

## ***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 #2**

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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, focusing on the additional information by using water isotopic information. They used an isotope-incorporated AGCM compared with multiple isotopic datasets, including satellite 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

C638

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 technically convincing. However, title of this paper, “added value” by water isotopic information 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, especially 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 without 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.

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.

P4458 L19: the moist bias -> the dry bias

P4460L18: Spell out LMDZ

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 im-

C639

provement 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.

P4462L3: What is LMDZ"4"?

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

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

P4463L10: What is the time interval of precipitation sampling?

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

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

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

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

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

P4467L10: Define "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.

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

P4467L26: What is "continental recycling"?

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

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

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

P4468L22: It's better to show more analyses for humidity. Here it is told that humidity

C640

is reasonable, but the motivation and main objective of this study was dry bias.

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

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

P4470L17: day -> dry

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

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

P4471L25: How about snow amount simulation?

P4473L5: In section 3.2, only  $\Delta D$  was evaluated. Where is "daily variations in q" from?

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

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.

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

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

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.

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

C641

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

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

P4480L24: Is “strongest enriched bias in  $\delta D$ ” OK? It seems contradictory to Figure 12b.

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