# *Interactive comment on* "Using GNSS-based vegetation optical depth, tree sway motion, and eddy-covariance to examine evaporation of canopy-intercepted rainfall in a subalpine forest" *by* S. P. Burns et al.

## Reply to Referee #2

**S. P. Burns et al.**

sean@ucar.edu

Date: May 30, 2025

The comments by Referee 2 are greatly appreciated. We have listed the comments by Referee 2 below in italics, followed by our responses. We added numbers to each of the specific comments so it is easier to reference the comments by Referee 2.

*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.*

This is an accurate summary of our manuscript and we appreciate the positive comments about the "valuable insights" our study provides. We address the specific comments below.

**Under the category "Major comments":**

1. *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.*

We agree that readability in the Introduction could be improved. This comment would be more helpful if a specific example of what the reviewer is referring to was provided (though some context is given in comment #10 below; please see our reply to comment #10). In an attempt to improve the readability, we moved a paragraph (lines 83–88) to Sect. 2.1 in the Materials and Methods section. We agree that Materials and Methods a more appropriate location for this information. Between moving this text and addressing comment #10 (below) we think the readability of the Introduction has been improved; however, if this is not what you were referring to, please let us know.

2. *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.*

One of our conclusions is that VOD at US-NR1 is not very sensitive to the internal tree water content. We agree that this could be expanded upon; and our response to a comment from Referee 1 contains additional details. To summarize that response: We noted that VOD at the US-NR1 site site (during dry days) is at around 0.35 which is much lower than VOD in other studies which are

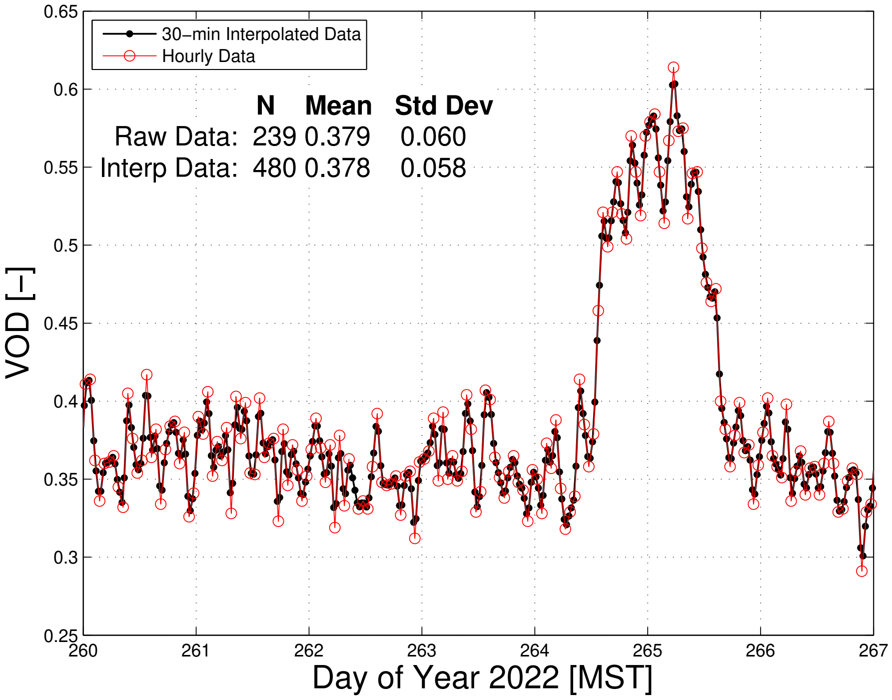


Figure R1: A portion of the the VOD time series shown in Fig. 2b in the discussion manuscript. Here we have included both the raw/hourly and interpolated/30-min data (see legend). Statistics (number of samples $N$, mean, and standard deviation Std Dev) from both sets of VOD data for the period DOY 160-170 are included as a table in the upper-left hand part of the figure.

on the order of 0.6 to 1.1 (e.g., Holtzman et al., 2021; Humphrey and Frankenberg, 2023; Yao et al., 2024). Based on this we postulate that the internal water content of the US-NR1 subalpine trees are low compared to the trees in more humid locations and therefore VOD is not detecting this (lower) water content signal. It could also be that the US-NR1 forest is less dense. A study by Luo et al. (2020) has shown that tree water content varies significantly between different species. We will add additional discussion about this in the revised manuscript, and, at the very least, point out that the US-NR1 VOD in dry conditions is lower than VOD in the other studies (as mentioned above).

