

The article compares remote-sensing model outputs with in-situ measurements of snow permittivity and density at Kingman Farm, New Hampshire, USA, during the spring of 2025. The article is well put together and easy to follow, even for someone who is not an expert in remote sensing. The methods and site description are clear and cover the scope of the study. The output of the different models is presented well, and the comparison between them is easy to understand. The introduction could be improved by adding a brief section explaining the key physical variables that affect snow density.

I have a few questions/comments about the article:

1) Do the results follow a normal distribution? Is presenting the mean a good indicator of the results, or would the median represent the results more adequately?

2) In lines 370-375, there is a mention of forest canopy and wind effects on snow formation. What is the dominant wind direction at the site, and has it affected the monitored snow formations? In addition, has the local topography of the monitored areas affected the distribution of snow within them?

3) Based on Figure 6 (a), should a different kind of model be used for open fields?

4) The research questions stated at the end of the Introduction and the answers to them are unclear; the conclusion needs more clarity on what was achieved with the research.

5) The comparison between OP and SF in situ measurement results should be more clearly discussed since there are clear differences in the snow formations on the different monitoring areas, and how does that difference become visible in the modeled values.

I suggest a minor revision of the manuscript and acceptance when these issues have been addressed.

Disclaimer; I am not a LiDAR or UAS specialist. My expertise lies in geoscience of snow and ice, which is the perspective in which I evaluate this paper. Limitations in my knowledge may influence comments on this paper. Particularly, sections 3.2.2-3.3 are hard for me to comment on.

Summary;

This study attempts to couple GRP/LiDAR on a UAS to evaluate models for permissivity to assess snow density during transitional seasons. The work's purpose is promote an alternative pathway for hydrological characterization in complex environments. I recommend this paper for reconsideration upon moderate revisions.

Major.comments;

- If some data must remain unpublished for permission reasons, it must be very explicit what is not available and why. Information that can be public must also be stated very clearly. A disclaimer probably isn't enough.
- Abstract, line 16. Description of methodology could be more precise. This continues throughout the paper. It could be beneficial to include tables or other step-by-step descriptions.
- Section 3.1. The repeatability of this study is concerning in that the test setup is not transparent. For a study on model evaluation, the parameters of the models are not clarified. The balance of privacy has not been made.
- The metrics of evaluation are potentially unideal. I suggest including justification of why the metrics were chosen based on the applicability of this paper to the literature.

Minor.comments;

- Color figures would be more accessible if they are translatable to black and white. Use of color should be specified in the figure description to indicate its use if the paper is viewed without color.
- The justification of the work is stated in general terms, but it may be helpful to give a specific use case. What does the work contribute to the literature? What gap does it fill?
- Similarly, the research questions are not explicitly clear. A list or paragraph specifically about the questions may be more clear.

- Line 152. “TWT” is not defined until line 157. Define it here.
- Data is shown in figures, but no tables. I suggest including alternative visuals of data, like a list of numerical results. Differences between figures it not always clear.

Disclaimer: I have only limited knowledge of snowpack characteristics and the processes involved, as well as of radar measurements. I therefore cannot fully assess the relevance of the paper within its specific field. Some of my comments below may result from my lack of understanding rather than from shortcomings in the paper. Nevertheless, I completed the review for the sake of the exercise.

Review of Technical note: Comparison of Modeled Snow Permittivity and Density Estimates of Shallow Wet Snowpacks using UAS_LiDAR and UAS_GPR Observations by Kang et al.

Knowledge of snowpack characteristics (depth, density, and water content) and their spatio-temporal variability is important for understanding the water cycle. The behavior of the relative permittivity of snow is poorly understood for shallow and wet snowpacks. Using reference values from LiDAR and GPR observations and in situ measurements, the authors assess the suitability of six empirical permittivity models and one physically based model.

I think the paper is interesting and addresses an important challenge. It is well written and easy to follow. The findings regarding the suitability of the CRIM model could be important. However, I have a few comments and questions that should be addressed before publication. Overall, I think the manuscript lacks clearly stated objectives, does not sufficiently place the results in the context of the field, and does not clearly explain what has been learned. I recommend major revisions.

Major comments:

1. The precise novelty of the work is unclear. Does it lie in the question addressed (whether empirical and physically based models can provide reliable estimates of permittivity), the models tested, particularly CRIM, the snow conditions under which the study was conducted, or a combination of these elements? The paper would benefit from placing greater emphasis on its novelty and on what the study reveals that was not previously known. Some of the following comments may stem from my difficulty in identifying the scope of the paper.
2. At line 72, you suggest that one potential drawback of empirical models is their limited transferability across climatic conditions and melt stages. It is unclear how this limitation affects the present study or what the study reveals about this limitation. If it does not affect your work or your conclusions, please explain why. If it does, please discuss its potential influence on your results. If your findings provide insights into this transferability issue, please state them clearly.
3. The results shown in Figure 6 are discussed mainly qualitatively, with quantitative information provided only for an outlier. Could you provide a more quantitative

evaluation using appropriate metrics and discuss whether the resulting performance is satisfactory relative to that typically achieved by permittivity models?

