

Interactive comment on “The added value of water isotopic measurements for understanding model biases in simulating the water cycle over Western Siberia” by V. Gryazin et al.

Anonymous Referee #1

Received and published: 12 March 2014

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 questions, comments and technical corrections, though. The paper is recommended for publication in ACP after these minor revisions have been taken into account.

Specific Comments

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

p. 4461, lines 20-22: "no distinction is made between transpiration, bare soil evaporation, 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?

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.

p. 4464, Eq. 1: does the tilde symbol (\sim) in the last term mean "approximately proportional to"? Since the expression is only truly 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 (\sim) in Eq. 1 means and why it is used.

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

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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.

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 evapotranspiration 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)?

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.

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.

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

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

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well understood" does not seem to be justified?

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.

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).

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).

Technical Corrections

*** Abstract:

The abbreviations LMDZ, GCM, TES, GOSAT, GNIP, SNIP and SWING2 need to be

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defined separately in the abstract.

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 Eastern..."

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

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

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

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)

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*** 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))"

*** Sect. 4. Evaluation over Kourouka:

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.

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

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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).

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

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

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*** Tabela 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4457, 2014.

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