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Climate of the Past
Discussions

Interactive comment on "Comparison of the oxygen isotope signatures in speleothem records and iHadCM3 model simulations for the last millennium" *by* Janica Carmen Bühler et al.

Anonymous Referee #1

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Bühler et al. explore the temporal and spatial variability of speleothem d18O for the past1000 time frame (850-1850) using a global compilation of speleothem data and a 1000-year run with an isotope enabled climate model. The authors briefly investigate the relation of d18O to temperature and precipitation in the model, and compare the modelled temperature and precipitation to speleothem d18O. Next the authors explore the spatial relation between a number of variables such as latitude, annual mean temperature, precipitation, and the mean speleothem d18O. They go on to compare the spectrum of temporal variability in the model to the speleothem data. Finally they investigate the teleconnection patterns in speletothem data and in the model. The main conclusions are that i) high frequency variability is dampened in the speleothem data

due to the hydrological residence time before reaching the cave ii) modelled centennial variability is underestimated iii) teleconnections are hard to find in the speleothem data, while more easily identified in the model iv) low signal-to-noise ratio for the speleothem data due to local processes makes it difficult to interpret.

This study contains a lot of interesting and useful work to better understand speleothem and modelled d18O. However, I find some aspects missing that would motivate some of the things studied in the paper, and more analysis is needed to round off the study. Overall the paper is well-written with minor typographical issues. I hope that the authors will see my comments as a positive contribution, as I think the study has a lot of potential, but requires some more work. In summary, I recommend that the manuscript requires major revisions, but will then most certainly be a valued contribution to the topic.

Major comments. 1) Why even look at teleconnections in the d18O data (Figure 8)? There is no mention of ITCZ variability, monsoon, NAO, or other mechanisms driving large-scale d18O variability. I can understand if the authors want to keep the analysis general, but through the whole paper there is no mention of any of the main climate patterns that could explain the teleconnections in Figure 8. See references listed in comment for L78-L80. This should at the very least be mentioned in the introduction, and included in the discussion. There is a lack in information of how HadCM3 performs when it comes to large-scale patterns, and what the imprint is on d18O. For example, add extra correlation maps in Figure 7 for the most important patterns, which quickly could be done. Correlating the monsoon index (e.g. Vuille et al., 2005) to d18O in precipitation should show a very clear pattern across the region around the Indian Ocean. This is not obvious when looking at grid point correlation of climate fields, because the main driving factor is not local precipitation amount, but down wind recycling of vapour in large-scale organized convection. 2) The authors mention external forcing several times as a driver of variability, but never explains or does any analysis to show how this this is related to climate or d18O. This is of course a big topic (e.g. Swingedouw et al., 2017) and might be beyond the scope of the paper. Please either perform analysis of the impact of forcings or be more careful when making statements about what variability is forced and what is not forced. 3) The authors have three simulations but appear to make very little use of the additional information to be gained from this. While three simulations is not a huge ensemble it still yields much more information on forced versus internal variability than a single simulation. When you perform correlation analysis between speleothem data and simulated d18O, this should be done using the ensemble mean. How similar are the ensemble runs in variability? How is the ensemble setup? There is very little information on this. 4) The study uses a shot gun kind of approach to age-model uncertainties. As I understand the different age-models of individual speleothems are sampled independently when testing the range of possible age-models. But are all age-models really equally likely, for example for neighbouring speleothems that we expect to be correlated? Related to this. When comparing the down-sampled modelled d18O to speleothem data in Figure 8, shouldn't the age-model uncertainties also be included for the model data to make the results truly comparable? For completeness there should be two more tests plotted in Figure 8: i) model data which is not down sampled (include SF7 a) and b) in Figure 8, I suppose?) ii) model data including age-model uncertainties. I think this issue with the comparison of model and speleothem data and differences in teleconnections depending on data treatment should be more emphasized.

Detailed comments.

L5-L12 Briefly say that d18O is a climate proxy before discussing all the implications of sampling etc.

L15 "We evaluate systematically. . . " change to "We systematically evaluate . . . "?

L16 ". . . and test for the main climate drivers for individual records or regions." change to ". . . and test for the main climate drivers recorded in d18O for individual records or regions."?