4. Figure 5 suggests that, except for the Webb model, the models perform broadly similarly and show similar patterns in the scatterplots. For example, a few outliers appear to reduce the performance of the Kendra and Sihvola models. Have you identified these observations and assessed the extent to which they affect your conclusions?

5. Building on comment 4, I also wonder to what extent measurement uncertainty could affect your conclusions. For instance, could some of the outlying predictions identified above result from measurement errors rather than model inadequacy?

6. I wonder whether agreement among the models could provide insight into the conditions under which each model is suitable. Did you assess where and when their predictions differed? Is there literature that could be used to discuss this point?

Minor comments:

1. At line 14, LiDAR has not previously been defined.

2. At line 125, is this standard practice in the field?

3. I found Figure 4 confusing. First, the x-axis of the third panel is labeled “Snow Wetness,” whereas the text refers to “Liquid Water Content,” which I assumed represented the same quantity. Please clarify the distinction between these terms, if any, or use consistent terminology throughout. The readability of the figure could also be improved by labeling the panels, for example, (a), (b), and (c). Second, some dielectric-permittivity values shown in the first and second panels, particularly the blue point at approximately 4, do not appear in the third panel. Was the corresponding snow wetness not measured? If so, this does not appear to have been indicated in the description of the in situ measurements in the Methods section. If I missed this information, please consider reiterating it when discussing Figure 4.

4. The inclusion of detailed numerical results in the Summary section was somewhat unexpected. Please consider summarizing these results more concisely or presenting the detailed values in the Results section.

Disclaimer: I am not familiar with snow hydrology methods and literature; I can give feedback on

reporting of results and the comparison of the models but not on methodological details and the

positioning within snow permittivity and density literature;

This manuscript compares snow density and permittivity estimates modeled with the physically

based CRIM model and six empirical models to UAS-based LiDAR- and GPR-derived permittivity

measurements in shallow wet snow fields in the eastern United States. The authors demonstrate that

CRIM and two empirical models are in strong agreement with the LiDAR and GPR measurements,

concluding that such model-based methods are promising for characterizing bulk snow properties in

complex environments.

The study addresses a methodological issue that seems important to a part of the hydrological

community. The manuscript is well-written. The presented method addresses several apparently

critical problems in snow hydrology. Despite the merits of this research, I see a few major and some

minor issues that need to be addressed before publication. After revision the study can be a valuable

contribution to HESS.

General comments

Ambiguous main contribution

The manuscript could be strengthened by being more clear and consistent about what the authors

see as the primary contribution of this technical note. Title, introduction, method and conclusions

seem to answer that question differently, and as a reader I found it difficult to understand the main

contribution at first sight:

- The title (l. 1-3) says it is a comparison of modeled permittivity and density that somehow

uses the input from the UAS platform. Emphasis is clearly on the model comparison

- The abstract suggests this study is primarily about assessing the applicability of LiDAR and

GPR on the UAS platform in the transitional snow environment (l. 13-15)

- The introduction suggests that the study is about investigating feasibility of the UAS platform

to acquire spatially distributed snowpack depth data and derive permittivity in shallow, wet

snow conditions (l. 87)

- The conclusion (l. 381-383) sounds like the main contribution is in the evaluation of dielectric

characteristics of the shallow, wet snowpacks using the novel UAS platform.

It may not be clear to the reader whether the study is primarily about comparing models with

each other and with UAS-measured data (or the assessment of models supported by the UAS data),

about investigating the applicability of the UAS-based platform for measurements in shallow wet

snow packs, about the application of CRIM for the studied snow conditions, or about the dataset

itself. This should be made clear, rather than having the reader guessing.

1

Incomplete reporting of the method

Especially for a technical note it is important that the method is fully reproducible.

Where data

and code cannot immediately be provided completely, it should be possible to reproduce the method

from the manuscript. At some points, however, the manuscript leaves critical questions on method

open, e.g., by providing incomplete descriptions of algorithms or omitting key parameters in the

data processing steps.

