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Climate of the Past Discussions

Interactive comment on "Variability of summer humidity during the past 800 years on the eastern Tibetan Plateau inferred from δ^{18}O of tree-ring cellulose" *by* J. Wernicke et al.

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Reply to interactive comment on "Variability of summer humidity during the past 800 years on the eastern Tibetan Plateau inferred from δ^{18}O of tree-ring cellulose" by J. Wernicke et al.

[reply] Dear anonymous referee # 1,

Thank you very much for your constructive comments and all the critical questions and suggestions you made on the interactive discussion paper "Variability of summer humidity during the past 800 years on the eastern Tibetan Plateau inferred from δ^{18}O of tree-ring cellulose". We express our gratitude that you recommend our paper to be published and to give us the opportunity to resend a revised manuscript (supplement of this reply letter). As suggested, we herewith resubmit our carefully and comprehensively revised manuscript (modified parts are highlighted in blue). We basically rewrote the discussion part and achieved new insights to the control mechanisms of relative humidity at the study site by applying spatial correlation

analyses. Furthermore, we omitted the spectral analysis part since a cross-spectral-like time series comparison is somewhat beyond the scope of this paper. Apart from the revised manuscript, each single comment was replied in detailed within this letter. Doing so, we hope that we satisfactorily responded to all critical points. If you have any further requests or questions, do not hesitate to contact us immediately.

Sincerely,

Jakob Wernicke

Anonymous Referee 1

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This paper presents an 800 year d18O cellulose record from eastern Tibet. The record is presented as an RH reconstruction. The data is very strong, there are good statistical constraints on the transfer function and the record comes from a place with limited data. For all these reasons, I support the publication of the data so others can take advantage of this important reconstruction. That said, I found much of the discussion on the climate dynamics and proxy processes to be extremely lacking. There is no extensive discussion of the processes that give rise to the RH control on the proxy. For example, there is a slope of -2.3 permille% RH. What can this slope tell about the processes that transfer a change in RH onto the tree ring cellulose? This needs attention. Further, some of the statistics, namely those devoted to spectral analysis to be superficial.

[reply] The focus of this study is to present a long-term temporally high resolved new

climate reconstruction at a place with limited data. That implies in remote areas, like the eastern TP, plant physiological studies might be infrastructural and technical very difficult to conduct. That means to really determine the plant physiological processes that transfer the rH signal into the cellulose must currently remain insufficiently clarified in most of the remote study sites in high mountain ecosystems.
We depart from the approach of determining spectral signals of our times series, because we might consider these analyses in a more concrete and comprehensive future study examining exclusively these such issues.

There are a number of weak and confusing discussions about land-surface thermal gradients, ENSO, NAO, solar controls on the monsoon. These discussions are often confusing and occasionally lack logic. Ultimately, a lot of the space that is devoted to large scale climate modes turns out to be unimportant. Therefore, the discussion of climate dynamics can be simplified and focused.

[reply] We explicitly referred to findings of Liu et al. (2013) and Sano et al. (2013), who found strong relations to ENSO, PDO and the Pacific sea surface temperature at locations approximately 500km south of our study site. We followed their work and expected to find similar strong correlations of our $\delta^{18}O$ record with ENSO (Sea surface temperature in region 3.4). Surprisingly, we couldn' t find reliable indications for a Pacific influence. Accordingly, referred to recent findings of Mölg et al. (2014), we assume that a strong westerly signal might modulate our record. Thus, we applied the monthly resolved NAO indices in order to find a link to the North Atlantic climate. However, we could not verify any significant and stable relationship with large-scale circulation modes (see discussion paper Figure 7). For that reason we summarized in the discussion paper (Wernicke et al, 2014) : ". . .a superimposing large-scale circulation influence neither from NAO nor Nino 3.4 SST can be confirmed ." Consequently,

we assumed that a more regional to local signal controls the reconstructed relative humidity at our study site. Therefore we conducted the heat flux spatial correlation within the discussion paper.

I suggest starting the discussion start from the simple question of what controls RH on the eastern Tibetan Plateau. RH can be controlled by 1) air mass trajectory, 2) whether air is principally subsiding or rising, 3) local land surface processes such as soil moisture, 4) boundary layer dynamics for example, the stable nighttime boundary layer tends to have 100% RH whereas daytime turbulence and mixing of free tropospheric air tends to lower RH. Once a clear understanding of controls of RH at the proxy site are established than the significance of this in terms of large scale climate can be explored. Ultimately, the observation of a century long decline in RH is fascinating but understanding the processes that actually led to this decline would make this study really breakthrough. I recommend this paper be published but only after a major rewrite of the Discussion is conducted.

