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Climate of the Past Discussions

Interactive comment on "Glacial–interglacial shifts in global and regional precipitation δ^{18}O" *by* S. Jasechko et al.

Anonymous Referee #3

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Interactive comment on "Glacial - interglacial shifts in global and regional precipitation delta18O" by S. Jasechko et al.

General comments:

The central idea behind the work of Jasechko et al. was the reconstruction of climatically-induced shift in the isotopic composition of global precipitation between the last Glacial and the Holocene, and to confront it with the predictions of state-of-the-art isotope-enabled general circulation models. The authors selected three different proxies of isotopic composition of past precipitation (groundwater, speleothem calcite, ice cores), conducted extensive literature search and came up with the reconstructed delta18O of precipitation for two time windows: (i) late Holocene (0 - 5000 calendar years), and (ii) Glacial (19500 - 50000 calendar years) at number of sites distributed

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globally. The resulting spatial distribution of the reconstructed Delta18O(ice age) was then compared with the modeled Delta18O(ice age) generated by five GCMs. This sort of global comparison was in fact long due and represents a valuable tool for assessing the performance of existing isotope-enabled GCMs.

I highly appreciate the efforts and gigantic work done by the authors in compiling appropriate information. Still, when attempting this sort of comparison, a great care is needed in proper selection of the data in order to minimize the possibility of falling into "garbage in - garbage out" trap. Therefore, the proxy data selected for comparison should be carefully scrutinized. I do have a number of comments and suggestions which might assist the authors in improving the overall shape of the paper and which should be addressed in the revised version. They concern both the methodology used and the structure of the paper.

Specific comments:

1. I would recommend adding a separate section in Chapter 2, where key characteristics of the selected proxies, of direct relevance to the presented model-data comparison, are discussed in depth. In particular, this discussion should highlight the following questions: (i) how well the given archive is preserving the (mean) isotopic composition of precipitation ? (ii) what are the potential biases? (iii) can reliable chronology of the given archive be established?. Some information on those issues is dispersed throughout the main text and in the Supplement but the paper would definitely gain in clarity if all this information is gathered in one place. While the authors address to some extend the question of establishing age of groundwater (see comment no. 3), they refrain from any comment on the uncertainties of the chronology of speleothem and ice core samples used in the comparison, which can be significant (see e.g. Landais e al., (2015) for ice cores).

2. It should be made clear that in view of significant uncertainties in establishing absolute chronologies of the archives selected for this work, particularly for the Glacial

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period, the boundaries of the selected time windows remain blurred. This is particularly true for the Glacial time window. Setting up a sharp lower boundary at 19500 calendar years does not make much sense in this context (should be rather ∼19000 or ∼20000 calendar years).

3. I have several objections with respect to the approach adopted by the authors to calculate groundwater ages, as described in the Supplement.

(i) to calculate groundwater age the authors use eq.(S1) which numerical factor (-8267) contains more recent value for 14C half-life (5730 years). Then, they convert ages derived using eq.(S1) to calendar ages using corrections based on the calibration curve proposed by Fairbanks et al., (2005). However, by definition the calibration curve relates conventional radiocarbon ages to calendar ages (cf. Fig. 2 of Fairbanks et al., 2005). Conventional radiocarbon ages are calculated on the basis of Libby's half-life (5568 years) which leads to numerical factor in eq.(S1) equal -8033, but not -8267. Besides, a more recent calibration curve (Reiner et al., 2013) synthesizing all available calibration data should be used rather than Fairbanks et al (2005) curve.

(ii) Figures S4 - S62 have a horizontal axis labeled "Groundwater age (14C-years before present)". Are those indeed radiocarbon ages calculated on the basis of eq.(S1), or perhaps radiocarbon ages converted already to calendar ages ?. In any case, they contain number of data points showing unrealistically high finite ages going up to 62000 years.

