Clim. Past Discuss., 9, C2336–C2339, 2013
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Interactive comment on "What controls deuterium excess in global precipitation?" *by* S. Pfahl and H. Sodemann

Anonymous Referee #2

Received and published: 19 October 2013

The manuscript "What controls deuterium excess in global precipitation" by Pfahl and Sodemann re-opens the scientific discussion on how to interpret the deuterium excess quantitatively. The deuterium excess (DXS in the following) is a second order quantity resulting from the analysis of the stable water isotopologues, i.e. it is the scaled difference between the Deuterium (HDO) and oxygen 18 (H218O). It is astonishing that this discussion now takes already about 30 years and it seems still not even clear what is the relative importance of the two main players affecting the DXS, relative humidity or sea surface temperatures at the oceanic source region. There is clearly an interest in making progress in this question since many paleo-climatic studies now routinely use the DXS. Therefore the subject is interesting for readers of CoP and the paper is in general suitable for CoP. However, I have numerous major concerns on the argumentation and even the methods used in this study. This might be based partially on

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misunderstandings about what the authors actually did. In the latter case the paper needs at least some rewriting to clarify these parts of the manuscript. I would consider this paper only as publishable if these major revision are made and these fundamental questions are satisfyingly answered. Major concerns: 1) As far as I understand the study computes DXS in the evaporative flux from the ocean based on linear relationships between DXS in water vapor and relative humidity. However, DXS_evap and DXS_vapour can be two very different things (in particular close to the continents or the sea ice borders). DXS in water vapor is always affected by a number of processes and parameters such as air mass movement, condensation, etc. on one hand and the evaporative flux on the other hand. If one takes the linear relationship of Figure 1 and re-enters this into equation 1 then DXS_Evap is not in agreement with DXS_Vapour. How important is this divergence? This is not just underlining the point made in the paper on local vs global closure. Fig.2 claims to present a detailed map of the DXS in evaporation. It should be checked if DXS_Evap and DXS_Vapor (from Fig1a) are consistent with each other or to what extent they aren't respectively. 2) The manuscript underlines the importance of using high-resolution sub-daily data sets (RH, SSTs etc.) weighted with latent heat fluxes. The final result (Fig 1b or even Fig 2a/b) of this computation however is very smooth. I wonder to what extent such detailed computation is really contributing and suggest to use instead monthly, zonally averaged ocean RH and SSTs instead. I think it is important to demonstrate that the complexity of the computation here is really needed and that for example 2*12 values per latitudinal band of 10 degrees is not as good as the results here to obtain Fig 1b. 3) The authors make the point that the correlation between DXS and temperature is very noisy (Fig.5). What happens actually with the dependence of the DXS on the wind regime (Merlivat and Jouzel, 1979). Since computations were made on such a high temporal scale the impact of wind on above ocean surface turbulence and finally the water isotopologues should be discernible. The measurements leading to Figure 1a are effectively all made in a smooth wind regime.

4) Several recent papers make the point on the importance of re-evaporation of falling

CPD

9, C2336–C2339, 2013

raindrops below the cloud base (e.g. Frankenberg, 2011). This has a major effect on the DXS in precipitation. Furthermore the relation of the equilibrium fractionation coefficients of Deuterium and oxygen 18 is far from being constant. The DXS in precipitation varies therefore even under equilibrium conditions due to changes of condensation temperatures. My feeling is that this has been completely been neglected here and was considered as a second order process. However when discussing Fig.2 a and b the paper stresses the good correspondence between source DXS and precipitation DXS in relatively some small regions as success for the presented model approach. However exactly on these smaller scales systematic influence of the mentioned second order processes are possible. Minor Points. There are in particular numerous problems with the structure of the paper. 1) Page 4747 L10 – 4748 L25 reads like a text book chapter on the DXS. The introduction should be closer to the actual scientific debate. 2) Page 4749 L22 – 4750 L15 belongs to the Data and method section. 3) The evaluation in Figure 1b is ok, but "striking" (page 4753 L8)? This is just a mass balance of the isotopes on near global scale. It would be in fact strikingly surprising if this comparison wouldn't work out. As I mentioned in point 2 above my suspicion is that a similar good result can be reproduced by using much coarser (temporally and spatially) input data. 4) The phase shift argument (Figure 4) is weak for the Northern hemisphere (in fact RH and SST seem equally related to the northern hemispheric DXS). Rephrase accordingly "is at odds with the timing" (page 4755 L 27). 5) Page 4757 L20-27. Might be the DXS seasonal cycle in ice cores versus SSTs needs a more careful consideration. Post depositional diffusion of both isotopes has been demonstrated to have some impact on the max/min position. The firn diffusion is temperature dependent and min/max of both isotopes are not only reduced (something that can be expected by a diffusion process) but also the phasing of the DXS is changed. (Johnsen, 2000). 6) Page 4758 L4. "affected by model errors". Be more specific. That something might be affected by model errors is hardly news. I guess the statistical model presented here might also have some uncertainties. 7) Page 4759 L 9ff. The tuning process of DXS in GCMs is certainly an important point. Several tuning processes for todays climate and

CPD
9, C2336–C2339, 2013

isotope observations with comparable outcome might give different results for paleo runs. However, once one decides on one specific tuning for the oversaturation why should that process be different during the last glacial maximum. At the end this tuning describes micro-physical processes of which physics should not be different during different time periods.

1. Johnsen, S., H. B. Clausen, K. M. Cuffey, G. Hoffmann, J. Schwander, and T. Creyts, 2000: Diffusion of stable isotopes in polar firn and ice: the isotope effect in firn diffusion. Physics of Ice Records, T. Hondoh, Ed., Hokkaido University Press, 121-140.

Interactive comment on Clim. Past Discuss., 9, 4745, 2013.