[reply] We conducted spatial correlations of the ERA interim data in order to determine the spatial variability of the rH over the TP for several elevation levels. We obtained a very regional pattern, which highlights the strong link of relative humidity at our study site to the relative humidity variability of the entire TP. Additionally, a strong negative relationship of the rH conditions in our study region to the west-central Asia region was found, which may be an indication for a westerly influence in the mid-altitude troposphere.
Furthermore, we examined references providing explanations for the remarkable rH decline since ~1870s (see revised discussion part). Unfortunately none of these studies give a conclusive explanation about the dynamic causes causes for the moisture

decline, which has to be considered in a more detailed future study.

Pg 3328 4: "This is the first chrononolgy for eastern Tibet..." 8: "variations...More moist conditions prevailed during the termination..." 10: Simply state that there is no systematic shft in the mean state during the LIA, which is contrary to Indian Summer Monsoon reconstructions. 10: Your record does not show a consistent decline through the 20th century. It appear to flatten off by the 1950's. 19-20: It is never clear what records share the same spectra. Cross spectral analysis is needed. 24: Vuille is a good reference for d18O of monsoon but not a good reference for the socio-economic impacts of the monsoon.

[reply] Pg 3328

- *We applied the suggestions of line 4,8,10 in the revised manuscript.*
- *Indeed, the trend slope between 1950 and 1996 is not significantly different from zero. Thus, the moisture decline is attuned since around the ~1950s, or a little later (~1970s), which coincides with the restrengthening of the thermal gradient between Bay of Bengal-North Indian Ocean and the Equatorial Ocean (see Figure 7).*
- *As mentioned above, we will consider the spectral analysis in a future study.*

Pg 3329 1: Do these references actually discuss changes in humidity or a decline in precipitation. The two are not identical, related-yes, but not identical. 1:..."explained

by a reduction in the thermal gradient..." 11: "... increases and can be used to facilitate targeted decision making regarding water and resource management." 12: "dislocation" is the wrong word. Northerly movement... 15: Intraseasonal oscillations such as the madden Julian Oscillation have strong controls on monsoon precip and particularly in generating the complex spatial patterns. 29: "stronger rainfalls..."

[reply] Pg 3329

- *1: . . .weakening trend of the ASM precipitation amount was reported. . .*
- *We included the comments of line 1, 11 in the text*
- *We implemented the suggestion concerning short-term monsoon variability, such as the MJO, at the beginning of our discussion part. Nevertheless, we want to point to the fact, that the climate signal recorded within the tree-ring cellulose contains annually integrated information, rather than signals on a monthly or daily scale. According to the nature of the MJO as a highly variable phenomena appearing between 30-90 days, these modulations are only hardly detectable within tree-ring cellulose. In principle, tree-ring inferred data provide the opportunity for intra-annual signals. In case of the very narrow tree-rings of our studied trees, an intra-annual analysis was not feasible. Thus, we cannot provide indications for the modulating effect of the MJO to our relative humidity reconstruction.*
- *29: changed into "strong rainfalls"*

Pg 3330 11: erase "sensitive" 14: "Therefore," 15: "... unclear to what extent ..."

[reply] Pg 3330
We have indicated this in the revised version.

Pg 3331 6: "The oldest tree is 804 years old"

[reply] Pg 3331
We have indicated this in the revised version.

Pg 3332 5: "During periods of the chronology with extremely narrow rings, we used shifted block pooling to obtain sufficient material." 11: Spectrometer misspelled

[reply] Pg 3332
We have indicated this in the revised version.

Pg 3333 22: fix "the the"

[reply] Pg 3333
We have indicated this in the revised version.

Pg 3334 9-11: More information on the met data is needed. Sunshine hours and vapor pressure are not common met products. Is the sunshine hours, photosynthetic active radiation or net radiation? Is vapor pressure obtained with a hygrometer or inferred through the RH sensor? What equation is used to calculate vapor pressure? Vapor Pressure Deficit would be useful to correlate against d18O cellulose, following the work of Ansgar Kahmen. 12: Evapotranspiration is used here but actually it is only transpiration that influences the leaf water fractionation. Unless you are referring to the secondary effects that evaporation had on soil water and consequently on the d18O of the plant source water. 13: "has demonstrated" 14: "temperatures on tree ring growth." 14-16: Why would May temperatures influence d18O of cellulose. I understand that growth is limited by temperature at high altitude sites but why would temperatures have an effect on the isotope ratio? Could this be tied to the temperature controls on RH? Please explain. 16-18: Sunshine has a negative impact on d18O of cellulose, which seems odd to me. Later on in the paper you discuss that less sunshine=more cloudiness=higher relative humidity which would lead to lower d18O cellulose. What is the mechanism by which more sunshine actually decreases d18O of cellulose? Perhaps, more sunshine=more convection=more rainfall=higher humidity=low d18O cellulose. Please elaborate on how sunshine directly influences the isotope ratio in the cellulose. 27: should be "r=-073"

[reply] Pg 3334

- *The meteorological data were provided by our Chinese colleagues. They obtained the data from the "China Meteorological Administration". Sunshine hours*

were accounted as duration of net radiation greater than 120W/m^2. Unfortunately, the equation how vapor pressure was calculated, was not made available to us on request. We have indicated this in the revised version.

