Response to RC1 comment on "Sublimation Measurements of Tundra and Taiga Snowpack in Alaska" by Kelsey Spehlmann, Eugénie Euskirchen, and Svetlana Stuefer.

RC1: 'Comment on tc-2023-153', Steven Fassnacht, 27 Dec 2023

Dear Dr. Fassnacht,

We thank you for your constructive and insightful comments. The following pages contain comments that appear exactly as they were received. Our responses are inserted next to each comment in blue text. Thank you again for taking time to review our manuscript.

Sincerely,
Kelsey Spehlmann, Eugenie Euskirchen, and Svetlana Stuefer

General

As the authors illustrate through the literature that they cite, estimating sublimation is very important for the annual water balance, and few studies have examined multiple (>3) years, which they do.
Overall this is a good paper, but there are a number of steps that are not well explained and thus the methods are unclear.

Response 1: Thank you for pointing out that estimating sublimation is an important subject and that few papers estimate long-term sublimation fluxes. We appreciate suggestions on how to clarify methods and details of the analysis.

All the details are listed below; here are several examples, 1) 30% of the EC data are gap-filled. How? This is apparently in one, or all, of the Euskirchen papers;

Response 2: Additional information on data gaps and gap filling methodology were added to Section 3.1. "Sublimation calculations use both filtered latent heat measurements (70%) and gap-filled data (30%). Filtering primarily refers to removing data when there is optical impedance by precipitation or aerial contaminants. This is denoted by the automatic gain control values measured by the infrared gas analyzers. These values are used as a quality assurance/quality control variable for both flux and radiation data, with 60% as the maximum threshold AGC value. Data gaps occur from instrument malfunction, instrument calibration, or occasional power outages in winter months. For data gaps of 1–6 days, missing observations were replaced by the mean for that time period (half hour) and based on adjacent days using the ReddyProc software (Euskirchen et al. 2024). For data gaps of 1–2 weeks, marginal distribution sampling is used to fill missing data (Euskirchen et al., 2024)."

2) section 3.3 presents "standard" statistical methods to evaluate the relationship between sublimation rates and meteorological and environmental variables. To what end is this done? Is this the same as what is stated at the beginning of section 4.2?

Response 3: Section 3.3 has been re-written to better explain what was specifically done and why. “Standard statistical methods were applied to the dataset for the following analyses (Gottelli & Ellison, 2004): 1) Ordinary least squares (OLS) regressions were used to evaluate sublimation rates with other water fluxes, with meteorological and environmental variables, and over time; 2) Analysis of variance (ANOVA) calculated whether there are differences in sublimation rates between the six sites, between regions, and between sites with a canopy. For tests with more than two groups, the ANOVA is followed by post hoc Tukey test s for pairwise comparisons to identify which means among a set of groups are significantly different from each other; 3) and Pearson’s correlation coefficient (r) and single (OLS) and multiple linear regressions (MLR) evaluated the relationship between sublimation rates and meteorological and environmental variables: Pearson’s correlation coefficient (r) and single and multiple linear regression (MLR). The three methods use a significance level of 0.05.”

We often do not have enough data, i.e., no EC measurements, to adequately estimate sublimation rates. The authors correlate EC-estimated sublimation with hourly and daily meteorological data (Air Temperature * VPD * Net Radiation * Temperature Gradient * Wind Speed). In Table 6, it looks as if these are multiplied together. The daily correlation is quite high (mean R^2 of 0.81 for the Lowland Boreal). The authors could consider doing a split sample analysis, i.e., leaving out 2 years to create the model, and then evaluating the model on the years left out. Further, are all five variables used in the MLR necessary? There can at least be a discussion of this.

Response 4: We used the * sign to indicate it is a fully crossed model, per methods in Gottelli and Ellison (2004). To clear confusion, we replaced the * with a comma and updated the Figure’s caption to clarify fully crossed. We better describe model selection in Section 3.3 Statistical Methods and comment further in discussion. See also Response #6 regarding the MLR discussion.

Section 5.1 presents a discussion of uncertainty. The focus is underestimation due to blowing snow sublimation and “data processing.” The former is informative. The latter is attributed to an overestimation of sublimation that is actually melt-evaporation. This is interesting. However, under the umbrella of “data processing,” the authors should at least mention measurement errors and uncertainty (e.g., Hultstrand and Fassnacht, 2018; https://doi.org/10.1007/s11707-018-0721-0).

Response 5: Thanks for sharing this paper, we added this reference to Section 5.1.

The MLR (section 4.4) and section 5.2 present and discuss sublimation rates as a function of meteorological data. Some readers will not see the point of this analysis. I think that it is useful, as we often do not have EC data. However, we often have meteorological data from a regular weather station. The authors should consider using the bulk flux method as a comparison (as was done in various papers cited). At least provide a more thorough discussion of why the MLR is relevant here (and elsewhere). Also, consider which variables in the MLR are not readily available at a regular weather station, i.e., Temperature Gradient and perhaps Net Radiation. How would the MLR model degrade if only available at a regular weather station were used? This should computed (or at minimum discussed), since we typically don’t have “Temperature Gradient,” as defined in this paper.

