Response to comments of Jesper Sjolte

We like to thank Jesper Sjolte for his comments, which help us to clarify our manuscript. Below, detailed responses to all comments are given.

1. 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.
We do not think that our conclusions are drawing too far. First of all, we do not claim that our model is able to fully explain glacial-interglacial variations of d in ice cores. We just show that present day d variability is, to first order, related to moisture source RH, and that there are no convincing arguments that justify to neglect the effects of RH when considering long term variability. With this, we would like to initiate a discussion and further, more detailed studies on the interpretation of ice core data (see, e.g., P4759 L18-21). Furthermore, using present day empirical relations for the interpretation of palaeoclimate proxies is a common practice (e.g., with respect to the classical temperature effect, see the discussion on P4759), and had also been pursued originally to argue for the interpretation of ice core d as an SST proxy. We think that the main value of our approach is that it reveals the most important processes determining d changes. Since present day d variability is to first order driven by moisture source RH, we argue that this should also be the starting point for the interpretation of palaeo data. Regarding the validation of the model and the uncertainties of ice core data, see our replies to points 2 and 4.

2. 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.*

When comparing modeled d in evaporation with measurements in precipitation, such as done in our Fig. 2, it is very important to keep in mind that the two quantities are not expected to correspond to one another perfectly, and that each station obtains moisture from various different oceanic sources (we will add another note on this to section 3.1). For instance, precipitation at the island stations in the North Atlantic is a combination of moisture from the western North Atlantic with very high d and from more tropical sources with lower d. This typically leads to less extreme d in precipitation compared to evaporation. With respect to the stations at Iceland and Southern Greenland, there may be local, secondary effects causing the relatively low d values in winter (see the discussion of such effects on page 4754; we will explicitly mention these stations in the revised manuscript). The discrepancies may also be due to biases in the measurements, the potential of which should not be neglected due to the (relatively long) one-month sampling time of the GNIP data during which evaporation effects may occur (this will also be noted in the revised paper). Nevertheless, the seasonal cycle at these stations is still consistent with the seasonal cycle of RH (higher d values in winter than in summer).

3. 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.

Our conclusion that RH is much more important than SST are not based on the study of Uemura et al., but on our empirical model results. On P4756, we just argue that the correlation between d and SST observed by Uemura et al. can be fully explained by cross-correlation effects and thus does not serve as an *independent* confirmation of a potential effect of SST (P4756 L26). In general, the purpose of section 3.2 is not to provide additional evidence for the importance of RH, but rather to critically evaluate existing arguments for the association of d with SST. We will add another sentence clearly expressing this purpose to the new manuscript.

4. 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 $\delta 18O$ 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), Figure 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 JunJul and maximum in DecJanFeb, therefore showing a ∼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).

It is true that a comparison to ice core data is difficult, and this aspect of our paper is intended as an indication that there is no contradiction between published records of ice core d and our proposed interpretation. It is certainly not meant to be a comprehensive comparison of our model results with ice core data, as a much more detailed investigation of the moisture sources for the ice core sites would be required for this. We merely want to make the point that from published ice core data, taking them at face value, there is no evidence that the seasonal cycle of d would correspond better to a signal from source SST than source RH. This clearly contradicts earlier arguments for the potential relevance of SST. Even with the additional dating uncertainties mentioned in the comment, this statement holds true. Ultimately, one would want to directly examine time series from snow samples, such as published by Fujita and Abe (2006) for Dome Fuji to exclude the problems of post-deposition effects. While a detailed examination of these aspects is clearly beyond the scope of this paper, we will extend the discussion of the comparison with ice core data to reflect the aspects brought up here.

5. P4759L23-26: From Steffensen et al. (2008) (p. 681, second column): "The moisturesource 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.

This is correct and we will adapt the wording in the revised manuscript to a statement that takes this into account.

References:

Fujita, Koji, and Osamu Abe. 2006. Stable Isotopes in Daily Precipitation at Dome Fuji, East Antarctica. Geophysical Research Letters 18 (33): L18503. doi:10.1029/2006GL026936.