

## Comment on cp-2021-53

Anonymous Referee #2

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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-RC2>, 2021

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Based on tree-ring oxygen isotope records, Singh et al. reconstructed the moisture dynamics in the central Himalayas. Subsequently, they linked the summer and winter moisture dynamics with the glacier mass balance during the past 273 years. Overall, the topic seems interesting and suitable for the readerships of this journal. However, I have four major concerns about the current manuscript. 1) Why didn't the authors choose the atmospheric moisture content data from MERRA-2 instead of other datasets? It's better to provide some reasons. 2) The reliability of the atmospheric moisture content reconstruction is questionable. The authors only showed the correlations between tree-ring oxygen isotopes and atmospheric moisture content, and didn't present any details for their interannual comparisons, which made the reconstruction questionable. 3) There are lot of the citation problems. For example, in line 40, the authors cited 10 papers in only one sentences. If the authors cited too many papers in one sentence, readers or reviewers will question whether authors had read these cited papers or not. For example, Thompson et al. (2000) interpreted the Dasuopuo ice core oxygen isotopes as a temperature record in the raw paper, while the authors treated Dasuopuo ice core oxygen isotopes as a hydroclimate proxy in the citation. 4) The part "3.5 Vegetation greening and radiative balance analyses" didn't seem to relate to the topic of this manuscript. I am not sure why the authors put this part in this manuscript. Detailed comments and suggestions are as follows:

- Line 65 "A set of information pertaining to the vegetation greening trends in the valleys, decadal changes in seasonal vegetation pattern, biophysical aspects and radiative fluxes were accessed using high-resolution satellite and CERES (Cloud and Earth Radiation Energy System) datasets.". After reading the whole manuscript, I didn't understand why authors used satellite data to support their conclusions.
- Line 105¼□for the Table 1, revise "Annual phenology" to "forest type"

- Line 110, the authors used the atmospheric moisture content data from MERRA-2 and soil moisture data NOAA. Two datasets are from different reanalysis products. I suggest that authors used both atmospheric moisture content and soil moisture only from the same product (MERRA-2 or NOAA). Besides, there are too many reanalysis datasets, the authors should explain why they chose the MERRA-2. Have the reliability of the selected data been tested or not?
- Line 245 "Earlier studies (Baker et al., 2016) also found that in warm-moist tropical rainforest,  $\delta^{18}\text{O}$  values are controlled by basin-intrinsic ecophysiological processes.". Baker et al. (2016) found  $\delta^{18}\text{O}_{\text{TR}}$  in Bolivia are indeed controlled by basin-intrinsic processes, with rainout over the basin the most important factor. This is totally different from how authors have cited in this manuscript. Therefore, I don't think this paper (Baker et al., 2016) has been cited corrected.
- Line 250, the authors only provide the correlations, and there are too limited information for the reconstructions. Interannual comparison for the observed and reconstructed data is required.
- Line 260, "Paleoclimatic evidence such as tree-ring  $\delta^{18}\text{O}$  chronologies (Singh et al., 2019; Sano et al., 2012, 2013, 2017; Xu et al., 2018), speleothems (Kotlia et al., 2012; Liang et al., 2015) and ice-core records from the central Himalaya (Thompson et al., 2000; Kaspari et al., 2008) show that regional hydroclimate shifted towards a drier phase since the mid-19th century,". Originally, Thompson et al. (2000) interpreted the Dasuopu ice core oxygen isotopes as a proxy of temperature instead of hydroclimate (e.g., precipitation amount). This is inconsistent with how authors have cited in this manuscript.
- Line 325, I don't understand the differences between Figure 4c and Figure 4d based on the current caption.
- Line 365, "Isotopic studies and modelling experiments indicated a high moisture recycling rates and an enhancement in positive trend in recent decades (An et al., 2017; Dirmeyer et al., 2009; Keys et al., 2016; Harding et al., 2013; Tuinenburg et al., 2012)." The authors didn't mention where these cited studies come from. These references are cited in a rough way. Did the cited studies support your statement?
- Lines 520 and 675, pay attention to the symbols in the reference list (e.g.,  $\delta^{18}\text{O}$ ). Please check the reference list.