Author's response to Reviewer #1

Thank you very much for the constructive review of our manuscript, as well as for recognizing this work and helping us improve it through valuable comments and suggestions throughout the manuscript. We carefully considered all the comments and proposed revisions for the manuscript accordingly. We address the reviewer's general, specific, and technical comments below. The reviewer's comments are given in italics, our responses in blue.

The authors present in this work a detailed and thorough analysis of estimating plant water status with the use of satellite (GNSS-T) retrieved vegetation optical depth (VOD) measurements. By focusing on sub-diurnal timescales, they open new ways of using this method to estimate plant water status, which is a highly relevant variable for many environmental related scientific fields. The manuscript is well-written, and both the methodology and interpretation are generally well-articulated and clearly presented. Below, I provide several general, specific, and technical comments that I recommend addressing prior to publication.

General comments

RC#1.G1 *Instrumental set-up: I wonder why the authors did not deploy leaf wetness sensors. Though I understand their hesitance as described in L588, deploying such sensors might have provided an additional constraint for defining canopy wetness (see also comment #3). This is not criticism per se on the methodology, but I am interested in whether this was an active choice (i.e. if the authors have objections against the use of leaf wetness sensors for such applications) or simply deal with what is available at the measurement site.*

Leaf wetness sensors would indeed help constrain dew estimates, but they were unavailable for our setup. Our reservation about leaf wetness sensors concerns only their spatial representativity across the canopy layers, not the sensors as such. For further validation of dew occurrence and amounts, they would be a useful tool in future work.

RC#1.G2 *Sect. 2.4 and 3.1: The derived attenuation coefficient b is based on Si modelled with the CanWat model. As b is a key parameter for the entire manuscript, I think it is important that here it is discussed properly whether the modelled Si, and by extension b, is expected to contain a random and/or deterministic error. In fact, as later described in Sect. 3.2, dew generally seems to be overpredicted. Could this be due to an error in b, making its extrapolation to dew events result in an overestimated Sd, and does accounting for this error make the Sd more plausible? Additionally, in Sect. 4.1 and Fig. 10 the authors show that b has a seasonal component. Could it be that seasonal bias for dew events also results in affect the overprediction of dew? Perhaps the authors could report the uncertainty on the derived b.*

This comment has two parts, uncertainties in b and possible reasons for dew overestimation.

1) Uncertainties in b. The attenuation coefficient b is a hydro-structural canopy property that describes the sensitivity of VOD to changes in interception water volume. It carries errors from both VOD and Si, and by extension from the CanWat model. We currently only propagate the VOD uncertainty as RMSE(VOD, Si) from Eq. 8, reported in Table 4, but we assume no error in Si. The reviewer rightly notes that Si is a model output variable of theCanWat model, and we currently don't take the model uncertainty into account. To respond to the reviewer's comment and account for the model uncertainty of Si, we refer to the analysis by Grunicke et al. (2020), Table 9, where the interception model is reported to have an RMSE of 0.5 mm throughfall over the study period. Normalised as nRMSE = RMSE/mean(TF), this is about 24 %. The same spread appears as interannual variability in their Fig. 5, which we associate with a random error, while the intraannual variability is small given the low phenology of the spruce stand. We will propagate this Si uncertainty to RMSE(b) by bootstrapping the regression VOD = b*Si with a random Gaussian error of 24% on Si, and update the regression lines in Fig. 4, the RMSE of b in Table 4, see RC#1.Fig1 and RC#1.Fig2 below.

