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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.

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The authors analysed a climate model simulation covering the last $\sim$ 1000 years mainly with respect to T, prcp, and d18O of prcp. They compared those values with speleothem data, obtained from a recently published data base. They compared the mean, variance, spectral characteristics, correlation pattern of speleothem data and model results on a global scale.

This project is a very ambitious one. To my opinion, the authors predominantly did a really good job. I appreciate the approach of analyzing the data on different aspects and with many, partly sophisticated, methods. The results that the speleothem and

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model data seem to do not fit too nicely is very interesting, as it points to the need to further improve both - the performance of climate models and the understanding of proxy data from speleothems. A very first step was already applied in this study: I like a lot that the authors tried to translate the temporal high resolution data of the model results into a speleothem-typical time series (accounting for water residence time in soil and karst, applying speleothem sampling resolution and T-dependent HCO3- → CaCO3 fractionation of stable oxygen isotopes). Nevertheless, I think it is possible to do it even better than that.

For example, and this is my largest point of critics: I am really not sure, if it is the mean annual d18O of precipitation, what speleothem are recording, but what seems to be used here from the model output. Isn't it rather the case that speleothems record the amount weighted d18O values of precipitation? Or in some locations, especially in more arid regions, with low amount of precipitation but elevated T, speleothem d18O reflect more likely the amount weighted d18O of infiltrated water. Thus, evaporation processes are important but not considered here. However, to my understanding climate models do provide those variables. They should have evaporation processes on land included. Wouldn't it be an option to try this variable for analysis? If I remember correctly, Wackerbarth et al., (2012, CP) did such an approach.

Especially, as the models should even account for evaporation-dependent fractionation processes of oxygen isotopes during evaporation, comparing the speleothem results with those of the models should potentially result in better agreement. The d18O values of the remaining, the non-evaporated water, which is finally entering the deeper soil layers and the karst system, would be known from the model. I guess this would be the easiest way (without any need to use cave-site specific insights from cave monitoring studies) to better compare model results and speleothem d18O values in a more comprehensive way. At this point, I don't ask to redo all the analysis with an infiltration weighted mean d18O instead of an annual mean d18O of precipitation, but it would be appropriate to at least include this possibility in the discussion section (e.g. Sec. 5.4)

– and try this in a potential follow-up study.

To my understanding, those evaporation processes could very well explain, why the d18O model data of precipitation tend to underestimate the speleothem-recorded d18O values in warm and more arid regions (see your Figs. 3d and 4d). In those regions, evaporation is very important and influences both – the amount of infiltrating water per month and the evaporation-dependent d18O enrichment of the non-evaporated water. I think that if one would account for that, it would potentially improve your model-data comparison. At least when comparing the mean values of the last 1000 a. But it has maybe even the potential to increase the average variability of the modeled d18O values, compared to your approach using rain water. And it maybe even brings some additional variance on the longer scales into the data. The reason for that could be that d18O of infiltrating water would - in addition to changes in the d18O of precipitation – potentially show a large change due to temperature effects on the amount of evaporation and the d18O of the remaining infiltrating water.

Otherwise, I have only comments of more minor attitude. Please find them listed below (with some repeatedly occurring instances, where I address the advantages of accounting for evaporation and amounted weighted means).

To sum up, I would really like to see this work published pending on an improved manuscript version, where my major concern is accounted for (in which way the authors feel more comfortable with) and the smaller issues from below are considered/discussed.

Best wishes, Jens Fohlmeister

L 18: "proxy-based variability of d18O": d18O is a proxy. Thus your phrasing sounds a bit weird. In the context of the text you might mean 'archive-based'?

L19-20: You might should add, that most of the difference on the side of the 'shortterm variability' (<∼20a) comes from smoothing due to soil water residence time and resolution. You showed quite nicely that on the long frequencies, both types of data sets do not agree – whatever you tried.

L 64: a <space> is need after 'system'

L65: I would be more specific here and state that 'The ratio of H218O to H216O in precipitation is an indicator of evaporation temperature, . . .' as it is possible to determine the d18O values in other reservoirs as well. And there other effects are also important.

L83: ' . . . hampered by non-linear growth processes (Dreybrodt 1980).' Is Dreybrodt 1980 the correct reference, as he focussed only on precipitation of CaCO3? Not on d18O variations. Maybe use one of his later studies, e.g., Dreybrodt and Scholz, 2011 or Dreybrodt and Romanov, 2016.

L85: '. . .as well as dating uncertainties'. Please explain, how dating uncertainties shall have an influence on the interpretation of d18O as you state here.