L17-L19 Maybe it is be better (worse) to explain in full sentences (fancy truncated syntax)?

L28 ". . . natural and human systems . . . " maybe change to ". . . human societies and the environment . . . "?

L36 The delta-notation comes in here before defining it or telling what the proxy is good for. Either move the definition up in the manuscript and description of the d18O proxy or call it "the relative abundance of 18-O" until you get to the definition, and then explain briefly that this is a climate proxy. You can't discuss the challenges of the interpretation before telling the basics.

L41 You need to include the simulations with GISS ModelE2-R (Colose et al., 2016) and iCESM1 (Stevenson et al., 2019).

L42 Sjolte et al. (2018) compared the variability of the modelled ECHAM5/MPI-OM d18O to Greenland ice core d18O and used the model to assimilate the ice core data to produce gridded reconstructions. Never compare the proxy data to the model – it's the other way around!

L44 Again: Never compare the proxy data to the model! It's not the observations that are being evaluated.

L56-L61 These are a very important points and is written in almost bullet point-style. Please add more details to make it more comprehensible to non-experts. For example, Laepple and Huybers (2014a) are talking about decadal and longer time scales. Laepple and Huybers (2014b) say that the models are too diffusive which is not the same as saying "too high diffusivity", depending on context. Here, they mean that the energy dissipates too quickly across the spectra of temporal variability, which is not clear in your text. My advice is to spend a bit more space on this part of the introduction and don't mix topics, such as too diffusive models and missing processes and feedbacks in the same sentence, unless linking these things directly together.

L64 Add white space after “climate system”.

L78-L80 There is quite some evidence that d18O is not primarily a proxy of neither local temperature nor precipitation, but strongly related to circulation modes, large-scale climate patterns and downwind fractionation. For example, in the North Atlantic region the North Atlantic Oscillation (NAO) is important for d18O variability (Vinther et al., 2010; Sjolte et al., 2011; Deininger et al., 2016), while downwind fractionation connected with the Indian summer monsoon impacts the cave d18O in the region around the Northern Indian Ocean, China and South-East Asia (Vuille et al., 2005; Fleitmann et al., 2007; Pausata er al., 2011; Kurita et al., 2013; Lekshmy et al., 2014; Liu et al., 2014; Sjolte et al., 2014; Zhang and Jin, 2015). I think these factors should be highlighted in the introduction.

L91-L99 Here you mainly list the contents of the paper. Can you make the science questions that you are pursuing more clear? Maybe you are testing the climate controls on the variability in simulated d18O using an isotope enabled climate model and compare this to speleothem d18O in a global dataset? Formulate more like hypothesis testing rather than say what kind of analysis you are doing.

L108 Add white space “. . . 30min . . . “

L115 What is meant by “ice sheet” here? I suppose the model doesn’t have an ice sheet model?

L120 “. . . features like latitude effect, amount effect, or the continental effect . . . “ this is partly repetition from L118. Why not lump these things together?

L123 So, what are the differences between the three model runs? Initial state of the ocean?

Figure 2, caption. Add white space “600yr”.

L153 “600y” add white space, and I believe Clim Past uses “yr” shorthand for year.

L161-169 As I understand you allow any type of age model to be used out of the many options, and you pick the best fit independently for each site/speleothem? What are the criteria for accepting an age model, be sides that it is the best fit? Are there cases where the “best” age model is outside of the uncertainty range of the U/Th dating?

L176 How do you decide on the nine clusters? Is this what you describe L236-L239. Please clarify.

L180 “. . . 10 or more δ18O sampled.” should it be “. . . 10 or more δ18O samples.”? Otherwise please rephrase.

L181 “We exclude six speleothems of mixed mineralogy.” Why?

L228 If you chose the highest correlation out of a large ensemble of possible solutions, how do you account for this when determining the significance of the correlation?

L256-L257 If you calculate the regional lapse rate of 18O in the model you can estimate the contribution of the model orography to the d18O biases.

L265 Did you try doing multivarite regression? To know the influence on one parameter you need to isolate it from the other parameters.

L273 Add white space “both in the annual mean andfor ...”.

L277-L278 “To analyze . . .” please rewrite this sentence more concisely and remember, again, that you are comparing the model to the data.