3. *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?*

This is a good question. There are two primary motivations for interpolating the VOD data from hourly to 30-min time periods; (i) all the water vapor flux data and tree sway data are calculated over a 30-min period, and (ii) more clarity in the diel cycle is obtained with as high a sampling frequency (or as short an averaging period) as possible. Rather than downgrading the measurement period of the tree sway and flux data, we decided to linearly interpolate the VOD data.

To check how the linear interpolation might affect the VOD statistics, a 7-day time series of the raw/hourly and interpolated 30-min VOD data is shown in Fig. R1. This figure includes 10-day mean and standard deviation of the respective VOD data, and the differences in mean values (0.379 vs 0.378) are very small (less than 0.5%), whereas differences in the standard deviation (0.060 vs 0.058) are less than 3%.

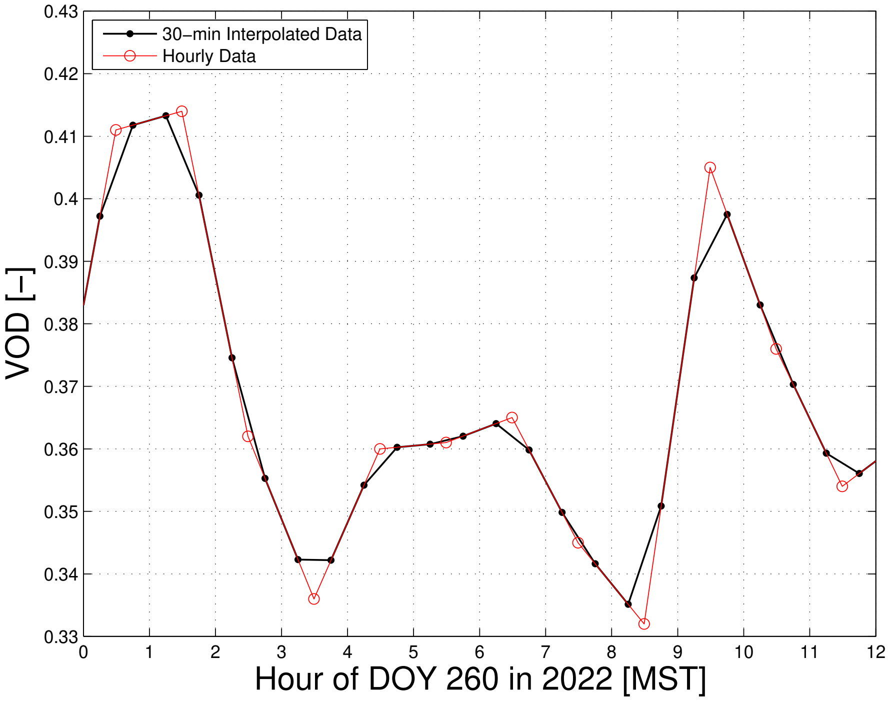


Figure R2: As in Figure R1, but only showing the first 12 hours of DOY 260.

If we zoom in a bit more and only show half of a day, then we see that the effect of linear interpolation decreases the range of the measured VOD (Fig. R2). This happens because the 30-min periods are centered on 15 and 45 minutes past the hour, while the hourly data are centered on 30-min past the hour. The decrease in the range of the measurements is why the linearly-interpolated data have a slightly smaller standard deviation than the hourly measurements.

As far as we can tell, the linear interpolation does have have any significant impact on our results or conclusions. We further test this by repeating Fig. 3d in the manuscript, but showing both the diel cycle of the hourly VOD data along with the 30-min interpolated VOD data (Fig. R3). There are small differences, but nothing that would change our interpretation of the results.

4. *Please clarify the climate classification of the study site.*

We added additional information in the site description section which describes the US-NR1 climate. Our revised text is:

> " The mean annual temperature at US-NR1 is around 2°C. According to the Köppen–Geiger climate classification system (Kottek et al., 2006) the site is type Dfc which corresponds to a cold, snowy/moist continental climate with precipitation spread fairly evenly throughout the year; the long-term mean annual precipitation at the site is around 800 mm and snow typically covers the ground from mid-November until late May (Burns et al., 2015)."

