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Comment on cp-2021-53

Anonymous Referee #1

Referee comment on "Tree-ring oxygen isotope based inferences on winter and summer moisture dynamics over the glacier valleys of Central Himalaya" by Nilendu Singh et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-53-RC1, 2021

Review: Tree-ring oxygen isotope based inferences on winter and summer moisture dynamics over the glacier valleys of Central Himalaya by Singh et al.

The authors use previously published d18O of tree-ring chronologies from central Himalaya to reconstruct past variations in the annual and seasonal atmospheric moisture content (AMC). The decadal coherency between summer-season AMC and previously published d13C-derived glacier mass balance data was observed despite a decline in summer precipitation since the mid-20th century. The authors attribute this to an increase in pre-monsoon precipitation and enhanced vapor recycling through an increase in evapotranspiration. This is a novel approach. However, various issues need to be addressed.

The GMB is affected by precipitation as well as temperature variability. It is surprising to note that the manuscript ignores the discussion regarding temperature variability (seasonal and inter-annual) in the region and its likely role in influencing GMB. This is one of the weak points of this work. Also, discussion regarding the correlation between the AMC and precipitation is missing.

There appears to be a contradiction in the authors' claim: it is claimed that the winter-westerlies rather than summer precipitation govern the ice-mass variability (line 22-23) but subsequently, the authors attribute GMB to increase in the pre-monsoon precipitation and to decrease in summer precipitation. Comparison of Figures 3b and 4a clearly shows the correlation between the summer AMC and GMB.

There is a serious issue with the main claim made in the manuscript, “Decadal coherency between summer-season AMC and GMB remained relatively stable since the mid-20th century, despite a decline in central Himalayan summer precipitation” (lines 23,24) and “However, it is intriguing that despite a strong decline in summer precipitation since the mid-20th century…, correlations remained stable (Fig. 4c)” (lines 332,333). The correlation between summer-season AMC and GMB (Fig. 4c) appears to be statistically insignificant. Therefore, the claim regarding stability/instability of the correlation is not valid. 95% p-value bands are needed in figures 3c, 4c and 4d. Further, figure 4a does not corroborate the assertion made in line 333: “… a strong decline in summer precipitation since the mid-20th century (Fig. 2; Fig. 4a)”. If the authors meant to say that the summer AMC showed a strong decreasing trend during the 20th century, then for that period there should be a positive correlation between summer AMC (Fig. 4a) and GBM (Fig. 3b). In that context, the ‘intriguing’ observation (line 332) merely reflects the fact that the AMC and GMB are positively correlated and the correlation can remain stable despite the decrease in the summer season’s AMC.

Post-1960, annual AMC shows a positive correlation with GMB (Fig. 3c). For the same period, the summer season’s AMC does not show any correlation with GMB (Fig. 4c) and the winter season’s AMC shows a negative correlation with GMB (Fig. 4d, which again appears to be statistically insignificant!). This requires explanation.

Comparison of Figure 3b and 4a suggests a good positive correlation between low-frequency variations of the summer season’s AMC and GMB which contradicts the claim made at line 358. Therefore, the interpretations can vary if the length of the period of observation is changed.

Although the manuscript describes the correlation between AMC and GMB, it does not describe the causal link between the two. The meaning of positive and negative correlations (figures 3c, 4c and 4d) is not clear. Further, if AMC and GMB have a causal relationship, shouldn’t it be reflected at the interannual scale as well? For example, the amplitude of inter-annual in AMC (fig 3a) is much higher than that in the 11-yr/51-yr moving trends. Given the annual nature of vapor recycling, the authors should demonstrate that the large variations in AMC do control the GMB variability. It would be also useful to give the statistics of correlation (for example, split period calibration-verification) between the summer AMC and GMB. The correlations of the low-frequency variations between AMC and GMB do not necessarily establish the causal relationship. Again, why the revival of winter westerlies-driven moisture influx leads to a negative correlation between winter season's AMC and GMB (lines from 356 to 358) (i.e. a higher winter season's moisture influx leads to a decline in GMB) is not clear.

The manuscript claims to present a new reconstruction of the winter season's AMC. Although referred at line 279, it is not clear how the AMC during the winter season was estimated. I assume it is the difference between the annual and summer seasons' AMC. There are problems with this approach. d18O of the broadleaf species is affected by the winter conditions as well (figure S2c). There are also issues with the carryover of photosynthates from a season to the next season. The best way to prove the validity of this approach is to demonstrate the statistics of the correlation between the estimated and actual wintertime AMC on the inter-annual scale.

Isotopic compositions, especially d-excess, of the seasonal precipitations and glacier ice can be used to show that pre-monsoon rain and evapo-transpired water is contributing to the glaciers. I am not sure about how much of the recycled moisture goes to the glaciers and associated dynamical constraints.

Line 355. ….AMC and GMB correlations remained negative (Fig. 4d). Figure 4d does not demonstrate this.

Line 19: it is not clear what a 'phase-shift' indicates here.

Line 97: not clear how this was done.

Lines 231-234: if the relative contributions from westerlies and the Bay of Bengal branch of ISM is changing significantly at the tree-ring sampling sites, using mean d18O to reconstruct past summer and winter influx has limitations. Can the relative contributions of these moisture sources explain the 8 to 10 permil variations in d18O?

Line 246-247: d18O of the atmospheric moisture is one of the parameters that control d18O of the tree ring. It is not the main factor.

Figure 3. If the 51-year moving correlation is shown, (a) and (b) should also show 51-yr moving averages.

Figure 3. “Dark lines in (a) and (b)……”

Figure 4. Add 95% p-value bands and error bars to the figures. If the 51-year moving correlation is shown, (a) and (b) should also show 51-yr moving averages.

Line 355. The negative correlation mentioned here is not seen in the relevant figure (4d)

Line 356. ..1920s and 1960s…wrong range. The drop is at 1950 to 1980 during which the correlation is negative.