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Geoscientific Model Development Discussions

Interactive comment on "From climatological to small scale applications: Simulating water isotopologues with ICON-ART-Iso (version 2.1)" *by* Johannes Eckstein et al.

Anonymous Referee #2

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This paper presents the new isotope-enabled model ICON-ART-Iso. It describes the main equations used for the fractionation processes and presents first evaluation results compared to various datasets and at different scales.As such, it deserves publication and it is well suited for GMD. For the evaluation, it makes use of some very recent observations, which is an additional strength of this paper.

My main regret is that he model-observations comparison could be more quantitative, and the physical processes responsible for the model-observations mismatches could be more discussed. But this is maybe not the priority for this first paper. Therefore, I recommend acceptation with minor revisions.

Detailed comments

- p2 l 1: isotope-enabled
- p2 l 19: it's awkward here to contrast "climatological questions" and "process understanding". Climatological questions can be answered through process understanding. In addition, limited area models are not the only tool to understand processes, global models can also be used for this purpose. Reformulate, by highlighting rather the differences in spatial scales or in convective representation.
- p 3 l1: "precipitation diagnostics" is mysterious here -> precipitation source regions?
- p 3 l 19: "climate prediction" -> "climate projections? It's impossible to predict climate for the end of the century. In case you refer to studies at the decadal scale, climate predictability is more appropriate.
- p 4 l 15: guarantee
- p4 l 17: syntax problem
- p 5 l 21: no, Risi et al 2010 and Werner et al 2011 did not use such a simple assumption. In stand-alone mode, both LMDZ and ECHAM models use a bucket model that collects the precipitation to represent the soil reservoir. To my knowledge, you are the first to make such a simple assumption. It's not a problem but it should be mentionned.
- p 8 l 5: for the liquid fraction, does it mean that you assume that all drops are sufficiently small to equilibrate totally? If so, it's not a problem but it should be mentionned that this is a simplifying assumption compared to offline models of rain-vapor exchanges in saturated environment (e.g. Stewart (1975); Lee and

Fung (2008) calculate the equilibration as a function of the drop size) or to GCM parameterizations (e.g. Hoffmann et al. (1998) assume that only a proportion of the drops equilibrate in a saturated environment depending on the precipitation type).

- p 8 l 30: how do you initialize water vapor composition at the model bottom and top? Interpolation needs these end members in addition to the tropopause values.
- p 9 l 9-14: why is this paragraph here and not in the Methods section?
- p 10 l 12: remove one “almost completely”
- p 10, section 3.1: previous studies using water tagging in models or water tracking tools should be cited and their results could be briefly compared to yours: e.g. Joussaume et al. (1984); Koster et al. (1986); Numaguti (1999); Yoshimura et al. (2004); van der Ent et al. (2010); Gimeno et al. (2012); Risi et al. (2013)
- p 18 l 13-18: but this does not improve the model-observations agreement...
- p 18: can the lack of daily cycle be related to the wrong precipitation daily cycle, a known problem in many models (Betts and Jakob (2002); Guichard et al. (2004))?
- p 20 l 9: how is this “sample” chosen?
- p 21 l 22: what explains the δD difference between these 2 parameterizations? Explain with simple physics what process is the main driver of this change. In addition, I understand that these 2 parameterizations differ for the representation of isotopic processes during rain evaporation, which occurs in the lower troposphere. Why does it have such a big impact in the upper troposphere?
- p 22 fig 7. A few explanations on these distributions could be useful. For example, do you see the signature of condensate lofting/detrainement? In the upper

tropospere, these processes are known to have a big impact on the isotopic composition (Moyer et al. (1996); Kuang et al. (2003); Bony et al. (2008); Sayres et al. (2010)). This could be discussed.

- p 23 l 9: this is very vague: how can different processes be identified from a scatter? Be more precise.
- p 23 l 10: remove “of”
- Overall section 3.4.2 and previous sections: be more quantitative when describing the model-observations agreement. Use quantitative metrics such as RMS error. This would allow to compare quantatively the model-observations agreement between different model version, different regions and seasons, different sampling criteria... It’s difficult to assess the model-observations agreement by comparing by eye 2 different plots.
- p 23 l 5-8: is the model-observations agreement better when removing these parcels with high proportion of initialization tracer?
- p 24 22: “discuss” -> show: you don’t discuss the reasons for these differences.
- p 24 l 32: “instances” -> use a more appropriate word?

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