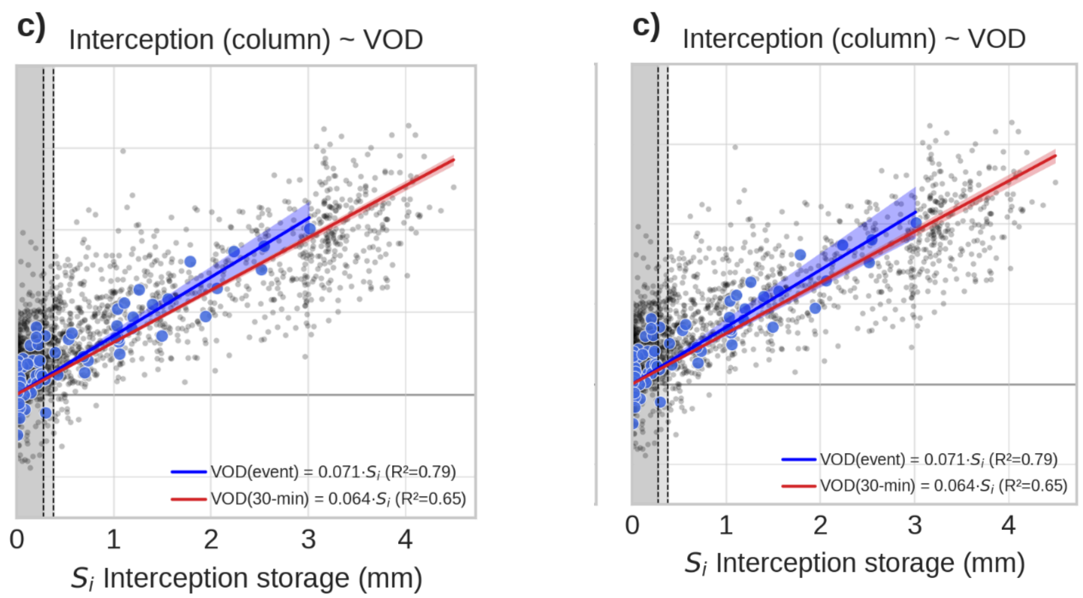
Current: No Si error bootstrapping

GNSS footprint	Scale	b [95% CI] (VOD/mm)	RMSE	R^2	N
cone	30 min	0.131 [0.128, 0.135]	0.051	0.634	1388
	event	0.149 [0.135, 0.163]	0.025	0.807	69
cylinder	30 min	0.064 [0.062, 0.065]	0.051	0.654	1611
	event	0.073 [0.065, 0.080]	0.026	0.797	69

New: Si error bootstrapping

GNSS footprint	Scale	b [95% CI] (VOD/mm)	RMSE	R^2	N
cone	30 min	0.131 [0.127, 0.135]	0.057	0.634	1388
	event	0.149 [0.129, 0.169]	0.030	0.807	69
cylinder	30 min	0.064 [0.062, 0.066]	0.057	0.654	1611
	event	0.073 [0.062, 0.084]	0.030	0.797	69

RC#1.Fig1. RMSE increases from 0.051 → 0.057 (cone, 30-min), also CI widens marginally.

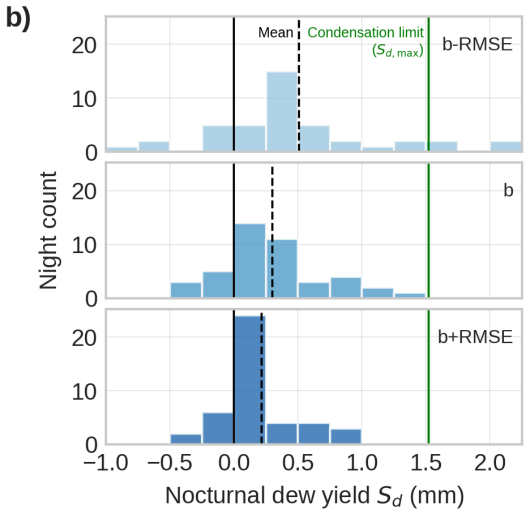


Current: No Si error bootstrapping *Revised: CI contains errors propagated from Si*

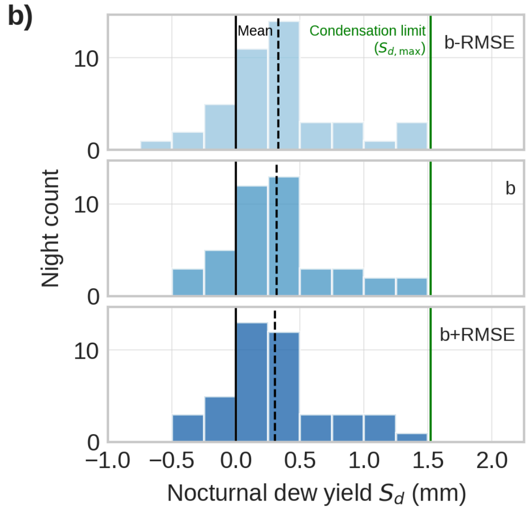
RC#1.Fig2. CI widens in Fig 4 with bootstrapping Si errors from Canwat.

