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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Kinetic mass-transfer calculation of water isotope fractionation due to cloud microphysics in a regional meteorological model" *by* I-Chun Tsai et al.

Anonymous Referee #1

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Summary Tsai et al. have made a case study of kinetic isotope effects in clouds during a precipitation event over Taiwan. The authors have implemented two isotopologues of water in the hydrological cycle of a regional model for this purpose. The study uses a growth model for the distribution of the droplet size in the cloud scheme. The conclusion is that kinetic effects have a significant effect on the isotope composition, and including these effects yields a result closer to observations.

Major Comments More studies of cloud process using high resolution models are most certainly needed to further our understanding, both of the cloud processes themselves, as well as for understanding the isotope fractionation processes. While this study at-

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Discussion paper

tempts to do these things I think that the manuscript in the current form only got the authors half way there.

1) I think the premises for this manuscript are not correct, or at least very imprecise. As I outline in the comments below (e.g. comment for L62-64) the authors use a 20 year old paper to motivate their study and generalize the research area. Quite some major studies have been published in the meantime. While the results by Tsai et al. might be correct and their model well functioning, the reader has little chance to know what is actually new in this study. Furthermore, it appears to me that the manuscript in it's present form might lead the reader to think that the approach of the study is more novel than it actually is (comments to L62-64 and L128). 2) Basic analysis of the relationship between precipitation and dD is missing. As I understand it, this isotope enabled version of the WRF model has not previously been published. For a new model I would expect more extensive validation. At least for the course-resolution model simulation. For the larger domain, much more data are available for a general evaluation. Then the detailed case study comes after, if the model shows reasonable performance. The authors mentions the amount effect once in the introduction never to return to it, neither in the analysis nor in the discussion. I suggest Kurita (2013) and Zwart et al. (2018) as a starting point. I also suggest a few more plots (see last two comments for Figures) that would be very helpful for the analysis and for the reader to have some fundamental understanding of the performance of the model. 3) When it comes to the writing, formulations are often not precise enough when describing specific processes (e.g. comments to L62-64, L86, L88, L128, L177-178, L259-260, L332-334). 4) The quality of plots and labels make it difficult for the reader to see the point the authors are trying to make. See comments for Figures.

Given these major comments and the specific comments below I cannot recommend this study for publication in its current form. I think it would be better to resubmit after recasting the study. I hope that the authors will take the time to do this, and maybe an updated study could be an interesting contribution to the topic of water isotopes in the

climate system.

Specific comments L18 Insert “obtain” after “to”.

L22 The authors are careful to use the term “isotopocule”, but the title says “water isotope”. The colloquial, but technically incorrect, expression “water isotopes” is widely used. Whatever the authors choose, please be consistent in terminology. Generally, “isotopologue” is more widely used for water isotopes than “isotopocule”, since they for water mean the same thing. A less heavy, but still correct, term would be “water stable isotopes”.

L31 Replace “often” with “generally” and replace “variation” with “variations”.

L32 Replace “location” with “space”.

L36-43 It would be helpful to the reader to add specific references for specific processes discussed.

L62-64 Please be more precise when describing the what you mean by “partial or full equilibrium state”. In Hoffmann et al. (1998) kinetic fractionation is taken into account during evaporation and snow formation, as well as partial evaporation of raindrops when the air is undersaturated. Kinetic isotope effects have been investigated in a number of GCM studies, here among: Schmidt et al. (2005), Risi et al. (2010), Werner et al. (2011) and Nusbaumer et al. (2017). Given the motivation and focus of the study, the authors owe it to the reader to highlight the existing literature and provide a precise description to elucidate what is novel in this study. Could it be that the authors are also not familiar with Yoshimura et al. (2010)? I would surely expect such a paper to be cited for this topic.

L72 Replace “Duterium” with “Deuterium”?

L72-74 Please reformulate. This means you only incorporate HDO and H216O? And if so, why? Kinetic effects have strong impact on the deuterium excess.