Response 6: This a very good point, however the bulk flux model is beyond the scope of this study. This study took advantage of long-term EC measurements that had not been previously analyzed for sublimation. A future study of model-based methods with direct EC measurements could be of value. Further discussion has been added regarding stepwise explanation and the decrease in predictive power.

Specific

Line 22: "phase change from ice crystals in the snowpack" – technically they are snow grains and not ice crystals (see Fierz et al., 2009; IACS Guide, etc.)

Response 7: This sentence was updated to change "ice crystals" to "snow grains" and include the reference.

Line 30: "errors associated with solid precipitation measurements in the Arctic (Goodison et al., 1998)" – while this is a good reference. The authors should also consider the numerous recent papers (last decade) related to WMO-SPICE

Response 8: We added reference to Nitu et al. 2018.

In the Introduction, consider the paper by Herrero and Polo, 2016 The Cryosphere) as they also compare various sublimation estimation methods.

Response 9: Thank you for the reference. We added information from this reference into the fifth paragraph of the Introduction, with a list of other direct methods to estimate snow sublimation.

Line 33: "eddy covariance (EC) method … continuously measure latent heat fluxes" – this is mostly true. Sexstone et al. (2016) illustrated some of the uncertainty with EC estimates of the latent heat fluxes.

Response 10: For this and other reasons, "continuously measure latent heat fluxes" has been removed.

In Figure 1, if any of the photos correspond to the Ameriflux sites (Figure 2) add those labels at least to the captions, but preferably in the figures with "b)," etc.

Response 11: Ameriflux site IDs were added to both Figure 1 and caption.

Figures 1 and 2: perhaps combine these two figures as they both relate to the study sites

Response 12: This is a good suggestion that we also deliberated prior to manuscript submission, but decided that the photos and site locations are better shown as separate figures.

Lines 81-82: the "mean annual precipitation (MAP)" was estimated as "140–270 mm, with 60% of that occurring as snow" by Euskirchen et al. (2017). Without reading that paper, I am curious if/how the precipitation was adjusted for undercatch. This is relevant and should perhaps be stated here. The estimate of precipitation impacts the computation of the % of sublimation to precipitation

Response 13: Added "for the years 1988-2007" to the lines to clarify that these values represent long term climate conditions. This is included in the Background section with the intention to add context to these regions. These values were not adjusted for undercatch, nor were they used in this manuscript's methods during computation of the % sublimation to precipitation. In fact, this is why we used the end-of-winter SWE, to serve as a proxy for more accurate cumulative solid precipitation.

Line 82: "air temperatures are below freezing" – use the term "colder than freezing" instead of "below freezing," as below has an elevational context

Response 14: Good catch. "colder than freezing" has replaced "below freezing" in this line.

Line 85, Line 122, and Figure 1f: How big are the "low stature" plants? I don't expect them to have any noticeable amount of canopy interception, but due to the thin snow cover (maximum of 50 cm), it is likely that the vegetation is exposed for at least part of the winter. How does this impact the aerodynamic characteristics across the snow surface?

Response 15: Added "(<0.5 m)" to this line. Furthermore, we believe the impact is minimal. There are not large woody shrubs. It is primarily grasses, sedges, small deciduous shrubs, and some forbs.

Line 88: instead of "can top 20 m s-1," use "can exceed 20 m s-1"

Response 16: "can top" replaced with "can exceed".

Lines 92-94: consider adding citations to these three sentences

Response 17: Two of the sentences were removed in the revised version of the manuscript.

Section 2.1: This is not crucial, but a monthly summary of mean temperature, total precipitation, mean and maximum wind speeds for the two sites (tundra and boreal) would be informative so understand the climate of the study sites

Response 18: Thank you for the suggestion. We added a table with monthly summaries of wind speed, air temperature, and precipitation to Section 2.1 to provide a comparison of meteorological settings between the tundra and boreal forest sites. This new table is shown below.

Table 1: Monthly meteorological summaries for tundra and lowland boreal regions.

