

This manuscript presents the process of canopy evaporation in a subalpine forest at the Niwot Ridge US-NR1 AmeriFlux site. The authors employ a combination of independent measurements that includes GNSS-based VOD, tree sway frequency, and eddy-covariance evapotranspiration, to understand the dynamics of water stored in the forest canopy, particularly following rainfall events. The observations are also compared with results from the Community Land Model CLM4.5. Overall, the paper presents a unique multi-sensor approach and offer valuable insights into canopy water dynamics. Although the research is insightful, I have a few comments that are detailed hereafter which should be addressed before publication.

Major comments:

- The manuscript would benefit from improved readability by restructuring the Introduction section. Some parts included in the Introduction seem more appropriate for the Materials and Methods section.
- GNSS L-band is used to retrieve VOD. Given that L-band is sensitive to water content in woody components (e.g., stems and branches), it would be helpful if the authors could provide a clearer justification or discussion regarding this aspect in the context of their interpretation.
- Page 5, Line 139: Authors mention “Linear interpolation over time was used to convert the hourly data to a 30-min time series”. What is the rationale for linearly interpolating hourly data to 30-minute resolution, instead of conducting the analysis directly at the native 1-hour interval? Does this interpolation introduce any artifacts?
- Please clarify the climate classification of the study site.
- Tu & Yang, 2022, Hua et al., 2020 discuss the overestimation of PET/ET particularly in arid and semi-arid environments while Sun et al., 2016 discusses underestimation of ET in cold areas using traditional methods. Could the authors clarify whether ET overestimation/underestimation is relevant for this ecosystem, what are the implications and uncertainties?
- A brief explanation of the detrending process for tree sway frequency in the main text would be beneficial, even if detailed in Raleigh et al. (2022). Further, please clarify relevance of detrending tree sway alone and not others.
- The study relies on 17 wDry days for the VOD and tree sway, while the ET results for wDry days (in Figure 6a,6b) are based on a significantly larger sample size of 176 days from a longer period (2004–2022 vs. 2022–2023 for VOD/sway). To strengthen the paper to explicitly discuss how this disparity might influence the comparison, perhaps a composite of ET from only the 17 wDry days used for VOD/sway could be presented in the supplementary information for a more direct comparison.

Minor comments:

- Page 1, Line 1: Is Interception only due to warm-season precipitation?
- Page 1, Line 9: Can the authors elaborate/clarify on this “changes in internal tree-water content than VOD.”?
- First paragraph of introduction section is detailed/explained well but not sufficiently referenced.
- Page 2, Line 22: Authors mention “Evapotranspiration ET is the sum of transpiration with soil and canopy evaporation” this is confusing; but $ET = \text{soil evaporation} + \text{canopy evaporation} + \text{transpiration}$?

- Throughout the manuscript, there are instances where terms like 'evapotranspiration' are written in full repeatedly after being defined by their abbreviation (e.g., ET). I recommend using the respective abbreviations consistently after the first mention to improve readability and maintain consistency.
- Page 10, Line 210: Authors mention “To ease comparison with other studies” and no references were cited here. I recommend including relevant studies to support the statement.
- Table 1 indicates VOD measurements began in June 2022, but the study period warm season is defined starting in Sept 2022. Please clarify on this.
- Please mention the temporal sampling interval for each observation type and ensure consistency throughout the manuscript.