Review of “Uncertainty in computed turbulent heat fluxes over seasonal snow arising from roughness length and similarity theory assumptions”

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The authors use multi-year measurement series from twelve eddy-covariance stations across the Alps to investigate the performance of different methods to estimate the roughness lengths for momentum and scalars over snow. They further assess the performance of bulk approaches to determine near-surface turbulent heat fluxes and combinations with roughness length estimates. The manuscript is interesting, well written, and provides new insights that are much needed. The methodology is very clearly presented and sufficient for the results. The analysis is good, but I have a few suggestions for improvement before publishing the manuscript (see my comments below).
In case of any questions, do not hesitate to contact me.

Main Comments

1. The scatter plot arrays in Figs. 5-8 contain a lot of information. Individual subplots are too small, so it is very hard to decipher features like error bars. In the manuscript, only a part of the information contained in Figs. 5-8 is discussed. Consider an alternative using a concise plot, such as aggregating data into few subplots, show them in the manuscript, put Figs. 5-8 into the appendix and discuss them more in detail.

2. The discussion would profit from a more in-depth comparison with other studies either using the same methodology or covering similar land surface features, for example (but not exclusively) the studies mentioned in the reviews Mott et al. (2018) and Sauter et al. (2026).

3. Roughness length is the basic concept of the study. It is introduced subtly in l. 51f. As the authors mention in their last sentence (”limited robustness of

the roughness length concept itself" in lines 736f.), I suggest adding a discussion of the different concepts and the ambiguity (geometrical, determined from eddy-covariance measurements, used as model tuning parameter) of the roughness length in the introduction and/or discussion.

4. In lines 250-280 the authors describe the filtering of the flux data in detail. I am not entirely sure, how all of your filtering steps play together: for example, you filter out blowing snow events (in step 5) and later, in step 7, you only look at data with a melting snow surface ($T_S = 0°C$) which does not allow blowing snow events, anyways. Furthermore, in step 1, you filter for near-neutral atmospheric stability, and in step 6, you require the temperature difference (as a measure for the static stability) between the surface and an air temperature measurement at some height $|\Delta T| \geq 1$K, which, in principle, is counteracting. Could you consider discussing these points? It might also be helpful to include a plot that visualizes which periods are rejected by each filtering step for a station in the appendix to see if some filtering steps are redundant. Please also clearly state when and why you use different filtering for the results (I guess, for example, for error propagation in section 4.2)

5. Since turbulent heat fluxes is one of the basic datasets of the study, I suggest adding a few sentences about the processing of the data beyond mentioning the reference publications in section 3.1. What averaging times were used and were they the same for all stations and times? How were the sloped data rotated, what are *vertical* fluxes? Did you apply double rotation to account for surface changes of snow-covered surfaces or planar fit? How is the instrument height measured in sloped terrain?

Specific Comments

1. Title: The title does not precisely describe the complete scope of the manuscript, but rather only a part. Please consider adapting it.

2. l. 26: Consider citing Schwat et al. (2025) and Hogan et al. (2026) as references analysing the SOS dataset, and the review of Mott et al. (2018).

3. l. 27: include Stiperski and Rotach (2016)

4. l. 47: please include the recent review Sauter et al. (2026)

5. l. 58 ff.: repetition from l. 37

6. l. 105 list of symbols: You introduce the symbols later for every equation anyways. Consider removing the list.

7. l. 124 f., equations (2) and (3): Please briefly mention somewhere your sign notation for turbulent fluxes. Also make sure that it is consistent. In the scatter

plots in Fig. 6, it looks like unstable conditions and positive observed sensible heat fluxes coincide, which would be inconsistent with the sign convention in (2) and (7).