L311 Add white space “3yr”. I see space missing many places before “yr”. Please check in general.

Figure 7, caption. Here, “insignificant” should be “non-significant”. I assume you use the term “significant” in a statistical sense?

Section 4.4 Did you look at the relation of d18O to climate modes? See comment above to L78-L80. For example, the monsoon index (Vuille et al., 2005) might have a stronger imprint on d18O in the tropical Indian Ocean than local precipitation amount.

L320 “and the climate variable is shown.” Change to “and the climate variable is also shown.”?

L325 What about the correlation of LM2 and LM3 to the proxy data? Using the model ensemble could give a clue if the variability is related to forcing.

L332 $p < 0.1$ is not a strong significance criterion. How many samples are there? And in the first place can we expect much correlation between a single model run and observed climate? Changing the initial conditions of the model run would likely affect these correlations, since this is just one realisation, no?

Figure 8: I found the choice of colours confusing in Figure 8d. The smoothed lines are red and blue in the same shade as the markers for the correlation, which made me think at first that the smoothed lines were for the data marked of similar colours, which doesn’t make sense. It’s quite a busy plot. Consider making it easier to read by choosing different colours or making an extra subplot.

L396-L398 “In general, . . . “ I don’t follow this sentence. Seems like a leap in topic. How can you say anything about forced variability without analysing it? Also, I believe Jungclaus et al. (2010) are discussing the hemispheric mean temperature, while the speleothem d18O data is temperature, precipitation, evaporation and circulation dependent.

L410 “However, we find little regional consistency . . . “ couldn’t this be due to time scale uncertainties? You find no structure in correlation for the speleothem data in Figure 8, but there could be a correlation/regional climate signal, just as well as there could be no correlation.

L428 “longer than50yr” Spaces!

L428 “by 4% (3, 4)” Upper confidence bounds same as median? Or is this due to the number of significant digits?

L434 “However, no systematic pattern and few significant correlations were found for the speleothem records (Fig. 7).” Again, I’m really not surprised that there is no correlation between a free running simulation and the proxy data. There might be forced common variability between model run and proxies (volcanic, solar), but then you need to check the model and proxy response to forcings.

L464 “We use a three member initial-condition ensemble from a single iGCM in this study.” Please describe the model ensemble initiation in the methods section.

L470 “ . . . as suggested by Dalaiden et al. (2020).” There are lots of examples of offline data assimilation. Maybe provide a few more? E.g., see references in introduction of Sjolte et al. (2020). Ice core data is synchronized using volcanic markers. Any particular age-model related uncertainties to take into account that might complicate the assimilation of speleothem data?

L483 “. . . such as δ13C cannot (yet) be implemented in GCMs . . . ” It’s not that far away (Scholze et al., 2008; Camino-Serrano et al., 2019).

L503 “. . . low signal-to-noise ratios . . . “ For the speleothem data?

Refrences

CaminoâĂŘSerrano, M., Tifafi, M., Balesdent, J., Hatté, C., Peñuelas, J., Cornu, S., & Guenet, B. (2019). Including stable carbon isotopes to evaluate the dynamics of soil carbon in the landâĂŘsurface model ORCHIDEE. Journal of Advances in Modeling Earth Systems, 11, 3650–3669. https://doi.org/10.1029/2018MS001392

Colose, C. M., LeGrande, A. N., and Vuille, M.: The influence of volcanic eruptions on the climate of tropical South America during the last millennium in an isotope-enabled general circulation model, Clim. Past, 12, 961–979, https://doi.org/10.5194/cp-12-961-2016, 2016.

Deininger, M., Werner, M., and McDermott, F.: North Atlantic Oscillation controls on oxygen and hydrogen isotope gradients in winter precipitation across Europe; implications for palaeoclimate studies, Clim. Past, 12, 2127–2143, https://doi.org/10.5194/cp-12-2127-2016, 2016.

Fleitmann, D., Burns, S.J., Mangini, A., Mudelsee, M., Kramers, J., Villa, I., Neff, U., Al-Subbary, A.A., Buettner, A., Hippler, D., Matter, A., 2007. Holocene ITCZ and Indian monsoon dynamics recorded in stalagmites from Oman and Yemen (Socotra). Quat. Sci. Rev. 26, 170e188.