L88: Please correct brackets around the reference in this line.

L116: '. . .freshwater hydrological cycle in the model shows only a slight overestimation in the local evaporation (Pardaens et al., 2003).' According to this statement, there is some evaporation included in the model. So it should be feasible to work with those data, instead to precipitation only (both amount and isotopic signature).

L130: 'Vegetation above the cave has an impact on the source water . . .'. This reads a bit strange. Really on source water? Or rather on the amount of soil water and its d18O, which is coming from some source with a certain isotopic composition? Alternatively, you could write something like: Vegetation above the cave can alter the amount of infiltrating water and its isotopic signature.

L130-131: Here you already state, what potentially can have some strong effect. Thus, I wonder a bit, why you do not have accounted for that stuff in your analysis.

L134-135: This sentence should be changed, as it is not completely correct, if you mean with 'surface' the atmosphere. In addition, the CO2 and Ca2+ charging processes should be mentioned to make this better understandable for the reader. Please consider to rephrase to something like that: 'Infiltrating surface water is charged with soil gas CO2, which concentration is about 1-2 magnitudes larger than that of the atmosphere and enables the carbonic acid driven CaCO3 dissolution of the host rock. The generally lower partial pCO2 pressure conditions in the cave environment compared to that of the soil and epikarst makes the drip water degas ...'

L138-139: This sounds very dramatic. Not from the wording, but from the implications. As it is written here, I hope this to be not true. Otherwise, nobody should trust such speleothem d18O reconstructions.

L180: samples instead of sampled

L202-203: Have you performed this averaging also for d18O in precipitation? From my understanding, of this sentence you do. But I think it would be better to use an amount weighted mean of d18O in precipitation? This is closer to the value really infiltrating into the soil/karst.

Based on that, what about evapotranspiration and changes of this during the modeled 1000 years? Have you accounted for that?

Maybe, I am wrong, but my understanding of those isotope enabled GCMs was, that they have at least an 'evaporation on land' module. This should also account for fractionation effects on the evaporated water, but also for the remaining water, what you are interested in. Would it be an option to use those d18O values instead of that of precipitation (again weighted by the amount of infiltration)? Maybe not for this manuscript, but in any future application.

L219-221: Here you use the first time d18O_(pw). It is not explained here nor somewhere else. What is this?

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In addition, I am sorry, but I do not understand exactly, why you are doing this Greens function approach? I get pretty ugly results in terms of mass balance with this, if tau is small (e.g., 1, 2 ,3 years). For example using a tau of 1 year: even after 100 years waiting time, only 58 percent reached the cave. For tau=3 that are 84 %, what reached the cave after 100 years. For larger tau it works better, I admit. But as you use this filter with a residence time of 3 years, I would be happy if you please could explain why you used this way of residence time description. Have you normalized this somehow?

L228-229: Is it possible to rephrase this sentence? I regret to not being able to understand, what you mean by this.

Sec 3.2 and 3.3: Please explicitly state the number of used/available records for those approaches as those are most likely less, than that number mentioned in line 180.

L249: 'Generally, modeled values appear to be more depleted overall than the mean values of speleothem _18Odw.eq . . .'. Wouldn't this be a hint, that evaporation is important and should be accounted for (at least in any potential follow up study)?

L269: 'offsets also show a strong influence of temperature (Fig. 4d)' Only to repeat my statements above: At warmer climates there is more evaporation, which lead to enhanced d18O values of the remaining water compared to that of precipitation. The remaining water soaks finally into the soil and cave. Thus, it would be worth to check if soil water d18O works better than precipitation.

But this will most likely not solve the offset at colder T in the northern Hemisphere. But there, maybe it will work when using the amount weighted d18O of precipitation (or even better with infiltrating water) instead of annual mean d18O. As you brought up the example from Bunker cave. For this cave system and this region it was shown, that summer precipitation barely contributes to infiltrating water (most is gone by evaporation and transpiration), which is able to reach the cave (Riechelmann et al., 2011; Wackerbarth et al., 2010). As summer rain is more enriched in d18O compared to winter precipitation, this would shift the infiltrating water isotopic composition towards lighter values compared with the heavier annual mean d18O of precipitation. And would thus potentially bring the model results closer to the observed speleothem values.

L298-279: I am sorry but I am confused again. You are writing: 'The global distribution of variance ratios between _18Odw.eq and _18O (Fig. 5a) shows overall higher variability in the speleothem records than in the simulation'. I agree with this observation, but this is somehow in contrast to Fig 5 b and c, where the variance ratio between d18O_dw.eq and d18O is smaller than one. Is it possible that in Fig 5a you are showing the variance ratio of d18O_dw.eq and the already down sampled d18O of the model simulation?