	Mean Daily Air Temperature (°C)		Mean Daily Wind Speed ($m\ s^{-1}$)		Max Daily Wind Speed ($m\ s^{-1}$)		Total Precipitation Normal[1] (mm)	
	Tundra	Lowland Boreal	Tundra	Lowland Boreal	Tundra	Lowland Boreal	Tundra	Lowland Boreal

January	-18	-20	2.6	0.9	22.7	12.6	9	15
February	-17	-16	2.9	1.1	20.1	8.3	13	13
March	-16	-10	2.6	1.3	15.9	8.8	9	10
April	-9	1	2.5	1.5	12.7	8.5	10	9
May	0	11	2.4	1.5	14.5	7.6	18	14
June	8	16	2.5	1.4	12.2	6.2	46	38
July	10	17	2.4	1.3	26.9	21.4	80	57
August	6	13	2.3	1.2	11.2	6.3	72	53
September	0	7	2.3	1.1	14.4	8.8	33	34
October	-7	-1	2.3	1.0	12.9	13.0	23	19
November	-15	-12	2.6	1.0	19.5	18.4	14	19
December	-18	-16	2.4	0.9	15.9	10.8	12	14
Annual	-6.3	-0.8	2.5	1.2	16.6	10.9	339	295

Line 130 and prior: "gap-filled data (30%)" – this is a lot. The methodology used to fill in the gaps is apparently presented in one of the four papers by Euskirchen et al. (2012, 2014, 2017, 2020). However, since 30% are gap-filled, the method used needs to be presented in the paper, at least in an Appendix or Supplementary Information.

Response 19: Additional information on data gaps and gap filling methodology were added to Section 3.1. See Response #2.

Line 145: are station pressure data not required?

Response 20: Correct, station pressure data are not utilized in this study.

Lines 161-162: why are "[s]tandard statistical methods … applied to evaluate the relationship between sublimation rates and meteorological and environmental variables?" To what end. Explain what was specifically done. I assume that this is to compute the % of total or winter precipitation lost to sublimation? If not, then why is this done?

Response 21: Section 3.3 has been re-written to better explain what was specifically done and why. See Response #3.

Line 165: No one cares that Arc was used to create the maps, since spatial data are not used in the analysis

Response 22: Sentence removed.

Line 170-171: what is meant by "mean rates are 5–7% of the maximum daily rate?"

Response 23: Clarified by adding that the "mean *daily* rates are 5-7% of the maximum daily rate.

Figure 4: needs the same legend that is in Figure 3, unless you combine those figures (not necessary).

Response 24: Legend added to Figure 4.

Consider adding 2010 to Figure 4b, even with no results, so that the reader can visually line up the year across the two study domains.

Response 25: We modified Figure 4b to include 2010.

Consider putting the gridlines between the years, instead of the mid-point, so we know which bar belong to which year when there are sites missing for a specific year (e.g., Figure 4a 2014, 2015, 2016 and Figure 4b 2014 and 2016.

Response 26: The Figure 4 gridlines were modified.

As stated above, it would be useful to show some annual or winter summary data, at least peak snow depth, peak SWE and winter precipitation totals

Response 27: We added additional summary data to Section 2.1. See Response#18.

Lines 194-195: this sentence is part of the Methods and should be moved there

Response 28: The sentence was moved to Methods Section 3.3.

Lines 204-205 and the next sentence: how were the "winter solid precipitation increase[s] at the lowland boreal sites (p value = 0.02 and r2 = 0.39)" computed? Explain the method used

Response 29: We clarified this sentence by adding "as calculated by OLS regression". OLS regression has also been described as part of the Section 3.3 Statistical Methods (Response #3).

Table 2: the standard deviation (SD) seems large compared to the mean sublimation and % of solid precipitation sublimated. Are these distributions skewed, i.e., are the SD values misleading?

Response 30: We ran Shapiro Wilks tests on the mean sublimation and solid precipitation data at the tundra and lowland boreal sites. Results all came back to accept the null hypothesis that the data is normally distributed, or suggests there is simply not enough evidence to reject the null. We put a footnote in the table with Shapiro-Wilks test results and also provide the range of the data in addition to the standard deviation.

Table 3: consider adding a sentence that the condensation is minimal compared to ET, but that deposition (downward sublimation flux) is not minimal compared to sublimation (away).

Response 31: Added "The relative importance of the downward fluxes varies, as condensation is minimal compared to ET (2% or less) while deposition is 15–20% of sublimation." as the last sentence in Section 4.2.2.

Figure 5: consider adding what the other components of the box and whisker chart are, beyond the mean (red dot)

Response 32: Added the other components to the Figure 5 caption: “Red dots represents the mean, boxes enclose the 1st and 3rd quartiles, horizontal line within the box is the median, whiskers denote the minimum value below the closest quartile ± 1.5 x interquartile range, and points outside the whisker are outliers.”

Table 4: From the text, these are correlations to daily sublimation. Add this to the caption.

Response 33: Added to the caption.

Table 4 “temperature gradient”: needs to be explained better, as this is the gradient from the sensor through the air and the snowpack to the soil interface.

Response 34: Temperature gradient explanation added to the Methods in Section 3.3 and elaborated further in the results and discussion.

Citation: https://doi.org/10.5194/tc-2023-153-RC1

Response 35: Thank you again for taking time and providing comments on this sublimation manuscript.