8. l. 159, equations (10)-(12): Please add a reference to equations (4)-(6)
9. l. 182: Why do you introduce the abbreviations? And why do the i-Box abbreviations not contain "AT" at the beginning? I would suggest sticking to the station locations in the following, as you mostly already do, and remove the abbreviations.
10. l. 183-185: Please mention that the sensor heights are measured above bare ground (I guess so).
11. l. 225 ff.: The consistency check with the SNOWPACK simulations is a bit hard to justify, as you mention yourself. Driving the simulation with the given surface temperatures can only prove your assumption, right? Maybe you add more detail on your reasoning, or you could also leave that paragraph out.
12. l. 235: Why don't you include the $\overline{u'w'}$ fluxes into the calculation of the friction velocity? And, if the data is already rotated, why should the vertical fluxes still be included?
13. l. 239 f., equations (14) and (15): relate to equations (1)-(3) and the values for L_V are already introduced in l. 126.
14. l. 247 ff., equations (16)-(18): this is trivial from equations (10)-(12). Consider removing the equations here.
15. . l. 259 f.: Why do you restrict the wind direction? Can you explain that a bit more in detail?
16. l. 387 ff.: I suggest moving the less restrictive filtering to the results section so it is easier to remember. Please also add a brief reasoning on why you use a different filtering there.
17. l. 386-397: Can you add a brief explanation on why you use a Monte-Carlo method in the second step, but simple error-propagation in the first step?
18. l. 416: Can you add an introductory sentence on why you look at $log_{10}(z_0)$ in the following? It might also help to add some real z_0 values in meter for comparison in the text besides table 3.
19. l. 431: please cite Kljun et al. (2015)
20. Figure 2: the unit of $log_{10}(z_0)$ is not meters. Formally, z_0 would have to be non-dimensionalized before taking the log. Same for Figure B2.
21. Figure 3: the footprint percentages are very hard to read and the different lines for smaller percentages are hard to distinguish. Same for Figs. B1 and C1

22. l. 441 ff. and Figure 4b: Maybe I missed it, but why are there no fits for "Level 1" in figure 4b?

23. l. 475: Over what is the average for the roughness lengths in table 5? With which method are the roughness lengths calculated?

24. l. 480 ff.: Consider only reporting the meter-values, not the log_{10} values

25. Table 5: Same as for Figure 2: You mention in the caption the log(m) unit, but the column headings say "m". Consider non-dimensionalizing the $log_{10}(z_0)$ values. Please also consider adding ratios between the momentum and scalar roughness lengths into the table and briefly discuss them.

26. l. 528: "... during stable conditions (blue in Figs. 5-8), ..."

27. Figure 5: (see main comments); "... (rows within each block, label on the right) ..."; please also add the ξ-criterion for unstable and stable in the caption; You could add the slopes of each station in the title of the subplots

28. l. 587 ff.: You could quantify the contribution of forest to the footprint using the normalized footprint values given by the Kljun et al. (2015) method.

29. l. 589 f.: You state in l. 171 f. that "trees [...] introduce additional and difficult-to-quantify uncertainties related to displacement height and snow interception." And here, you find no "systematically higher z_0 values". Can you discuss the influence a bit more in detail, or at least reference your statement from earlier that you would not expect such an easy dependence?

30. l. 601: sensitive to sensor height relative to the local vertical flow structure

31. l. 602 ff.: This paragraph is not consistent regarding the effect of slopes: "roughness lengths [...] should be interpreted with caution, particularly on slopes." and "limited reliability not only on slopes".

32. l. 619: avoid usage of non-quantifying words like "substantial" or "considerable" throughout the manuscript

33. l. 632: Should you start a new subsection here?

34. l. 634 ff.: But you also found these scales for a continuous snow cover (i.e. in Table 3), which eliminates the effect of underlying grassland or pasture, no?

35. l. 649 ff.: Could you add a brief comment on why the more complex C_{Rib} approach is still used in models, even if it is outperformed for experimental data by C_{log}?

36. l. 654: It would be easier to understand, if you explained, what the "first z_0 parameterization" is.

37. l. 655 f.: The stability corrections COULD explain the increased deviations. However, for near-neutral conditions, the corrections are also small.

38. l. 657 ff.: Can you discuss more in-depth the performance of the different approaches that you introduced in l. 76 ff.? Do you have a better suggestion based on your results?

39. l. 662: Please add citations for the fixed ratios used in snow models.

40. l. 676 ff.: I think it is important to note here, which data is available in models (i.e. no EC data to estimate z_0) and why there is still the need for stability corrections in models. Also, could you detail on how the citations support your statement?

41. l. 681: Fig. 9?

42. l. 738, Data and code availability: I would strongly suggest to publish the code and documentation on where to find the data on an accessible repository in alignment with the Data policy of The Cryosphere (`https://www.the-cryosphere.net/policies/data_policy.html`).

43. l. 740 ff.: It would help to add a bit more context in the text of Appendix A to understand Figs. A1-A3.

44. Figure A2 and A3: Please add a note in the caption that the color bar is log-scaled.

45. l. 744 ff., Appendix B: Could you include obtained z_0 values in meter in the text for comparison.

References

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