By adding a random error on Si, the RMSE of the VOD-Si relationship slightly increased, providing a more realistic estimation of the relationship.

2) Dew overestimation. Overestimated dew implies a too large b, which can result from an underestimated modeled Si. This bias cannot be explained with the Si error (reported above), where we can only add a random error. However, as the reviewer mentioned, we found seasonal biases related either to canopy structure and phenology or to interception properties (Fig. 10). We acknowledge that this seasonal variation in b can influence the dew overprediction, and we calculated Sd with seasonal variable b. Here, we report that we can reduce the overestimation of b when making b variable (RC#1.Fig3), which further highlights that structural changes in the canopy alter the relationship of VOD to water storage. If the reviewer agrees with these findings, we will add the new finding as a replacement for Fig 6, and move the results of Fig 10 in the interception results chapter. We will have to make additional changes in the methods where b is calculated to acknowledge the temporally variable b method.



Dew predictions with static b.



New: Dew predictions with seasonally variable b (Fig 10).

RC#1.Fig3.

Finally, we argue that the remaining overestimation of Sd is more likely caused by random VOD noise, consistent with the unphysical negative dew values in the predictions.

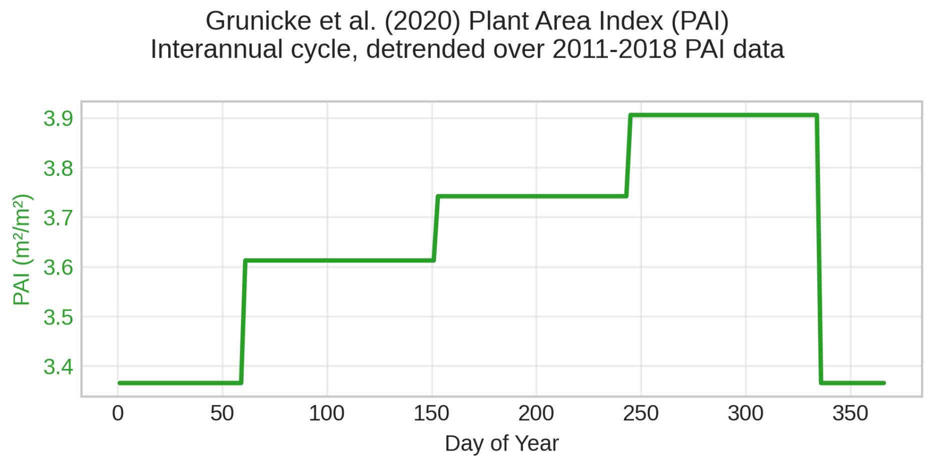
RC#1.G3 *Sect. 2.5: In this section, the authors define criteria during which they assume presence of dew. By setting these criteria as strict, they aim to prevent false positives in their analysis. However, I wonder if such strict criteria may not result in too many false negatives. The authors reflect on this in L583-593, but do not mention if these false negatives could also impact the comparison between dew and non-dew state earlier on in the manuscript. These false negatives might in fact be present in Fig. 6a, where the tail of the non-dew pdf extends to relatively high VOD values. Perhaps the authors could investigate an extra classification with "potential wet canopy due to dew", or maybe just exclude these points from the comparison with dew events.*

