

Response to comments of Martin Werner

We like to thank Martin Werner for his constructive comments, which help us to improve our manuscript. Below, detailed responses to all comments are given.

1. p. 4751, l. 21-22: How well does RH calculated from ERA-Interim data agree with the observed RH values shown in Fig. 1a? Please add a short comparison of modeled vs. observed RH.

In general, RH near the surface is reasonably well represented in ERA-Interim (e.g., Simmons et al., 2010; Pfahl and Niedermann, 2011). Of course such a reanalysis data set may suffer from certain errors compared to direct observations, but for the requirements of the present study (full spatial and temporal cover, also over data-sparse regions), it is the best available data source (a short comment will be added to the revised paper). The 4D-Var data assimilation system applied in ERA-Interim is, as we think, the most effective method for obtaining gridded near-surface RH time series with high temporal resolution from a variety of unevenly distributed observations (e.g., from ships). The figure below shows a comparison of the ship-board RH measurements from Gat et al. (2003) and Uemura et al. (2008) with co-located ERA-Interim RH data. In general, there is a good correspondence between the different data sets (correlation of 0.87). The reanalysis has a small positive bias of about 5%.

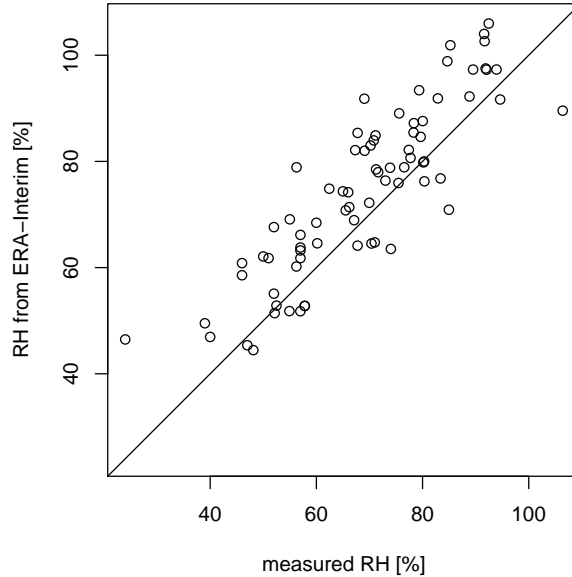


Figure 1: Comparison of measured RH (from Gat et al., 2003 and Uemura et al., 2008) with RH from ERA-Interim. ERA-Interim data have been sampled at the time and linearly interpolated to the location of the measurements. Since no exact time information have been available from Gat et al. (2003) ERA-Interim data at 12 UTC have been used.

2. p. 4751, l. 24-26: I do not understand this point about the exclusion of spin-up effects for latent heat flux. Please clarify these statements. Furthermore, why is the latent heat flux, but not the evaporation flux chosen for the necessary weighting calculations? Evaporation is also available within the ERA-Interim dataset and should correspond more closely to the precipitation-weighting of the GNIP data. Please explain this selection in more detail in a revised text.

Surface latent heat flux is directly proportional to surface evaporation (the latent heat of vaporisation is assumed to be constant in the ECMWF model), and thus weighting with latent heat flux is equivalent to weighting with evaporation (a note on this will be added to the revised manuscript). We routinely neglect the first six hours of a ERA-Interim forecast because of potential spin-up effects. This may be more important for precipitation than for latent heat flux, but for consistency reasons (and due to possible feedbacks between precipitation and evaporation), we think it is important to

keep this more conservative approach and use forecast steps between 6 and 18 hours. Most likely the differences to also using the first six hours are very small in most cases.

3. p. 4752, l. 22-23: *Can one really exclude moisture exchange between the two hemispheres on a seasonal time scale? E.g., in some regions the position of the ITCZ shifts between 20° N and 20° S during the year. Please discuss this assumption and the implications for your calculations in some more detail.*

Average moisture fluxes are mostly zonal in the tropics (e.g., Trenberth, 1999), which suggests that inter-hemispheric transport can be neglected. However, this is not always the case: The Indian summer monsoonal circulation is associated with substantial moisture inflow from the southern hemisphere. This is a potential error source in the comparison of hemispherically averaged model results and observations, which will be mentioned in the revised manuscript. We think that this does not lead to very large errors because the predicted spatial gradients of evaporation d are relatively small in the northern part of the Indian Ocean (see Fig. 2b).