Kurita, N. (2013), Water isotopic variability in response to mesoscale convective system over the tropical ocean, J. Geophys. Res. Atmos., 118, doi:10.1002/jgrd.50754.

Lekshmy, P., Midhun, M., Ramesh, R. et al. 18O depletion in monsoon rain relates to large scale organized convection rather than the amount of rainfall. Sci Rep 4, 5661 (2014). https://doi.org/10.1038/srep05661

Liu et al., 2014 Z.Y. Liu, X.Y. Wen, E.C. Brady, B. Otto-Bliesner, G. Yu, H.Y. Lu, H. Cheng, Y.J. Wang, W.P. Zheng, Y.H. Ding, R.L. Edwards, J. Cheng, W. Liu, H. Yang, Chinese cave records and the East Asia Summer Monsoon Quat. Sci. Rev., 83 (2014), pp. 115-128, 10.1016/j.quascirev.2013.10.021

Pausata, F.S.R., Battisti, D.S., Nisancioglu, K.H., Bitz, C.M., JUL 2011. Chinese stalagmite delta O-18 controlled by changes in the Indian monsoon during a simulated Heinrich event. Nat. Geosci. 4 (7), 474e480.

Scholze, M., P. Ciais, and M. Heimann (2008), Modeling terrestrial Biogeochem. Cycles, 22, GB1009, doi:10.1029/2006GB002899.

Sjolte, J., G. Hoffmann, S. J. Johnsen, B. M. Vinther, V. MassonâĂŘDelmotte, and C. Sturm (2011), Modeling the water isotopes in Greenland precipitation 1959–2001 with the mesoâĂŘscale model REMOâĂŘiso, J. Geophys. Res., 116, D18105, doi:10.1029/2010JD015287.

Sjolte J, Hoffmann G (2014) Modelling stable water isotopes in monsoon precipitation during the previous interglacial. Quat Sci Rev 85:119–135

Sjolte, J., Sturm, C., Adolphi, F., Vinther, B. M., Werner, M., Lohmann, G., and Muscheler, R.: Solar and volcanic forcing of North Atlantic climate inferred from a process-based reconstruc- tion, Clim. Past, 14, 1179–1194, https://doi.org/10.5194/cp-14-1179-2018, 2018.

Sjolte, J., Adolphi, F., Vinther, B. M., Muscheler, R., Sturm, C., Werner, M., and Lohmann, G.: Seasonal reconstructions coupling ice core data and an isotope-enabled climate model – methodological implications of seasonality, climate modes and selection of proxy data, Clim. Past, 16, 1737–1758, https://doi.org/10.5194/cp-16-1737-2020, 2020.

Stevenson, S., Otto-Bliesner, B. L., Brady, E. C., Nusbaumer, J., Tabor, C., Tomas, R., et al. (2019). Volcanic eruption signatures in the isotope-enabled Last Millennium Ensemble. Paleoceanography and Paleoclimatology, 34, 1534–1552. https://doi.org/10.1029/2019PA003625

Swingedouw, D., Mignot, J., Ortega, P., Khodri, M., Menegoz, M., Cassou, C., and Hanquiez, V.: Impact of explosive volcanic eruptions on the main climate variability modes, Global Planet. Change, 150, 24–45, https://doi.org/10.1016/j.gloplacha.2017.01.006, 2017.

Vinther, B., Jones, P., Briffa, K., Clausen, H., Andersen, K., Dahl-Jensen, D., and Johnsen, S.: Climatic signals in multiple highly resolved stable isotope records from Greenland, Quaternary Sci. Rev., 29, 522–538, https://doi.org/10.1016/j.quascirev.2009.11.002, 2010.

Vuille, M., M. Werner, R. S. Bradley, and F. Keimig (2005), Stable isotopes in precipitation in the Asian monsoon region, J. Geophys. Res., 110, D23108, doi:10.1029/2005JD006022.

Zhang X, Jin L. Association of the Northern Hemisphere circumglobal teleconnection with the Asian summer monsoon during the Holocene in a transient simulation. The

Holocene. 2016;26(2):290-301. doi:10.1177/0959683615608689

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