- *The findings of Kahmen et al (2011) about an integrating climate predictor may be applicable in case of a $\delta^{18}O$ response to both, temperature and relative humidity. Our correlation analysis revealed only a weak and non-significant response of δ18O to temperature. Therewith Vapor Pressure Difference (VPD) is expected to explain not more of the $\delta^{18}O$ record. Nonetheless, using the Magnus formula, we calculated the Saturation Vapor Pressure and subtracted the vapor pressure in order to obtain the VPD. The mean VPD of July-August significantly correlates with our $\delta^{18}O$ record during the calibration period (r = 0.68, p <0.01). This was not unexpected, since VPD was evaluated from rH. However, the expected weaker relationship was achieved and thus less explanatory power of VDP to our record can be confirmed. Additionally, in perspective of climate projections or forecasts, working with "hard" climate elements, such as rH (precipitation, temperature), is more targeting than the reconstruction of integrated climate elements.*
- *12: Thank you, we changed evapotranspiration into transpiration*
- *13,14: The tree growth is limited by temperature and early summer temperatures might alter the snow melting time. Thus, if temperatures in May are reasonable high, the snow melt would be initiated early and might contribute to plant accessible water. Accordingly, tree metabolism and plant water fractionation would start earlier. However, that is only an assumption we are not able to validate with our data and therefore exclude this discussion from the manuscript.*
- *16-18: The sentence of in line 16 was misleading, because we referred "inversely" to the negative correlation of $\delta^{18}O$ with relative humidity. Sunshine hours are of course not inversely correlated to $\delta^{18}O$, but positively correlated to $\delta^{18}O$*

(see figure 3). The relationship of high sunshine hours, less cloudiness, decreased relative humidity and therewith an enrichment of heavy isotopes is correct. This positive feedback was validated by findings from the southeastern Tibetan Plateau (Shi et al., 2012). We corrected accordingly in the revision.

- *27: We fixed that in the revised version*

Pg. 3335 1-2: "more robust than for single months" 13: should that be "binomial" 19: The slope between d18O cellulose and RH is -2.3. How does this compare with previous studies such as Roden 2000. Please consider quantitative comparisons of the slope you found with as many previous studies as possible. For modeling proxies, it is useful to understand how global these slopes are or whether they are species and region-specific 23-25: It really seems that the decline in RH begins in 1871 and ends in the 1950s. The low pass filter suggests the trend continues but this appears to be an artifact of edge effects. I would like the slope of d18O calculate for 1950 through the present and see if it is statistically different than 0.

[reply] Pg 3335

- *1,2,13: We fixed those in the revised version*
- *Several studies have documented that the leaf water enrichment with heavy isotopes is related to air moisture and leaf temperature (VPD), but also to the isotopic composition of atmospheric water vapor (Flanagan et al., 1991). Thus, under stable moisture conditions the kinetic isotope fractionation mostly depends on the isotopic composition of atmospheric water vapor. Due to mean moisture*

*conditions and water vapor isotope content of air varies among different regions, the relationship between several microenvironments varies respectively. Additionally, the "effective path length" differs among species, which induces different slopes of the regression function (Kahmen et al, 2009). Nevertheless, under natural circumstances negative slopes between rH and $\delta^{18}O$ were identified globally (McCarroll and Loader (2004); **?**). We added the relevant information in the revised manuscript and appreciate these suggestions in order to achieve deeper insights about plant-physiological processes.*

- *We considered your comments and received a slope that apparently flattens at ~1950s, perhaps a little later (~1970s). The slope is very small (m = 0.01) and statistically not significantly different from zero (p = 0.63). Chung and Ramanathan (2006) see the reason in uneven warming trends over the northern and equatorial Indian Ocean. We tested their argumentation by adding a gradient calculation in the manuscript (see Figure 7). The graph shows the difference (blue line) between the HadSST2 data for the Pacific Ocean along the equator and Bay of Bengal region. Since ~1950s the temperatures seems to evenly increase, while the gradient becomes larger since ~1970s. Based on the traditional temperature driven gradient approach of strengthening/weakening monsoon moisture conditions, the observed gradient increase since ~1970s implies an attenuation of the distinct moisture decline.*

Pg 3336 4: consider an alternative word to "confuted" 10-13: From the wavelength analysis it appears that the cycles are very intermittent. It would be good to show the global wavelet and also standard FFT to argue that these cycles are statistical and persistent through the record. If these cycles are going to be compared against other records cross-wavelet or cross-spectral analysis is needed. It is not sufficient to say they are commonly forced signals without doing a cross spectral analysis. 26-27:

