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Climate of the Past
Discussions

Interactive comment on "Proof in climatology for circulation effect of stalagmite δ^{18}O in East Asia: analysis on the ratios among water vapor transport passageway intensities in East Asia" *by* S. Nan et al.

Anonymous Referee #2

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In this manuscript, Nan et al., present an argument that: 1) there has been an increase in precipitation/stalagmite d18O in 1975-1995 relative to the previous two decades and 2) that this change in the isotopic ratio of the precipitation is causally related to a change in atmospheric circulation patterns, which influenced the source regions of the monsoonal moisture. As the authors suggest, there has been a lot of controversy surrounding the interpretation of Chinese stalagmites and it remains a topic ripe for discussion. Although a number of papers including Pausata et al., 2011 (referenced) and Lewis et al., (2010) (Water vapor source impacts...) have dealt with this topic on millennial timescales, the circulation effects have not been well addressed on interannual/decadal timescales over the observational period. To this extent, the paper has some novel analyses and some discussion that is potentially valuable for the interpretation of these controversial stalagmite records. However, the arguments in the paper were ultimately unsubstantiated and do not convince me that, 1) there was clearly an isotopic shift in 1976 and 2) that this shift was necessarily tied to circulation. Part of the failings of the paper may be tied to the writing, which was very unclear and so saturated with acronyms that some potentially good arguments could have just been obscured. A second major failing of the paper is that it took a lot of conjecture from a previous paper (Tan 2013) and presented these hypotheses as though they were well-accepted truths. Granted the other paper is published and therefore can (and should) be referenced but many of the ideas in that paper need more support before they can be used to build new ideas from.

If the Nan et al., manuscript were a simple presentation of interannual trends in monsoon moisture sources it could be close to acceptable but it makes jumps about the way circulation influences the d18O of precipitation, which are simplistic and not likely valid. While indeed circulation influences the monsoonal precipitation, continental recycling, local amount effects associated with convective processes, and seasonality ALSO influence the d18O of the precipitation and cannot simply be ignored. My opinion is that the "proof" the authors are seeking cannot be reached with the methodology used. As opposed to an empirical approach, I suspect the authors would need to use an atmospheric model, where the various processes that influence the d18O can be held constant allowing the influence of the different processes to be isolated. Similar perhaps to the approach taken by Pausata et al., 2011.

Major Comments (some reiteration of statements above):

1) I was not convinced from the Supplementary figure that d18O in many of these stalagmites was different in 1955-1975 relative to 1975-1995. In order to make this case, the authors need to use statistics and show the population in the first set of decades

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is different than the population in the second set of decades. Also, this analysis would need to consider age model error and analytical uncertainty. 2) Perhaps this is just a rhetorical issue, but why is BoB considered “remote” and WNP “local”? The distances seems about the same visually. 3) BoB provides about double the flux as the other sources. Therefore, from a mass balance consideration its influence would completely override the influence of the other sources unless of course the isotopic composition of the different regions produces moisture that is radically distinct. There are various datasets such as HDO from satellites and many isotope-enabled GCM simulations that should be able to show whether the moisture from the BoB is really different from the moisture from WNP and SCS. If the moisture sources from the three regions are similar (and I expect they are) than subtle shifts in the contribution of WNP and SCS should have only a tiny impact on the d18O of the precip. 4) A recent paper in JGR by Lee et al., (Asian monsoon hydrometeorology from TES and SCIAMACHY water vapor isotope measurements and LMDZ simulations: Implications for speleothem climate record . . .) show that vapor traveling over Asia does not follow a rayleigh distillation pattern but rather becomes enriched as it travels into the continent. This challenges the simple notion that “local” vs. “remote” moisture sources are necessarily isotopically distinct. Please discuss this paper and its implications for your interpretation. 5) All trend analysis of vapor sources and vapor source ratios need to be done with considerations of statistical confidence. I am not convinced visually of the presence of trends in these datasets. 6) The abstract needs to be rewritten. Within the first few sentences the reader is accosted with at least 12 acronyms. By the end, I was completely lost. I realize that acronyms are a necessary evil but at least for the abstract distill the main points of the paper in a way that a reader can immediately appreciate the significance of the study. 7) This is similar to #1 above, stalagmites integrate water in the karst and discrete sampling also integrates. So some consideration of this needs to discussed when arguing for the presence of decadal-length shifts. I was surprised for example, that in the composites in Figures 4-6, there were similar numbers of extreme positive and negative events coming from 1955-1975 as from 1976-1995. This made me think that because stalagmites are integrators, that perhaps instead of discussing changes in trend, the authors should calculate total (as opposed to averaging) grams of H2O from BoB, SCS and WNP in these two different decadal windows this would better represent the influence that extreme years might have on the isotopic values. 8) In Figure 4a none of the differences are statistically different so it is not really appropriate to discuss the differences as though they are meaningful. 9) It would be valuable to in some way diagnose whether NCEP Reanalysis is actually capable of capturing the monsoonal moisture fluxes. The best way I can imagine doing this is to do the same calculation with an independent Reanalysis dataset such as JRA, ERA, or MERRA. These datasets do not go back to 1948 but you could see if they produce the same results over the overlapping period. A comparative analysis of Reanalysis monsoon moisture fluxes alone would warrant a valuable manuscript. 10) There are a lot writing errors throughout.

Interactive comment on Clim. Past Discuss., 9, 4263, 2013.