZ"). This leads to confusion and should be avoided. If there is no good reason to explicitly mention "-iso", I would suggest to remove it everywhere in the paper (it is also used in the captions of Tables 1 and 2 and Fig. 11). Or else, at least state clearly that sometimes "LMDZ-iso" is used in stead of "LMDZ" without any difference in meaning.

Now we use LMDZ everywhere.

p. 4470, line 10: "Comparison" -> "A comparison"
p. 4470, line 10: "of LMDZ-iso simulation" -> "of THE LMDZ simulation"
p. 4470, line 11: "on Fig. 8" -> "in Fig. 8"
p. 4470, line 13: "values" -> "correlation coefficients"
p. 4470, line 15: "between model and observations" -> "between observations and model"
p. 4470, line 17: "A day bias" -> "A dry bias"
p. 4470, line 24: "about 20 per mil too enriched" -> "enriched by about 20 per mil"
p. 4471, line 2: add a comma after "period"
p. 4471, line 3: "than observed values" -> "than the observed values"
p. 4472, line 3: "around 2 km on average" -> "around an altitude of 2 km on average"
p. 4472, line 4: "The dD decreases" -> "dD decreases"
p. 4472, line 6: "from a water vapor" -> "from water vapor"
*** Sect 5. Processes controlling water vapor and dD:
p. 4474, line 13: "spring to summer is associated" -> "spring to summer (Fig. 8a) is associated"
p. 4474, lines 15, 18 and 22: please remove the parentheses around "(g kg-1)"
p. 4474, lines 26: "(0.40)" -> "(r=0.40, see Table 2)"
p. 4476, line 17: "Another possible" -> "A possible" (if I am correct, there was no other possible cause mentioned earlier)
p. 4476, line 19: "at surface" -> "at the surface"
p. 4476, line 19: "on coarser mesh" -> "on the coarser mesh"
Tables 3 & 4: please provide units of q (or delta q) and dD (in the captions).

All these items have been corrected.

p. 4477, line 14-16: "the fractionation coefficients": Which fractionation coefficients were assumed, and which condensation temperatures?

Now we write: “We calculate the fractionation coefficient using the same temperature as the one used to calculate the saturation specific humidity.”

p. 4480, lines 13-18: the meaning of the term "end member" is unclear and should be explained.

Now we avoid this unclear word: e.g. section 6: “the composition of the water vapor from the different air masses that are being mixed”

*** Tabels and Figures

*** Caption of Table 1: " results of simulation by LMDZ-iso calculated to..." -> "LMDZ simulation results of..."

Caption of Table 1: "At DJF season data available" -> "At the DJF season data was available"

Caption of Table 1: "If p value" -> "If the p value"

Caption of Table 1: "5% then we assume" -> "5%, we assume"

Table 2: The ratios of the standard deviations are not discussed in the text, and don't seem to add a lot of new information. I would therefore suggest to remove these columns from Table 2.

Table 3: Please mention the units.

Table 4: Please mention the units.

All these items have been corrected.

## 2 Reviewer 2

We thank reviewer 2 for his/her constructive comments.

Review results of Gryazin et al. "The added value of water isotopic measurements for understanding model biases in simulating the water cycle over western Siberia".

In this manuscript, the authors made a set of intensive analyses to investigate the reason of AGCM bias, specifically dry (and warm) bias over Western Siberia, focus- ing on the additional information by using water isotopic information. They used an isotope-incorporated AGCM compared with multiple isotopic datasets, including satel- lite retrievals for vapor isotope, precipitation isotope network, and in-situ daily surface vapor and precipitation isotopes. In conclusion, they found two significant biases in their model: the first is the systematic and independent overestimation of isotope and the second is bias associated with humidity. The reasons of these biases are also suggested in air mass origin and in horizontal advection and/or surface evaporation.

Overall, the manuscript is well written and the interpretation of the analyses is techni- cally convincing. However, title of this paper, "added value" by water isotopic informa- tion is not fully appropriate. As the authors pointed out, the continental dry/warm bias is a very big issue in the climate modeling community. If isotopic information can solve this issue or provide a unique hint to solve it, it would be indeed great contribution. But the analyses in the paper are still mainly targeting "the reason of bias in the isotopes". The community already knows that something is wrong in the hydrologic cycle, espe- cially in the terrestrial processes as mentioned in the paper. It is important to clearly show what we can know with isotope information where we could not have known with- out the isotope. In this regard, I don't think that they have showed the "added value" clearly enough.