(iii) In their uncertainty analysis the authors apparently forgot to include the analytical uncertainty associated with the measured radiocarbon content in the given sample (quantity A in eq.(S1)) Large majority of the reported radiocarbon data (my guess would be that this is around 80-90%) was obtained by laboratories using conventional (i.e. decay-based) analytical techniques. Typical analytical uncertainty of radiocarbon analyses in such laboratories (one sigma level) usually varies between ca. 0.5 and 1.0 pmc (percent of modern carbon). In addition, sampling of groundwater in the field for

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radiocarbon analyses introduces additional source of uncertainty (possible contamination with modern radiocarbon from the atmosphere). Therefore, a realistic value for the Limit of Detection (LoD) can be set in this case around 1 pmc. Modern AMS laboratories can do a bit better but still the problem of contamination during sampling remains open. This LoD of approximately 1 pmc leads to conventional radiocarbon age of ca. 35000 years, not accounting for any geochemical correction - just taking into account radioactive decay only. This age limit transfers to approximately 40000 calendar years. Consequently, all groundwater ages higher than ca. 35000 conventional radiocarbon years or ca. 40000 calendar years should not be reported as finite ages but rather as "> 35 ka BP or >40 cal. ka BP".

(iv) The authors use so-called Pearson correction model to account for the dissolution of carbonate phases in the aquifer (eq.(S2)). However, it is well-known since long time that this model does not describe correctly all possible interactions between the TDIC reservoir which is dated by the radiocarbon technique and the aquifer matrix. Often, more complex models which merge evolution of carbon isotopes with the evolution of water chemistry along the flowpaths need to be applied in order to obtain realistic groundwater ages. More advanced correction schemes may result in radiocarbon ages differing by several thousand years from the ages returned by application of the simple Pearson correction model.

Summarizing, the groundwater data need a major overhaul here.

4. In their compilation of groundwater data the authors do not refer to the IAEA database. This is by far the largest collection of isotope data for groundwater systems worldwide. Therefore, I would strongly recommend that three IAEA Atlases are consulted (Atlas of Isotope Hydrology - Africa, IAEA 2007; Atlas of Isotope Hydrology - Asia and the Pacific, IAEA 2008; Atlas of Isotope Hydrology - the Americas, IAEA 2009) and, if appropriate, additional data obtained from this source included in the global picture of reconstructed Delta18O(ice age) presented in the paper.

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5. In Chapter 3.3 the discussion of each region should be accompanied by appropriate regional maps showing the locations of relevant sites, each labeled by two numbers: reconstructed and simulated Delta18O(ice age). Colors should be avoided because they hide to some extent the real differences. Such regional maps would guide the discussion and would help to identify regions which are most problematic with respect to the model-data comparison pursued in the paper. Figure S3 should be then removed from the Supplement.

6. Figure S1 should be moved to the main text. Perhaps the authors may consider adding on the map presented in Fig. S1 the Holocene-Glacial noble gas recharge temperature differences reconstructed for number of aquifers which are included in the model-data comparison discussed in the text. Noble gas temperatures are considered excellent proxy of ground level air temperatures and it would be instructive to confront Annan and Hargreaves (2013) reconstructions with those derived from noble gas data. From this perspective, the 15 degree Celsius of temperature suppression for Hungary at the last glacial maximum, reported on page 848, line16, is clearly an exaggeration (noble gas data indicate only 8-9 degree Celsius - see e.g. Corcho Alvarado et al., 2011).

7. The conclusions should stress the fact that the compilation of reconstructed Delta18O(ice age) presented in the paper constitute a strong challenge for isotope-enabled GCMs. Figure 3 makes it clear that the selected crop of isotope-enabled GCMs is not performing particularly well. The frustrating thing is that apparently not much progress has been made in this respect in the past twenty years or so. Perhaps the necessary first step towards improving the situation would be a comprehensive model-data comparison with the present-day spatial distribution of mean delta18O(delta2H) in global precipitation.

Interactive comment on Clim. Past Discuss., 11, 831, 2015.