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Climate of the Past Discussions

Interactive comment on "What controls the spatio-temporal distribution of D-excess and ^{17}O-excess in precipitation? A general circulation model study" *by* C. Risi et al.

Anonymous Referee #1

Received and published: 30 December 2012

General:

The paper lists and discusses the main controlling mechanisms of water stable isotopes (d18O) as well as the second-order parameters D-excess and 17O-excess. In contrast to the conventional water stable isotope parameters (d18O and dD) that have been studied since long for temperature reconstructions of ice cores, the second-order effects as observed through D-excess and 17O-excess are less deeply studied so far. This is particularly true for 17O-excess, since it is a parameter that only recently got attention through its potential to yield information about the relative humidity conditions at the moisture source region. There is hope by combining these primary and sec-

ondary water stable isotope parameters to better constrain past climate variations both at the site of precipitation as well as at the source of the original moisture.

However, as the authors point out already in the introduction, the main driving mechanisms for the secondary processes are not well understood yet in particular those for the 17O-excess, this is in part due to the still very sparsely available data. In addition the mechanisms are complex and rely often on empirical parameters such as the supersaturation. Related to this is the main concern of the reviewer: How valuable is such a model-data comparison when major driving mechanisms such as supersaturation as well as the surrounding relative humidity are not well understood. Both parameters are fitting parameters which are poorly constrained by measurements.

Being aware of these shortcomings, it is very ambitious to discuss Present-day (PD) to Last Glacial Maximum (LGM) conditions. The results depend strongly on the chosen value for supersaturation that may have changed between these two climate states.

The rather poor agreement of the model-data comparison highlights the importance of additional measurements of the second-order water stable isotope parameters. Both in-situ water vapour measurements as well as laboratory experiments are highly recommended in order to improve the understanding of the driving mechanisms of those parameters. In this regard the new laser-based techniques are extremely helpful for in-situ isotope measurements of water vapour. Yet, it is not possible – at least so far – to determine 17O-excess with this technology. In the present manuscript the reviewer misses a strong statement that such measurements are urgently required.

The paper is well structured and well written. However, it is only of interest to a specific community, mainly due to its dependences on empirical parameters that were incorporated as tuning factors in the model. Nevertheless, since it is the first approach of modelling 17O-excess using a General Circulation Model, I recommend publishing it with a focus for calling for new experiments mainly related to the limiting processes, i.e. supersaturation as well as re-evaporation and diffusive exchanges during rain falls.

Detailed comments:

Abstract, third paragraph: The explanation of the balancing effects of distillation, transport and air mixing should be improved. It was rather difficult to follow unless further reading of the main text.

Intro, l 13: It might be worthwhile to reference Sodemann's work as well in this regard
Intro, l 20: Uemura et al., 2010 should be listed here as well as reference

P 5496, l 2: Indeed stratospheric intrusions have not been discussed throughout the paper and they are also not mentioned in the conclusions to be at least part of the PD-LGM differences. It would be worthwhile to add a sentence on this issue.

P 5496, l 19: . . .its difficulty to simulate some aspects of . . .., be more precise here, what kind of aspects are not well captured by GCM models.

P 5497, l 4-10: this might be skipped.

P 5497, l 18: a statistical could scheme, it would be worthwhile to do some sensitivity tests with this approach regarding implications on water stable isotopes including second-order parameters. Has this been done?

P 5498, l 5: A reference is required for your assumption that over land all evapotranspiration occurs as non-fractionating transpiration.

P 5498, l 20: What is the difference, be more explicit. It is interesting to note that water amount of the total column of a falling rain drop (from the cloud to the surface) is by far larger than the water in the rain drop. Therefore, exchange of the water molecules are expected during the fall. However, this has implications on the condensation temperature, which temperature has to be used? It might be significantly different than the cloud temperature where the first condensation occurs.

P 5498, l 23: . . ..with hddft being the relative. . .

P 5500: It would be useful to compare results with the different forcing data-sets, what

implications would you expect for the results? Percent range or much stronger? It would be worthwhile to comment on this issue.

P 5500, l 20: How was this regridding done? Just a linear approach?

P 5500, l 25ff: Have you also used a delay factor? It might be possible that there Is an time offset.

P 5501, l 13ff: Until the calibration issue is not fully understood, it should be handled with care, even for small ïĄď18O variations.

P 5502, l 6: You exclude sublimation as potential post-condensational effect why? The reviewer cannot follow the authors argumentation that for solid precipitation no post-depositional effects occur.

P 5502, l 15: delete constant

P 5503, l1: Assuming that all processes add up linearly. . .. . .this is a strong assumption that requires information, either a reference or a sensitivity study with two different model settings.

P 5504, l 28f: Underestimation of rhs and SST, could this underestimation be caused by an under representation of air mixing effects? This relates to the mismatch of the water vapour results which are in contrast to the precipitation results (disregarding the underestimation of the changes by the model). It would be helpful to give information about vertical gradients of water stable isotope parameters for future measurement campaigns what to expect or where such experiments should be performed.

P 5507, 3.3. It is puzzling that seasonalities agree well between data and model but the latitudinal gradients are underestimated by the model. Why is this? Has it to do with the tuning parameter phi?

P 5509, l. 26f: The reviewer could not verify the sentence that the sum of all these contributions make the total signal (black line). The sum of the 4 contributions does not match the black line in all figures 7-9. Please explain it.

P 5515, l 5-25: This is still very speculative. To sum up large effects of different signs without having good confidence in their magnitude is weak. Again this calls for laboratory experiments.

P 5516, l 0-15: Stratospheric intrusions should not be ruled out to add the at least part of the measured signals in 17O-excess.

P 5519: Acknowledgements should be bold and increased in size.

P 5533: The term mixing as mentioned in table 5 should be defined in the main text. What do the authors mean with mixing?

P 5538: Fig. 5: I found only 5 bins not 6.

Fig. 7-9: Single effects do not add up to give the black line. Check this.

Interactive comment on Clim. Past Discuss., 8, 5493, 2012.