"data sets and the Lhamcoka ....On the contrary, the tree ring width"

[reply] Pg 3336

- *4: We changed "confuted" to "were not corroborated" in the revised manuscript.*
- *10-17: We excluded all frequency analysis of our and other time series, due to these analyses are beyond the scope of this paper of presenting a new rH reconstruction on the eastern TP. However we are aware that these analyses have should be conducted in a comprehensive future study.*
- *26-27: We entirely rewrote the Discussion paragraph.*

Pg 3337 18-22: If the humidity decline is associated with a change in the thermal gradient than show this. Please calculate the thermal gradient using ocean and land temperature datasets such as from Hadley Centre and correlate it to the reconstructed humidity. It is not sufficient to say this, when data is available to test this. 22: "reduction are not sufficiently clear." 24: replaces "discovered" with "found" 26-28: The solar argument for the humidity decline requires significantly more explanation. If solar forcing is heating the land and ocean evenly, than this would not generate a change in the thermal gradient. If solar is heating up the land more than the ocean than this would increase the monsoon and humidity. If solar is heating up the ocean faster than this could theoretically increase the thermal gradient and be a plausible explanation for the change in moisture. There are solar reconstructions and recent observations that could be used to support your argument. In general, this argument needs to be significantly elaborated. Further, I would argue that your record shows flattening off

since the 1950's which is consistent with the timing of massive aerosol loading over Asia.

[reply] Pg 3337
18-22: We implemented a new graphic (Figure 7), which displays the SST gradient between the equatorial and northern Pacific Ocean. The variations are rather small, but seemingly increase since the ~1970s. This uneven temperature contrast might be the reason for the attenuation of the moisture declining trend at our study site.

Pg 3338 5: Why would increasing distance from the Bay of Bengal result in an amplified signal. Please explain the logic here. 13-18: The presence of a North South bipolar in Tibetan Plateau humidity is interesting. Is there a modern analog for anti correlation between the north and south TP? This would help to support the proxy observation. 24: "postulated a dominant influence ..."

Pg 3339: 1-2: Please rewrite this sentence beginning, "However..." 4: It is unclear to me, do you argue that cloudiness directly influences RH or that cloudiness is a proxy for precipitation, which directly influences RH? The argument gets obscure in text. 9: "positively associated with the NAO via its impact on Eurasian snow cover and thus invokes ... " 11: "might induce an el nino..." Is this saying the NAO causes ENSO events? Unclear what is meant by this.

Pg 3340 1-3: Many previous studies have noted a recent reduction in ENSO's influence on the monsoon. See the pioneering work by Kumar on this. 4: Should that read "cannot be confirmed"? There are a number of issues with this section 1) A lot of time is spent laboring through ENSO and NAO influences and then at the end you just reject

that idea and invoke a local control argument. Why not just remove all this NAO-ENSO discussion ... it is confusing, takes up a lot of space and is ultimately irrelevant. 2) The statistical significance of the running correlations is not really properly treated. The df is low for running correlations. 6-15: I found the discussion on the correlation with sensible heat to be rather confusing and missing some key discussion points. It is true, that the correlations are very strong but the argument that sensible heat flux is a "direct expression of vertical air motion" is incorrect. Within the ERA Interim model there are directly modeled "vertical velocity" and "convective precipitation rate" terms, which are actually direct indicators of convection as opposed to sensible heat, which is a combined term sensitive to surface temperature, soil moisture etc.... Furthermore, the argument that sensible heat is an indicator of evapotranspiration is also odd. Latent heat flux is the better indicator of moisture fluxes. It is also important to discuss that ERA Interim is a model not an observation. I would like to see correlations with latent heat flux, soil moisture, skin temperature, vertical velocity etc...all the component that control sensible heat flux to get a sense of the actual process that leads to these strong correlations. Further, I think it is worth discussing that the spatial correlations with sensible heat appear to be focused along a latitudinal band. I wonder if this is an indicator of westerly controls or on interannual variation in the northern extent of the ASM.

[reply] Pg 3338-3340
We appreciate your suggestion of rewriting the discussion discussion part entirely after intense discussions with other colleagues. Thereafter we focused the discussion on what controls rH at our study site (new spatial correlations displayed in Figure 5), took large-scale modulations into account, discussed the different behavior of other proxies from the TP (hopefully accomplishing), and summarized dynamic reasons for the rH variability on the eastern TP. Thus, we hope that all confusing and inflated argumentations were removed. However, we believe that according to the results of Liu et al. (2013) and Sano et al. (2013) (already mentioned previously) the association

of our $\delta^{18}O$ record to common and frequently used large-scale circulation indices (ENSO,NAO,El Nino/La Nina Events) was targeting, even though we were not able to verify a large-scale effect with these methods.

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