L288: I think the reference to Fig 5 is not correct, as this figure does not give a hint on a 'smoothed model pattern'.

Figure caption 6: 'the simulated _18Opw but down-sampled to the same temporal resolution as in (a) with 3 year filter. 'Just to be sure, as it is not written somewhere: You first applied the three year filter and then did the down sampling, correct?

L332: 'We find 18, 15 and 22 significant correlations from 87 entities. . . ': Out of curiosity, are the records/sites within those observed significant correlations in the three model runs always the same or are they varying? I mean if for example a record from one cave is significant for one run is the same cave record as well significant for the other simulations?

Figure caption 8: indicates instead of indicat.

Figure caption 9: gridboxes instead of gridboxe

L385: 'spatial pattern for the offsets were not distinguishable.' I would slightly disagree here. Isn't it the case that in warmer climates the offset is more negative than in colder climates? See fig. 4d. You even described the low to high latitude trend in the northern hemisphere by yourself in lines 249-250.

L388: ‘They find a stronger influence of seasonality of precipitation in warmer climates, highlighting the importance of a karst recharge model’ Wouldn’t it highlight the fact, that the model d18O values should be calculated as a precipitation weighted mean? Or even as infiltration weighted mean?

L389-390: ‘observed a strong temperature dependency reflected in the offset and _18Odw.eq over the last Millennium, showing the influence of fractionation . . .’: You claimed, that you accounted for the temperature-dependent isotope fractionation during CaCO3 precipitation to calculate the d18O if the drip-water equivalent. So I think, this reason can be safely excluded.

I would evaluate it more likely that, as evaporation scales with temperature, the d18O values of precipitation have been changed by this process before the water even enters the epikarst zone. Thus, I would like to highlight it again (sorry for repeating myself), that using some infiltration weighted d18O mean is probably a better choice.

L410-411: ‘However, we find little regional consistency and high heterogeneity in the variance estimates from the speleothem records’. While there is indeed some high hetorogenity in the variance, I wonder, if this could be an argument for a strong influence of cave internal processes? That is a tricky one.

Later you discuss the correlation pattern, and this seems quite convincing that changes happen in the same direction and at about the same time at least at regional scale. Only the magnitude seems to change - as derived from the variance analysis. Wouldn’t this rather mean, that each cave/stalagmite strengthen or weaken the initial climate signal, but that it is still contained? As you said, this alteration could happen by soil/karst/cave processes. This way, one could argue about the magnitude of the changes, but not about the variation itself. How do you think about this?

L416-417: ‘Studies observing transit time in karst systems find increases in drip rate after an increase in precipitation e.g. after days (Riechelmann et al., 2011).’ I suggest to term this Riechelmann et al., (2011) work rather as a study, which investigates the cave reaction time to precipitation events - not a transit time study. And the cave reaction on heavy rain events is often fast (as observed in other caves as well), but it will not change the transfer time (too much). The residence time can only be found by tritium or other appropriate tracer isotopes (as you correctly describe below.)

L442: I guess, cave monitoring would not only help to compare two caves but also to improve the comparability between a cave and climate model results. You could add this as well. Furthermore, I think that not only monitoring would help, but that (model-based) weighted infiltration values would also help (as written already earlier). Even with your sentence in line 438 you imply so by yourself.

L455: 'but could also be influenced by non-climatic overprints on the _18O signal up to the centennial scale.' I would argue, that nearly all changes in the processes in the cave are climate driven. Unfortunately, sometimes they counter the pure climate imprint and on other locations they amplify it.

L476: 'as a bias of 1C in the simulated temperature would account for a change in _18Odw.eq of approximately 2‰.' If you really take the d18O-T relationship of Tremaine (or any similar) this statement appears to be wrong. Fractionation during CaCO3 precipitation is T-dependent by ~0.25 permil per °C. So this would mean an offset of 1 permil, if the modeled T is wrong by 4 °C. I guess this would only be the case in mountainous regions, were the orography of the model is not close enough to the true altitude of the cave. You mentioned some examples earlier in your manuscript.

L477:'A bias of _1‰ in the _18Odw.eq however, accounts for a temperature change of 0.1_C for the lowest simulated annual mean cave temperature (3.1_C in Norway), and a change of 13.1_C for the highest simulated annual mean cave temperature (32.5_C in the tropics).' This puzzles me now quite a lot. You said in your earlier sentence, that 1°C is 2 permil. This does not fit with your statement in this sentence. Maybe, you mean something different?

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