Again, I like the paper very much and their analyses are technically quite reasonable. My comment above is indeed challenging and a step forward for the water isotope community. I think the authors are the one of the closest for such achievement and the paper is almost showing the potential. That is the reason why I request the authors to show more apparent and convincing "added value" to the readership. This is my major comment.

- We completely agree with the reviewer's point of view.
- We have tried our best to go deeper in our analysis. However, we couldn't find how to use the existing isotopic measurements to identify for sure the cause of the model summer dry bias in humidity. We were a bit disapointed and our additional analysis led us to be less optimistic about the potential “added value” of water isotopes. As a consequence:
  - we have changed the title into: “To what extent could water isotopic measurements help us understand model biases in the water cycle over Western Siberia“.
  - In introduction, we have decreased our ambitions: “we investigate to what extent comparing the simulated water stable isotopic composition of water vapor to measurements can help us diagnose the source of model biases over continental regions in summer. “

– We have added limitations in the abstract: “However, $\delta D$-$q$ diagrams using the available data do not tell the full story. Additional measurements would be needed, or a more sophisticated theoretical framework would need to be developed.”

- We have added sensitivity tests to the land surface scheme to explore the effect of the representation of the land surface. This is detailed in sections 3.3.4 and 5.2.3, and tables 3 and 8.
- We have added a more detailed discussion on the $\delta D$-vs-$ln(q)$ slopes. Figure 14c compares the slope at the daily scale at Kourovka in summer to the slopes for the latitudinal and longitudinal gradient in average over the summer. This is discussed in section 5.2.2.
- In the conclusion, we added a discussion of what we can do and cannot do with the $\delta D$-vs-$ln(q)$ diagrams. e.g. section 6: “However, even using such diagrams, it is difficult to discriminate for sure between Rayleigh lines and mixing lines. In addition, different kinds of $\delta D$-$ln(q)$ regressions may have the same slope”.

Other than this, there are some minor issues as follows:
P4458 L16: "strongest dry bias" and "strongest enriched bias in dD". Is it true? Figure 12(b) does not show such relationship.

reworded: “LMDZ simulates the strongest dry bias on days when it simulates the smallest enriched bias in $\delta D$.”

P4458 L19: the moist bias -> the dry bias

OK

P4460L18: Spell out LMDZ

OK

P4461L19: Is this simple representation only for isotope or as a whole land surface model? If latter, the land process is apparently too simple regarding the current improvement in the land surface models. This would not be the case in the other latest GCMs, therefore the conclusion of this paper (i.e., insufficient evapotranspiration bias) would be only model-dependent.

- This limitation has been added: “The representation of land surface is much simpler than in current coupled models used for CMIP3 ([Meehl et al., 2007]) or CMIP5 ([Taylor et al., 2012]). Therefore, some of the conclusions reached in this paper regarding the role of land surface processes might be model-dependent and specific to GCMs with very simple land surface schemes.”
- We have tried to asses the impact of the representation of the land surface on our results: “To check to what extent our results are sensitive to the representation of the land surface, we performed different sensitivity tests, by varying the stomatal resistance, the soil capacity or the fraction of the surface covered by bare soil.”. Results of these tests are added in the paper (tables 3 and 8).

P4462L3: What is LMDZ"4"?

We now write: “the fourth version of LMDZ (called LMDZ4)”

P4462L12: Spell out the abbreviations. And add the references for the models.

Now we have added table 1 that spells out all model names and gives references.

P4463L9: Briefly explain the observation system configurations. Particularly the calibration interval needs to be specified.