Some examples:

- l. 141 Which interpolation algorithm? Parameters?
- l. 149 Background removal and adaptive algorithms? Parameters?
- l. 150 Which deconvolution algorithm? Parameters?
- l. 150 Which semi-automated picking algorithm? Parameters?
- l. 199 Progressive TIN Densification parameters? Grid size is reported, but is this sufficient

for full reproduction?

Model comparison issues

The model comparison is based on a simple correlation analysis (r and RMSE). This leaves some

important questions open and is not sufficient, given the structure of the data. Several issues should

be addressed:

- The analysis seems to implicitly put more weight on the OP site (with 20 observation points)

than on the SF site (with 10 observation points)

- Spatial autocorrelation between the points from one snow field during a campaign can lead to

correlated errors for those points. The residuals and measurement errors are not independent

- The evaluation of goodness of fit relies on r and RMSE alone. The metrics both do not separate

bias from random error. Reporting and discussing further metrics could be interesting, for

instance to adequately evaluate bias. E.g., mean error, PBIAS, to see, e.g., whether a model is

systematically over-/underpredicting

- There is no breakdown by campaign/date and site/snow field. The sites were chosen for their

contrasting conditions (l. 101) and it would be interesting to see if this contrast translates to

differences in model performance.

- There is an unexploited hierarchical structure: campaign -> site/snow field -> point. Addressing

this can also resolve the pseudoreplication issue

The manuscript could benefit from a more sophisticated approach for model comparison (such as regression).

Are the sites really contrasting?

The two sites are described as “contrasting” (l. 101), which motivated the site selection. Not an

expert on snowpacks, I was wondering whether the contrast is sufficiently pronounced. The sites are

both managed fields on the same research farm in spatial proximity to each other. Both are south of

a forest. There is of course a relative contrast (one being more sheltered, the other more open), but

I have my reservations about calling the site conditions ‘contrasting’.

2

Minor comments

l. 13 »However?characterizing.its.spatiotemporal.variability.is.challenging?particularly.in.the.shallow?

ephemeral.snowpacks.of.the.eastern.United.States« .It is not clear why this is particular to the eastern

United States

l. 34 ff. »In.recent.years?understanding.and.modeling.hydrological.processes.has.
increasingly.required

observations.of.snow.conditions.across.space.and.time.(Jennings.et.al;?867④.Robinson.
et.al;?7⑤⑤9)«

1993 is not recent and even 2018 is 8 years ago. It is further not clear how observations
of snow

conditions used to be required less for understanding.hydrological processes across
space and time in

the past.

l. 97 »The.study.area.encompasses.approximately.9②6.acres.of.mixed.agricultural.and.
forested.areas« .

Use metric/SI units [https://www.hydrology-and-earth-system-
sciences.net/submission.html](https://www.hydrology-and-earth-system-sciences.net/submission.html)

l. 125 “the average value was used”. This is colloquial and unspecific. Do you mean the
arithmetic

mean?

l. 125 states that the average LWC from three Snow Fork-based measurements was
used. Averaging

over triplicates is standard practice, but I am missing reporting of the variance/standard
deviation,

which could help estimate measurement uncertainty

l. 142: cite relevant regulations

l. 190: acronym (GCPs) introduced and then used exactly once in the same line. No
added value

Fig. 5 “R” refers to Pearson’s correlation coefficient “r”, and not to the coefficient of
determination

“R²”, right? The notation with capital R (not squared) is not ideal

Not an expert in this field

The technical note presents interesting and relevant work where the authors used an integrated approach of UAS-LiDar and UAS-GPR observations to appraise different dielectric permittivity models to retrieve snow densities. I believe that readers working in this area or related fields will find this approach useful. Overall, I find that the manuscript still requires more careful attention to detail, particularly to editorial and technical consistency. Specific comments are as follows:

Title

- The title seems okay in first instance but when you read the abstract, the overall aim of the study is broader, and the title is more specific which should probably be the opposite.

Abstract

- The abstract reads well. There appears to be a mismatch between the study's objective and corresponding methodology. The described methodology derives snow permittivity from UAS-GPR and LiDAR data and compares it with dielectric model estimates. The authors should clarify how densities were retrieved and this appears to be a key focus of the study.
- Line 21 not clear which field observations
- Line 26: Only one physical model was evaluated instead of physically-based models stated.

Introduction

- Line 34: The sentence begins with "In recent years" but goes on to cite a 1993 reference
- Line 43: This is possibly a broader statement without either citing these traditional in situ methods or stating them
- Line 49-51: Grammatical issue - "enabling combine with other dataset"
- Line 80: The authors could add "To the best of our knowledge" so that the claim is properly qualified.
- The introduction extensively focuses on CRIM whilst the actual study evaluates six other models. Is there any reason for this preference?

