Response to comments of Anonymous Reviewer #2

We like to thank the Referee for her/his comments, which help us to clarify our manuscript. Below, detailed responses to all comments are given.

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.

It is correct that we use observation of d in water vapour as a proxy for d in evaporation. With regard to the ship-based data sets of Gat et al. (2003) and Uemura et al. (2008), we assume vapour over the ocean to be directly associated with local evaporation; for the station data used in Pfahl and Wernli (2008), evaporation d has been reconstructed with a trajectory method, explicitly taking moisture advection into account. The very good agreement between the three data sets strongly supports our assumption. Moreover, the high correlation of d with locally measured RH from Uemura et al. (2008) also indicates that advection of air masses with different d signature has not been very important at such a low measurement height. Nevertheless, this assumption may be a potential source of error, and we will mention this more clearly in the revised manuscript. Figs. 1 and 2 are consistent by construction. Independent observations of d in evaporation would be required to further verify our approach (e.g., from eddy covariance measurements), which are not available. Current laser spectrometers are not yet able to achieve a sufficiently high temporal resolution for doing such eddy covariance measurements of water isotopes. We would be happy to have direct observations of d in evaporation to better constrain our model, but given that such data are not available, using measurements in vapour as a proxy

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is the best alternative we can think of.

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.

We would like to emphasize that the high time resolution is not the main point of our study, but the fact that RH is sufficient to explain the main features of the large scale d variability (see also our replies to points 3, 4 and 7). We have tested our model with monthly instead of six-hourly input data (at each grid point). As shown in the figure below, this leads to a reduction of the predicted d values compared to the six-hourly input data (cf. Fig. 1b). This reduction is due to the episodic nature of strong oceanic evaporation (evaporation is particularly large in periods with low RH, corresponding to high d), which is neglected using monthly data. The fact that the match with the precipitation observations is not worse than with 6-hourly data is due to a compensation of errors: the statistical model has a small positive bias, which is compensated by the systematical underestimation of d when using monthly data. If in addition RH is averaged zonally, this leads to a further smoothing of RH peaks and and a corresponding reduction in d (not shown). In summary, since using monthly data *systematically* underestimates d (due to the anti-correlation of RH and evaporation), we think that the six-hourly time resolution is appropriate for our approach.
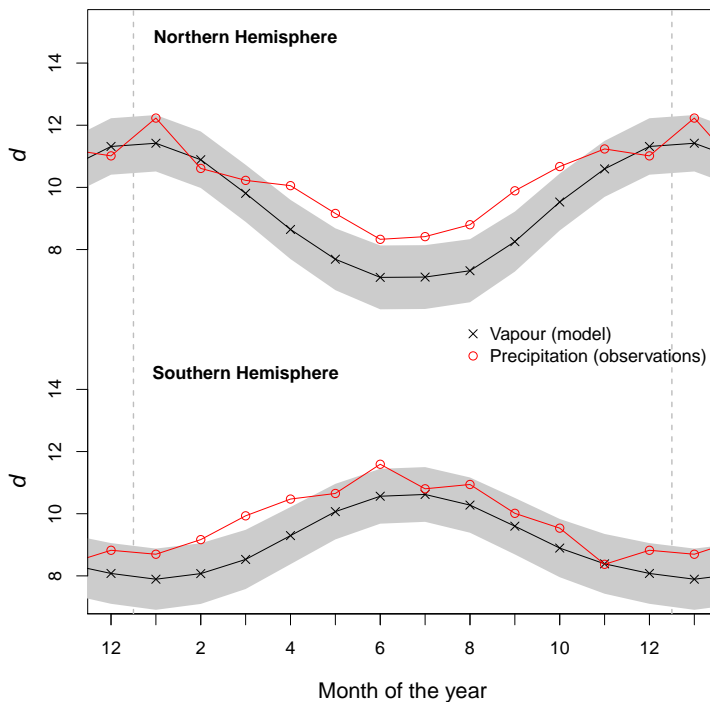
Figure 1: Seasonal cycle of d in ocean evaporation (black crosses and gray shading), as obtained from the linear regression model based on reanalysis RH with a monthly time resolution, averaged over the Northern and Southern Hemisphere. Red circles show hemispherically averaged d of precipitation from GNIP stations.

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.