Our strict filters prevent false positives in the dew class at the cost of more false negatives in the non-dew class. We appreciate the suggested transition class “potential wet canopy” to remove those false negatives and sharpen the classification. However, such boundaries can be drawn from the hydrometeorological variables in many ways, with little theory to guide them, since the literature typically reports only dew-presence/condensing boundaries (e.g., Ritter et al., 2019); we would gladly incorporate any boundary definitions the reviewer can suggest. Since the paper's primary aim is to show that dew can be distinguished from non-dew conditions via VOD, a more precise separation would very likely only strengthen this evidence without changing the conclusion's direction. Accordingly, at L583 we will note that false negatives likely inflate the non-dew class and thus underestimate the dew/non-dew separation. We therefore prefer to retain our conservative approach, prioritising physically interpretable and reproducible classifications over an additional, uncertain class, and to clarify this rationale, we will again highlight the conservative “dew” class in Section 3.2, where Fig. 6 is discussed. We remain happy to discuss this further and ultimately implement a transition class if the reviewer wishes.

Specific comments

RC#1.S1 *L137: Please specify "low variability in LAI throughout the year".*

The reviewer is correct that we did not show evidence for low intra-annual LAI variability in the manuscript. We draw this from Grunicke et al. (Fig. 3), using only data after the last thinning in 2011, after which the stand was not thinned again. The mean annual PAI cycle (max 3.9, min 3.38) varies by about 13 %, which we interpret as low and typical for old-growth spruce forests (RC#1.Fig4, data from Grunicke et al, 2020). LAI and PAI are closely related structural descriptors, but we only have reliable data for PAI. We will therefore change LAI to PAI in L137 in wording and provide the intraannual variability in this sentence.



RC#1.Fig4. *Plant Area Index intra-annual cycle based on measurements of a singular location close to the tower in DE-Tha between 2011 and 2018, reported in Grunicke et al. (2020).*

RC#1.S2 *S3a: When shifting from pooled to per-sat, the density peak shifts towards a lower VOD (besides the lower noise). Can the authors comment on this?*

The pooled mean VOD can be dominated by high-VOD satellite outliers, whereas the per-satellite mean weights each satellite equally mitigating outlier influence. While having a mechanical explanation, we have not investigated which satellites introduce this shift, but will do so if the reviewer deems this necessary.

RC#1.S3 *L242: is b (or VOD for that matter) affected by the active rainfall (rate) itself (I'm not an expert on this).*

It should not be. Si is the liquid water on the canopy and is by definition independent of the input flux. Precipitation alters Si, but if CanWat models Si correctly, the rainfall rate does not affect b. The parameter b is mostly representing structural properties in the canopy.

RC#1.S4 *L375: Perhaps the authors could also mention the canopy scale LAI somewhere, as this would provide valuable information for other studies that might not have a 3D representation of the canopy.*

The LAI at DE-Tha was reported as 7.6 for 1999 in Grünwald and Bernhofer (2007), which agrees well with the PAI of 7.1 m^2/m^2 reported in Queck et al. (2014), given the variability between measurement methods. We expect only minor changes on the stand scale over time, since the overall stand structure has not changed fundamentally. We will add both numbers in L375 to ease transfer of our method to other sites.

RC#1.S5 *L458: Please clarify whether (part of) these 19 forest ecosystems have a comparable climate to Tharandt.*

Of the 19 forested sites in Ritter et al. (2019), five are evergreen-needleleaf (RMNP, DEJU, JERC, TALL, OSBS). They range from montane and boreal (RMNP) to warm-humid (TALL), with an average dew frequency of 24 %. This gives a general indication of dew occurrence in this forest type. Closer to Tharandt in ecoclimate, we already added the temperate Douglas-fir forest in the north-western U.S. (dew frequency 28 %, Sibley et al., 2022) in L458. Further evidence from literature is not known to the authors but indeed would be welcome hints from the reviewer if literature was left undetected.