We have added these explanations: section 2.4: “A detailed overview of the WS-CRDS measurement system setup, calibration, and maintenance can be found in [Bastrikov et al., 2014]. In summary, every six hours of ambient air measurements are followed by a two-standards calibration lasting 30 minutes for each reference water standard using Picarro Standards Delivery Module. The liquid standards are vaporized at 140°C using Picarro Vaporizer Module A0211, then mixed with dried room-air dessicated with drierite (W.A. Hammond Drierite Company, Ltd., USA) and measured by the analyzer. The water standards have been calibrated on the VSMOW-SLAP (Vienna Standard Mean Ocean Water - Standard Light Antarctic Precipitation) scale by accurate laboratory mass spectrometer measurements at LSCE (Laboratoire des Sciences du Climat et de l’Environnement). ”.

P4463L10: What is the time interval of precipitation sampling?

We have added this information: “at the daily time scale”

P4464L22: What is R? The definition is different from the one used in L4461L3.

The definition is the same in both equations. We have clarified the definition in the first occurence: section 2.1: “$R$ is the ratio of $HDO$ or $H_2^{18}O$ to $H_2O$. “

P4464L22: What is ""? This symbol is not standard for "nearly equal".

Now we write: “$\delta D$ can be approximated by: $\delta D \simeq ln\left(R_v\right) \cdot 1000$”

P4466L2: Delta(ln(Rv)) and ln(DeltaRv) are different. Please clarify.

This is not what we meant. We have clarified this: “Since $R_v$ remains close to unity, $\delta D$ can be approximated by: $\delta D \simeq ln\left(R_v\right) \cdot 1000$”.

P4466L15: I don’t see any subtraction in Figure 2 a-d.

Correct. We have clarified this: “Here we plot $\delta D$ values without any subtraction because LMDZ happens to show values similar to those of GOSAT and TES.”

P4466L22: From Figure 2h, it is hard to find poleward and eastward trends.

We agree that it’s difficult to see poleward and eastward gradient on a map. This is why we have plotted transects on figures 3 and 4. Now we write: “For a more quantitative evaluation, Fig. 3 and Fig. 4 show North-south and West-east transects around Kourovka and are described below”.

P4467L10: Define "boundary layer".

Now we add: “800 hPa corresponds approximately to the top of the boundary layer.”

P4467L20: What is the reason of the d-excess noise in observation? What is noise? It seems that the model simulation is too smooth.

Yes. Now we explain the possible reasons for the noisier aspect of the data: “This could be due to the large uncertainty in the d-excess measurement. The extent of post-sampling evaporation effects are difficult to quantify, but they could reach several ‰ ([Kurita et al., 2004]), which is of the same order of magnitude as the North-South d-excess gradient simulated by LMDZ. The apparent data noise could also be due to the potentially large spatial heterogeneity of d-excess at the scale of a few kilometers: for example, the local surface type could affect d-excess ([Welp et al., 2012]). LMDZ cannot capture this heterogeneity. This could also explain why LMDZ looks smoother than the data.”

P4467L23: "LMDZ captures this d-excess trend." How can we know this?

Now we explain how we know this by referring to the figure and by giving quantitative values in the text: LMDZ captures the d-excess decrease with latitude, with a decrease from 14‰ at 35°N to 5‰ at 70°N (Fig. 3d).

P4467L26: What is "continental recycling"?

We have reworded this paragraph to better explain what is continental recycling and how it modulates the continental effect: "This is why the amount of continental recycling (i.e. the fraction of the precipitation water which is returned to the atmosphere through evapo-transpiration) is known to modulate the continental effect (i.e. the inland depletion of water vapor and precipitation) ([Salati et al., 1979, Kurita et al., 2004]). "

P4467L27: What is "continental recycling gradient"? Is it just zonal gradient?

Now we write: "East-West gradient in evapo-transpiration"

P4467L28: In P4465L22, d-excess eastward trend in the observation was mentioned.

OK, now we say everywhere that it's hard to detect a d-excess eastward trend in the observation.

P4468L3-4: Why ECHAM models should be suddenly mentioned here?

Because we think that [Butzin et al., 2014] deserves to be cited. Now we moved this sentence to section 4.3 on the multi-model comparison.

P4468L22: It's better to show more analyses for humidity. Here it is told that humidity is reasonable, but the motivation and main objective of this study was dry bias.

We agree. We have added precipitable water (to be consistent with total-column $\delta D$) on figures 2, 4, 5, 6 and 7. We have added a few sentences on the model-data comparison for $q$.