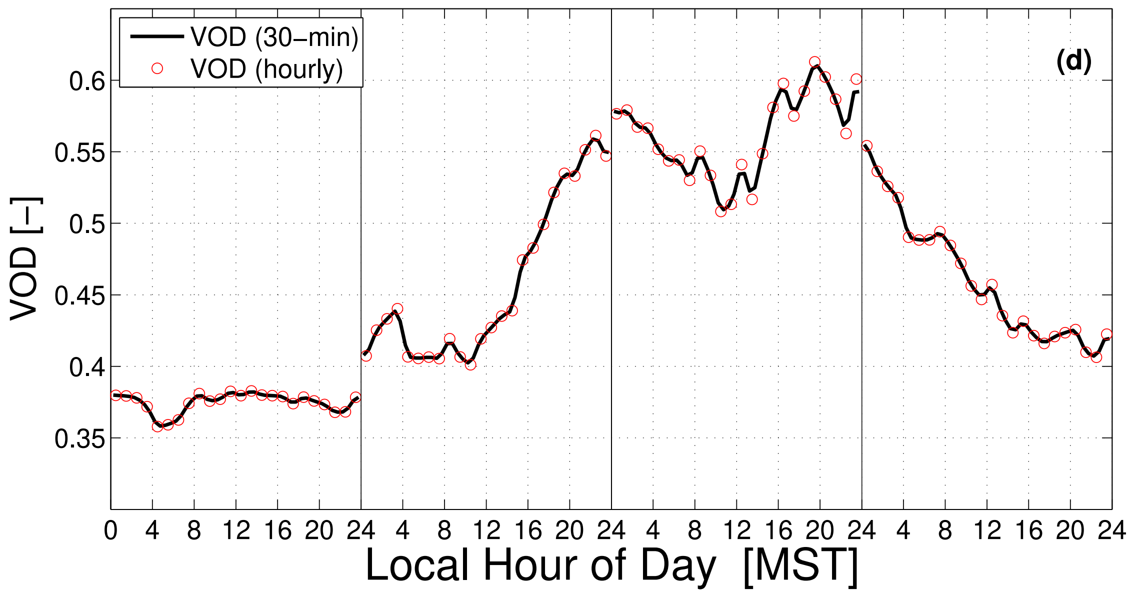


Figure R3: Similar to Fig. 3d in the manuscript, but showing the mean diel cycle for both the hourly and 30-min interpolated VOD data (see legend). See Fig. 3d within the manuscript for additional plot details.

5. *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?*

We considered the following papers brought up by the reviewer: Hua et al. (2020); Sun et al. (2016); Tu and Yang (2022). If any of these are incorrect, please provide the DOI, so we can make sure to find the correct paper.

These papers are primarily concerned with modeling ET. Sun et al. (2016) use modeled ET to look at effects on the air and land-surface temperature. Hua et al. (2020) uses Penman-Monteith to model ET and compare with surface met data and MODIS products. Tu and Yang (2022) focused on potential evaporation using a variety of models and compared these to ET from flux towers; however there are some open questions about net radiation assumptions within the chosen model (e.g., Szilagyi, 2022). While these are all reasonable/useful studies, we used a direct measurement of ET in our study. The studies could be relevant to the CLM4.5 portion of our study, but we examined how the model reacts to precipitation, not the metrics used in those studies. Perhaps these citations would be appropriate for a future study (i.e., that is trying to model absolute ET), but not the current one.

6. *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 low-frequency detrending of the tree sway frequency data is described on lines 155–157 and an example is shown in supplemental Fig. S4. The detrending uses a 10-day sliding median filter. The low-frequency detrending was not part of Raleigh et al. (2022). The tree sway data has an obvious low-frequency trend in the time series (i.e., Fig. 2c in the manuscript). As we have shown, the low-frequency detrending did not impact the results (see discussion on lines 245–247 and Fig. 4). None of the other variables showed an obvious trend with time so there was not a need to detrend any other variables (in fact, it was not really needed for tree sway frequency either).