RC#1.S6 *L467: Are these values per unit area ground surface or unit area plant or leaf surface? In fact, this could be clarified throughout the manuscript.*

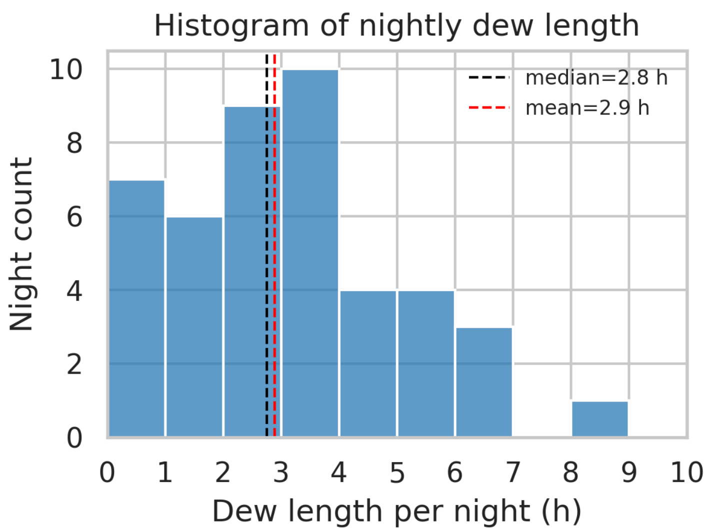
The dew prediction values are given per unit ground area, projected from the 3-D cone subset that encompasses the bulk of the canopy biomass. We will clarify the reference area in the dew methods (2.5) and the results (3.2).

RC#1.S7 *Fig. 6: Both panels show negative dew-yield. Is this in fact evaporation after false negatives? Or just because of random noise (if so, perhaps the authors can add the detection limit in this figure for further clarification).*

We kept (unphysical) negative dew yields to keep the mean dew yield estimate unbiased. A small fraction could be false negatives lost to evaporation, but this is unlikely. We use no data after sunrise, and VPD is usually lowest at the end of the night, so dew rarely evaporates then. Negative dew yields more likely reflect the VOD uncertainties the reviewer mentions. We agree that showing the detection limit adds confidence in the VOD observations, and will shade the area 0 to δ (0.38) in gray.

RC#1.S8 *L526-527: Does this mean that all dew events are 6 hr or longer? Are events that have a duration of e.g. 5 hr also averaged to 6hr VOD values? What is the average length of the dew events according to the criteria stated in Sect. 2.5?*

The information in Table 6 seems to be not straightforward to interpret, unfortunately. It shows how VOD (after noise suppression) raises sensitivity to interception and dew events. For dew, we used the distribution of predicted dew yield per night across the years, so nighttime-aggregated data. Table 6 is an if-then scenario. If we noise-reduce GNSS-T VOD to the equivalent of a 6 h window (δ = 0.26 mm), about 35 % of dew nights would be detectable. We do not test whether dew length within a night overlaps the 6 h VOD window, so the table shows the expected sensitivity gain from noise reduction, not a matching of VOD and dew observations. We still think it helps the reader judge which temporal averaging is at least needed to detect changes in surface water. Dew length varies strongly, with a mean of 2.9 h, a maximum of 9 h, and many nights of only 30 min. We will add the dew length figure to the supplement (RC#1.Fig5) and clarify in Sect. 3.4 that the detectability metric is not matched to observed events of the same window as the VOD averaging window. We have rewritten this sentence for clarity, documented in RC#2.T16.



RC#1.Fig5. *Histogram of nightly dew length over the study period, based on the hydrometeorological filter set defined in Section 2.5 (median 2.8 h, mean 2.9 h).*

Technical comments

We thank the reviewer for the careful technical reading and have addressed all points below.

RC#1.T1 *L324: Please explicitly define* θ*.*

θ is now explicitly defined as water content at L324.

RC#1.T2 *L426: "Note, however, that the those ...." --> remove "the".*

Corrected.

RC#1.T3 *L461: Should reference not be to Fig. S9?*

The reference is intentional and points to Fig. 6a, which shows the likelihood of correctly classifying dew presence with GNSS-T VOD, so it does not refer to Fig. S9.

RC#1.T4 *L465: Please explicitly define AUC.*

AUC is now explicitly defined as the area under the curve at L465.

RC#1.T5 *L473: "(...) the a fair comparison (...)" --> remove "the" or "a".*

Corrected, thank you.

RC#1.T6 *L473: "Generally, the there is a (...)" --> remove "the".*

Corrected, thank you.

RC#1.T7 *L574: "reported observations" --> "reported observations in literature".*

Corrected, the text now reads "reported observations in literature".

References

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