- The specific objectives are very clear and align to the overall theme and objective of this work .

Study Area

- Line 101: Is this another additional specific objective. I propose the authors state their objective clearly as in the introduction and not add other micro objectives throughout the manuscript.
- Line 108: I could not locate Fig S1. Is there a supplementary document that I might be missing?

Materials and Methods

3.1 In-situ measurements

- This section is well written and very clear about the in-situ measurements that were conducted, along with the instrumentation used.

3.2 Uncrewed Aerial System (UAS) observations

3.2.1 Ground Penetrating Radar (GPR) measurements and processing

- This section is well written and clear.

3.2.2 LiDAR measurements and processing

- No issues, well written and clear.

3.3 Permittivity models

- Line 204: “To quantitatively evaluate the performance of the UAS-GPR system” not clear if this is a new specific objective and how it aligns with prior ones. Its important that the authors not introduce other objectives in the materials and methods section.
- Line 209-211: Same issue as above. Now this highlights that “empirical models were assessed together with the physically based CRIM to determine the suitability of existing parameterizations for characterizing shallow, wet snowpacks. Whereas in the previous sentence the objective was “To quantitatively evaluate the performance of the UAS-GPR system”

- • Line 227-229: Same issue as above. Now this indicates that “The CRIM is implemented in this study as a physically based dielectric mixing model for estimating the effective permittivity of snow”
- • Line 245: Same issue as above. “Accordingly, CRIM is adopted here as the primary theoretical model for linking GPR derived permittivity with snow density and LWC”. It is not clear why CRIM is now the primary model. The objective of the study as stated in the abstract “This study evaluates the potential of integrating Ground Penetrating Radar (GPR) and LiDAR onboard an Uncrewed Aerial System (UAS) to assess the applicability of dielectric permittivity models for density retrieval in a transitional melt environment”. Its not clear how and which dielectric permittivity models are used for density retrieval. In Line 205, its stated that “six dielectric permittivity models using in situ measurements (snow density and LWC)” which makes it unclear how densities were retrieved using the models as stated in the objective.
- • Overall, this section could be revised to clearly present the use of the models and how they were implemented.

4 Results and Discussion

4.1 Field observations of snow depth, density, and wetness

- • Line 250: Repetition issue: In the study area section, this has been discussed.
- • Because the study uses both in-situ measurements and UAS-based surveys, it is not immediately clear which field-based observations are being referred to.
- • The captions of Figures 2 and 3 do not immediately make it clear that these are results from in-situ measurements. Although this is noted in the text, it would be better to clearly indicate in the captions that these are results from in situ measurements.
- • Overall, I think the section clearly addresses the first objective on characterising the spatiotemporal variability of snow depth, density, and LWC based on in situ measurements.

4.2 Relationships between permittivity and snow properties

- • No issue: Sufficient to support the objective on assessing the relationship between GPR-derived permittivity and in situ snow properties

4.3 Comparison of Permittivity Models

- Line 350-352: To examine whether our observed combinations of permittivity, density, and liquid water content reflect physically reasonable snow wetness states, we compared the field measurements with CRIM-modeled curves for snow densities ranging from 0.1 to 0.7 g/cm³. Similar issue of introducing another specific objective and highlighting a methodology in the results and discussion section.
- This section seem to attempt to address the specific objective on “evaluating the applicability of empirical and physical models for retrieving snow density in shallow, wet seasonal snowpacks. However, empirical methods are not assessed in this section and only the CRIM is used. It is also not clear how these CRIM modeled curves were derived as this is not clearly stated in the methods section. It seems here the comparison is between field measurements with CRIM-modeled curves, do the field measurements here relate to in situ measurements? Why is this now the issue when the objective is integrating Ground Penetrating Radar (GPR) and LiDAR onboard an Uncrewed Aerial System (UAS) to assess the applicability of dielectric permittivity models for density retrieval in a transitional melt environment.
- Line 38: Figure S2 , I don't seem have this figure.

5 Summary and Conclusion

- Line 381-383: This sentence does not clearly indicate what the study set out to evaluate. Here it attributed the study as using co-located GPR TWT and the LiDAR-derived snow depth observations to evaluate the dielectric characteristics of shallow, wet snowpacks. According to the objectives and methodology presented, this is clearly not the case.
- Line 383-391: The statement indicates that there was snow density model comparison. This seems to be a misrepresentation as the study only compared modeled permittivity from the dielectric models against GPR-derived permittivity.
- It is good that the authors indicate the limitations and priority areas for future studies.

References

- Check the formatting of the reference list for consistency. For example, the Ajo-Franklin et al. (2004) reference includes a DOI, whereas the reference immediately below it does not.