7. *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.*

It is correct that in Fig. 6a,b, the period of 2004 to 2022 is used for the ET diel cycle. This is discussed as a limitation of our study (lines 388–391) in the discussion manuscript. We note that a composite of the above-canopy ET for the 17 wDry days is shown in Fig. 3b. There are several reasons that a longer period is used for ET in Fig. 6: (i) the subcanopy flux system was not working correctly in Fall 2022 into summer of 2023, and (ii) a smooth ET diel cycle greatly benefits from having many samples/years of data. This can be seen by comparing the above-canopy ET shown in the wDry part of Fig. 3b with that in Fig. 6a. With only 17 ET periods the line in Fig. 3b is a bit jagged whereas the one in Fig. 6a is much smoother. To extract a smooth diel cycle is an even larger challenge in the subcanopy, where we use an open-path IRGA.

For these reasons (primarily item (i) above), we cannot do this suggestion by Referee 2.

**Under the category "Minor comments":**

8. *Page 1, Line 1: Is Interception only due to warm-season precipitation?*

Is "Page 1, Line 1" referring to the title which has "canopy-intercepted rainfall" in it? This comment is a bit unclear, but we think you are likely referring to the fact that fog/dew are also a source of intercepted water? If this understanding is correct, this is a good point and we have added text to our list of limitations in our study as,

"Nor did we consider possible effects of fog/dew as a source of intercepted water."

We mentioned that we think fog/dew is generally a small source of intercepted water at our site (see lines 68-69 in our discussion manuscript).

If we mis-understood this comment, clarification would be appreciated!

9. *Page 1, Line 9: Can the authors elaborate/clarify on this "changes in internal tree-water content than VOD."?*

This point is discussed at the end of Sect. 3.2 (lines 268–275) and shown in Fig. 5 within the discussion manuscript. This comment is also closely related to comment #2 above. As we replied in comment #2, the low-level of VOD during dry days is something we need to point out more clearly within the manuscript (see comment #2 for additional details).

10. *First paragraph of introduction section is detailed/explained well but not sufficiently referenced.*

All the references listed in lines 30–31 are the sources of information for the descriptions prior to that. We agree with Referee 2 that the references are too far removed from each of the descriptions. This accidentally happened as the manuscript was being modified/edited. In the revised manuscript, we have put the references in locations closer to the phenomena being described.

11. *Page 2, Line 22: Authors mention "Evapotranspiration ET is the sum of transpiration with soil and canopy evaporation" this is confusing; but ET = soil evaporation + canopy evaporation + transpiration?*

These were intended to be equivalent statements (are they not stating the same thing?). We have reworded line 22 to be:

> "Evapotranspiration ET is the sum of transpiration with soil evaporation/condensation and canopy evaporation/condensation (e.g., Stoy et al., 2019; Miralles et al., 2020). For our forest site, we assume that evaporation/condensation is primarily evaporation (i.e., transport of water vapor from the soil/canopy surfaces to the atmosphere) instead of condensation of water on these surfaces."

If this was not the intention of your comment, please clarify.

12. *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.*

We found a few places where this occurred. We have removed those instances in the revised manuscript; however, we have kept "evapotranspiration" written out fully within the abstract, figure captions, and the conclusions.

13. *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.*

Good point. We have revised this sentence to be:

> To ease comparison with other studies (e.g., Klaassen et al., 1998; Humphrey and Frankenberg, 2023), we have expressed ET and the rate of precipitation in units of mm of $H_2O$ per hour ($mm\,h^{-1}$).

14. *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.*

Thanks for pointing this out. The GNSS measurements started in June 2022, but we initially had both systems side-by-side at the top of the tower (this is described on line 140–141 in the discussion manuscript). However, it is true that VOD can only be measured with both an above-canopy and subcanopy system; therefore, we have revised Table 1 to show the VOD dates as "Aug 2022" rather than June.

15. *Please mention the temporal sampling interval for each observation type and ensure consistency throughout the manuscript.*

On line 210 we wrote, "Unless otherwise noted, ET and all other statistics are calculated over 30-min periods.". We have also added a new column to Table 1 which lists the averaging periods used for each variable.

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