The strategy of our modeling approach is just to take RH as the only input variable in order to show that it explains the first order d variability. There is also an influence of wind speed, but it is clearly second order (even more clearly than for SST). We will add a short note to the revised manuscript mentioning that mismatches between modeled and observed d may also be

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partly due to the influence of wind speed that is not taken into account. Note that the data used in Pfahl and Wernli (2008) partly reflect higher wind speeds at the evaporation source (rough regime), but the influence of wind speed on the d measurements is not discernible. This is discussed in detail in Pfahl and Wernli (2009). Uemura et al. (2008) did not measure wind speed, the corresponding wind regime is thus unknown, and Gat et al. (2003) did not report their wind data.

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

As noted above, our model is designed to represent the first order d variability through its relationship with moisture source RH and does, by design and intention, not take into account any other processes. In this way, it can be shown that RH is indeed the most important variable for such large scale d variations (e.g., the hemispherically averaged d in precipitation as shown in Fig. 1b). Nevertheless we discuss the potential influence of second order processes as those mentioned by the Reviewer on regional scales in section 3.1 (P4754 L14 - P4755 L4 in the original manuscript). As already stated there, these can become very important when examining individual stations, but cancel out in a hemispheric view. We will expand this discussion in the revised manuscript, also mentioning discrepancies between model and observations at specific stations (see also our reply to comment 2 by Jesper Sjolte).

5. Page 4747 L10 - 4748 L25 reads like a text book chapter on the DXS. The introduction should be closer to the actual scientific debate.

As the control factors of d are a long-standing matter of debate (as also noted by the Reviewer in his introduction paragraph), in our opinion a thorough introduction of this topic with references also to some older papers

is required. Nevertheless, we think that also the actual scientific debate is properly reflected in our introduction, and we are not aware of important recent references that are missing.

6. Page 4749 L22 – 4750 L15 belongs to the Data and method section.
We do not agree with this statement. The first part of this section and the corresponding figure give a synopsis of observational results from other recent papers that provide the basis for our study. In the second part, we outline our approach for giving the reader an idea of what he can expect, and we emphasize differences to other methods that have been reviewed in the preceding paragraphs. In our opinion this fits well within the introduction.

7. 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.
The striking aspect here is not the fact that the mass balance is fulfilled on hemispheric scales, but that (flux-weighted) RH is used as the *only* input parameter of the model (this will be made clearer in the revised manuscript), and despite this simplicity a very good overall quantitative match of amplitude, seasonal timing and annual mean values (see our reply to point 4 of M. Werner) is obtained. In this way, it is shown that RH at the moisture source explains seasonal variations of d in precipitation on hemispheric scales, and this is in fact the main point of our study.

8. 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).
Assuming a positive correlation of SST and d, as done in all palaeoclimatic studies we are aware of, the seasonal cycles of the two variables are in antiphase (according to this positive correlation, one would expect d to peak in late summer and be at its minimum in winter, but it is the other way around!). We thus think that 'at odds with the timing' is the correct term.

9. 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.

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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).

It is correct that the seasonal timing of d from ice cores is not easily determined due to diffusion effects in the ice (see also our replies to J. Sjolte). We do not intend to make a comprehensive comparison of our model results with ice core data (a much more detailed investigation of the moisture sources at the ice core sites would be required for this). We only want to make the point that from the ice core data we show here, there is no evidence that the seasonal cycle of d would correspond better to source SST than to RH (which has been used as an argument for the potential relevance of SST in earlier modeling studies). See our reply to comment 4 by J. Sjolte for further discussions.

10. 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.

The wording will be slightly adapted: '... and such a conclusion may be affected by model errors, **as** isotope-enabled GCMs still have difficulties to properly represent the spatial and temporal variability of d' The reasons for such errors are unclear, and it is beyond the scope of this paper to speculate about them.

11. Page 4759 L 9ff. The tuning process of DXS in GCMs is certainly an important point. Several tuning processes for todays climate and 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.

The tuning indeed does not explicitly describe the microphysical processes leading to supersaturation (which are complex and not completely understood). The parameterisation simply represents the supersaturation as a linear function of temperature. In reality, many processes may actually determine the supersaturation of a given air parcel, e.g., the cooling rates it has experienced and the availability of ice nuclei. Changes in these processes during the glacial maximum are not taken into account by the simple linear scaling with temperature.