P4468L25: These statements are confusing. In conclusion of the paper, deltaD discrepancy is associated with humidity (and temperature). Also in Figure 12b, they say that modeled deltaD-q relationship is similar to the observation.

Figures 2-7 show the spatial and seasonal scales, whereas figure 12 shows the daily time scale.

P4469L3: In P4467L26-28, it was told that continental recycling was satisfactory.

- It was told that the East-West gradient in continental recycling was satisfactory. Here we discuss the latitudinal gradient. We have clarified this by breaking down this part into sub-sub-sections.
- We kept this sentence, but we added: "Sensitivity tests with ORCHIDEE will however suggest purely atmospheric processes are responsible for the latitudinal gradient mismatch (section 3.3.2)".

P4470L17: day -> dry

corrected

P4471L9: what is "a.g.l."?

expended: "above ground level"

P4471L25: Is the good agreement in deltaDp-deltaDv because of snow precipitation? Since there is isotopic exchange between raindrop and ambient air, there might be more impact in case of rain.

Yes, we added this: “This good agreement is probably due to the fact that most of the precipitation is snow. The isotopic composition of snow is easier to simulate than that of rain because it is less affected by post-condensational processes.”

P4471L25: How about snow amount simulation?

We have added a sub-figure comparing observed and simulated precipitation amount (now fig 11) and added in section 4.2: “Precipitation is snow, except during the first three days. Its amount is well captured by the model (Fig. **??**a).”

P4473L5: In section 3.2, only deltaD was evaluated. Where is "daily variations in q" from?

Now we evaluate in detail the precipitable water against TES and GOSAT in section 3. The daily $q$ variations are evaluated in section 4.1. We write in the introduction of section 5: “We showed that LMDZ reproduces well, at least qualitatively, the seasonal (sect. 3.2, Fig. 6) and daily (sect. 4.9, Fig. 9) variations in $q$ and water vapor $\delta D$ in the lower troposphere. ”.

P4474L23: "Since the boundary layer is most active in summer, we exclude. . ." I cannot understand the logic in this sentence. Elaborate.

Now we have clarified the rationale: section 5.1.1: “To qualitatively separate the effect of surface evaporation and BL processes, we use the fact that surface evaporation (which is a model output) and BL mixing (which is expected to be more active in summer, especially during the warmest days) have opposite effects. For example, if the “surface evaporation and BL processes” are more moistening when both surface evaporation and BL mixing are stronger, then we suggest that surface evaporation drives the moistening effect. In contrast, if the “surface evaporation and BL processes” are more moistening when both surface evaporation and BL mixing are weaker, then we suggest that the weaker BL mixing drives the moistening effect.“

Now the sentence in section 5.2.2 becomes easier to understand: “Since the boundary layer is expected to be most active in summer, we exclude an increase of boundary-layer mixing as an explanation for the dehydration. “

P4474L26: Why evaporation and BL processes cause "spikes"? Usually evaporation amount has less variability than precipitation amount, so that the impact would be more stable.

Evaporation and BL mixing show significant variability in the model at synoptic scales. We have replaced “spikes” by “large values”.

P4475L1: "This is consistent. . ." -> Since the model in Risi et al (2013) is the same, the result should be consistent.

We added this: “This consistency could be explained by the fact that the model is the same.”

P4475L6: "boundary-layer" -> "evaporation and boundary-layer"

OK

P4475L10: In P4464L23, it was told that boundary-layer was excluded in the season. But now it is told that BL process is larger than evaporation. Confusing.

We have clarified this in section 5.1.1 as explained above.

P4475L18 and some others : "boundary-layer" -> "evaporation and boundary-layer".

OK

Sometimes, the authors intentionally distinguish evaporation and BL processes, but they are not distinguishable in the model, right?

We have clarified this in section 5.1.1 as explained above.

P4479L12: What is the definition of "vapor origin"? It is vague expression.

Yes. We have reworded this expression at all occurences. For example, in the conclusion: “composition of the water vapor from different air masses that are being mixed”

P4480L24: Is "strongest enriched bias in deltaD" OK? It seems contradictory to Figure 12b.

Yes, we have corrected: “strongest dry bias on days when it simulates the most depleted $\delta D$.”

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