L86 With “saturation adjustment” do you refer to parameterizations of supersaturation? E.g. where supersaturation is a linear function of temperature below a threshold? Please provide an example and reference.

L88 “according to kinetic mass transfer principles” so this is not a parameterization, but explicit physics?

L98 I assume H2O means H216O? Please specify.

L119-122 Have you tested the sensitivity to these formulations? In most models the formulation for ice/vapor by Merlivat and Nief (1967) is still used.

L128 As also noted by Robinson and Scott (1981) this equation is derived using similar arguments as Fick’s law, and essentially says that the flux is proportional to the gradient of the concentration. The formulation of kinetic fractionation for bulk vapor is derived using Fick’s law (e.g. Merlivat and Jouzel, 1984), and thus very similar to what is done in Eq. 5. My point being, that the real difference from your study to other isotope model studies is not that you take kinetic fractionation into account and they don’t, because they actually do (see comment to L62-64). The difference is in the widely used bulkwater formulation, while you use a growth model for the distribution of the droplet size, and formulate the isotope fractionation accordingly (following existing principles). Giving the reader something to hold on to in comparing your work to previous work (e.g. Eq. 5 following similar principles as Fick’s law, which other models base their kinetic isotope scheme on) is important to convey where your study is placed compared to other studies.

L145-147 Is this the motivation for the text L72-74?

L145 In Fig. 2, can you provide a comparison with liquid/solid phase vapor pressure using a the more common parameterizations found in GCMs?

L162 What about spin up of simulations? Longer spin up is usually required for isotope simulations.

L177-178 How is the nudging done? Spectral nudging or?

L197-215 So the observed d18O of sea water (e.g. LeGrande and Schmidt, 2006) is not used as lower boundary conditions, and your vapor isotope boundary conditions are derived from mean conditions? Sturm et al. (2005) showed that regional simulations are very sensitive to isotope boundary conditions, obtaining the best results with a nudged global model run as boundary conditions. Please provide some arguments how you can use this type of boundary conditions for a case study. For example, if the variability generally is small (please quantify) then it could be argued that, an observed mean "climatology" is representative. I am aware that you partly test the boundary conditions with the "NoLnd" run, but this is not what I'm asking for.

L234-236 That the model captures the location front is no surprise due to the nudging. Or is the nudging not constraining the model very tightly?

L259-260 You need to explain the continental effect somewhere, as you use this argument several times. This could for example be done in the introduction where the classic isotope effects are only touched upon in very general fashion.

L267 Replace "mechanimss" with "mechanisms".

L285-287 Please reformulate. Maybe simply replace "discrepancy" with "biases". Also, hasn't several studies already shown that this is generally true (e.g. Risi et al., 2010)? Does your study have smaller biases than other studies?

L288-291 This is one explanation, which sounds like an instantaneous process no matter the history of the air parcel. What about the progressive rain out of heavy isotopologues during adiabatic ascend?

L295 Replace "." with white space?

L297 Replace "heavier" with "heavy".

L332-334 Do you mean that there is a local marine source at the onset of the precip-

itation? In this context what does microphysical processes? I don't follow what the authors mean in this paragraph. Please rephrase.

L340 Replace "mid night" with "midnight".

Figures: * Please add axis labels and units to all graphs. E.g. Figure 4b has neither axis labels nor units, also no mention of units in the caption. * Labels and axis on Figure 9 and 10 are next to impossible to read. Please provide readable plots. * It is very difficult to see the differences between the curves (especially Figure 7 and 11) if you want to assess the discrepancy in time between the different curves. I suggest i) place subplots in upper and lower panels instead of side-by-side to "stretch" time ii) add axis grid in plot iii) make more readable axis labels that don't line up so close, for example by rotating them 45 degrees, and make it clear what is date and what is time. * It would be extremely helpful to plot dD and precipitation rates in the same plot to see if the timing of changes in dD and precipitation amount is similar in model and observations. * I suggest that the authors also add plots of dD vs precipitation and compare the model to observation and assess the classic amount effect. This is featured in many studies of isotopes in precipitation at low latitudes, and would give a way to compare to other studies.

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