4. p. 4753, 1st paragraph: *Please give some numbers on the global annual mean budgets. How well do means of d in precipitation and evaporation agree?*

The observed global mean d in precipitation is 10.0 permil, while the predicted global mean d in ocean evaporation is 10.8 permil. This will be mentioned in the revised paper.

5. p. 4753, l. 10-11: *Could recycling of continental water also have an effect on the seasonal d signal in precipitation?*

Yes, this is exactly what we refer to when citing the prolonged residence time. We will make this clearer in the revised manuscript.

6. p. 4753, l. 13-16: *The explanation of the negative bias in the SH due to sparse spatial coverage of GNIP stations is not very convincing. Apart from Europe, the spatial coverage of GNIP stations in the NH and SH seems to be rather similar (cf. Fig.2). One could further test this hypothesis by excluding some parts of the Southern Ocean for the calculation of the SH d values.*

Excluding the ocean south of 40 degrees S from the statistical model leads to a slight improvement, but this does not explain the full bias. We will mention two other error sources, namely the d measurements in precipitation and the

formulation of the linear model in the revised manuscript.

7. p. 4754, l. 6-7: *I can't see a clear east-west gradient of d in precipitation over Eurasia. Please clarify this statement.*

There is a large variability of d at the eastern Asian stations in winter, but nevertheless the average values are higher than over Europe: the DJF mean of all stations in south-eastern Asia between 100-140 degrees E and 20-40 degrees N is 12.2 permil, while it is only 9.4 permil for the European stations between 20 W - 20 E and 30-60 N.

8. p. 4754, l. 11-13: *How large is the modeled interannual variability of d in evaporation? Can this really explain the differences at the Pacific Island stations?*

The predicted interannual standard deviation of d in evaporation over the SH varies between about 0.5 and 2 permil and is thus on the same order as the hemispheric model bias. Mismatches at certain stations are larger and are most probably associated with local-scale (second order) processes. Nevertheless, as we discuss these processes in the following paragraph and have only stated that the differences may be due to interannual variability 'to some extent', we think that the discussion is sufficiently balanced.

9. p. 4756, l. 15-20: *I cannot follow the authors' argument why a possible change in d due to a shift in precipitation seasonality can be rather related to RH than SST changes. Please explain this argument in detail or omit it.*

In general, it is not the main purpose of this paragraph to provide new evidence for the relevance of RH also on palaeoclimatic time scales, but rather to critically revisit existing arguments for the association of long-term d changes with source temperature variations. This will be stated more clearly in the revised manuscript. In this line, we argue that the common argument 'glacial-interglacial changes in mean RH are small, so it must be SST' does not apply, because RH may still influence d via changes of the source regions or precipitation seasonality. We do not make a statement about effects of SST via a changed precipitation seasonality.

10. p. 4758, l. 1-17: *Again, I cannot follow the authors' argument. Recent isotope climate model studies may show substantial differences in simulated d values. But how can these model deviations allow us to drawing conclusions about the importance of SST and RH changes for the d signal in precipitation?*

Please explain.

We are not sure if we understand this comment correctly. Recent model studies (e.g., Lewis et al., 2013) have looked at long-term variations in moisture source conditions and the relation to d . They found some potential influence of SST. Here we just want to make the point that it may be dangerous to fully rely on such model results in the interpretation of d variations from proxy records and in particular for deciding on the relative importance of RH vs. SST because models still have problems in the representation of d . We think that a thorough model validation with respect to the present-day distribution and seasonal cycle of d (and its relation to moisture source RH) is required.

11. p. 4758, l. 18-20: I rate this statement as too single-sided and suggest using the same wording as in the abstract: "All together, there is no convincing evidence that RH might be less important for long-term palaeoclimatic d changes compared to moisture source temperature variations."

We will adapt our statement as follows: 'All together, we do not think that in the light of this study, there remains sufficient evidence that would justify to neglect the influence of RH on palaeoclimatic d variations. Either the interpretation of d variations in palaeo-records will have to be adapted to reflect climatic influences on RH during evaporation, or new arguments for an interpretation in terms of moisture source SST will have to be provided based on future research.'

References:

Simmons, A. J., K. M. Willett, P. D. Jones, P. W. Thorne, and D. P. Dee, 2010. Low-frequency variations in surface atmospheric humidity, temperature, and precipitation: Inferences from reanalysis and monthly gridded observational data sets, *J. Geophys. Res.* 115, D01110, doi:10.1029/2009JD012442.

Trenberth, K. E., 1999. Atmospheric moisture recycling: role of advection and local evaporation, *J. Climate* 12, 1368-1381.