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Interactive comment on "What controls deuterium excess in global precipitation?" *by* S. Pfahl and H. Sodemann

J. Sjolte

jesper.sjolte@geol.lu.se

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Pfahl and Sodemann has developed a simple model for the hydrological second order parameter deuterium excess (d). They argue that it is relative humidity (RH) and not sea surface temperature (SST) which is the main control on variations in d. The model is based on an empirical relation between RH and d found in a collection of studies measuring the isotope ratio of near surface water vapor. The authors go on to extrapolate the RH-d relation using global reanalysis data of humidity. By comparing to the spatial and temporal climatology of d in GNIP and ice core data they conclude that the model explains the main variability found in these records. The authors then extend the consequences of their finding to reinterpret glacial, and glacial to interglacial changes in d.

Although I generally agree that it is overly optimistic to interpret d as a pure SST proxy, I have several concerns regarding the discussion paper by Pfahl and Sodemann. While their approach might prove useful for some applications, the authors draw very far reaching conclusions for glacial-interglacial climate changes based on a present day empirical relation. The validation of the model is questionable as it seems to fail in capturing several regional features of the observed d. In addition, the comparison of hemispherical mean modeled d to ice core data does not take into account (or discuss) dating uncertainties and uncertainties arising from post depositional effects in the ice core data. Specifically, I think the following points should be addressed.

P4753L17-P4754L13: The following should be noted for the GNIP data comparison: (Fig. 2a) The very high modeled d to the east of North America is absent in the GNIP data. Also, the GNIP data from Southern Greenland, Svalbard and Iceland (very long high-quality series from Reykjavik) shows lower values. To a less of a degree the model also seems to have a high bias off East Asia (Fig. 2a) and in the South Pacific (Fig. 2b). Only the discrepancies in the South Pacific are mentioned. I think this should be discussed when looking at the model biases.

P4756L23-26: As also mentioned by the authors, SST and RH covary in the study of Uemura et al. Therefore, I cannot follow the conclusion that RH is more important than SST. It might as well be the other way around.

P4757L25-27: The comparison of the model to NEEM data is questionable. The ice core data is interpolated to monthly values, while the dating error is minimum one season. To interpolate to monthly values the timing of the δ18O annual cycle is assumed to happen a certain time of year. It is not based on an absolute time scale. In Figure 2 of Steen-Larsen et al. (2011) the NEEM ice core δ18O peaks around Aug-Sept (by definition in Aug, but Sept is almost as high), while the measured temperature peaks in July. Within the dating uncertainties, the δ18O annual cycle could easily be shifted 1-2 months to peak at the same time as temperature. Also, modeled δ18O at the NEEM site from two climate models peak in June and July (Steen-Larsen et al. (2011), Fig-

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ure 12), which supports the idea that the NEEM δ18O annual cycle should be shifted to peak up to 2 months earlier. The timing of the annual cycle of d is based on the δ18O annual cycle and accordingly has the same uncertainties. However, the timing annual cycle of d is also affected by post depositional diffusion. From Steen-Larsen et al. (2011) (p. 8, first column):

"The raw d-excess seasonal cycles exhibit a 3 month lag [Johnsen et al., 1989], but the phase lag between δ18O and d-excess is affected by diffusion. The back-diffused deuterium excess is minimum around Jun–Jul and maximum in Dec–Jan–Feb, therefore showing a $\sim$4-5 month lag with respect to δ18O and closer to being in antiphase."

Back-diffusion is of course not without uncertainty, so based on this the peak in d should be somewhere 3-5 months after the peak in δ18O. It is likely that δ18O, like temperature, peaks in July, and d would then peak somewhere in September-November. I think these uncertainties should be considered for the comparison with the empirical model (Figure 4).

P4759L23-26: From Steffensen et al. (2008) (p. 681, second column): "The moisture-source evaporation conditions can change either because of a shift in atmospheric circulation, resulting in relocation of the moisture source, or because of changing sea surface temperature, humidity, or wind conditions at a stationary moisture source". Although only the estimated 2-4K change of source temperature is quantified, Steffensen et al. leave room for other factors in their interpretation. When citing, it would be fair to include that Steffensen et al. are in fact not interpreting d as pure SST proxy.

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