Responses to reviews by the Editor and Reviewers

Thank you for these comments and recommendations. Our responses are in blue italics.

Editor's review

Please address comments from the second round of reviews. I'll note that both reviewers ranked the scientific rigour and presentation quality of the paper as either fair or poor - indicating room for improvement in the manuscript that I encourage the authors to seriously consider.

The red sensor needs better justification of inclusion in the abstract and introduction, because this is really a paper about irradiance profiles in snow, and effective absorption by branches, but the presented motivation is photochemical reactions - which do not occur in the wavelengths for the 'red' sensor included in this study.

Indeed, red radiation is not relevant to photochemistry. We had briefly mentioned metamorphism as a possible application for irradiance profiles (line 43). While our primary focus remains on photochemistry, we have developed the metamorphism aspect a bit more. Studying red radiation is of potential interest for metamorphism, as explained in the detailed responses to your comments.

The Abstract

You focus it around photochemical reaction but, again, it is known that this isn't relevant for the longer 'red' sensor - so clearly state what motivated included a red sensor. Move up the description that photochemical reactions are triggered by solar radiation in the UV and blue wavelengths, but also state that ice is more absorptive in the red and makes for an interesting comparison. Use this to set up the chosen study wavelengths, and be descriptive, i.e. "Here we monitored irradiance at blue (390±125 nm) and red (715 - 1000 nm, effective 760 nm) wavelengths..." And then you can use red and blue in the abstract for clarity rather then different wavelength descriptors for red: "In the blue wavelengths, dense shrub branches were found to reduce irradiance similarly to about 140 ppb of soot. For the red wavelength, insufficient data and the greater ice absorption do not allow accurate conclusions."

Thank you for these suggestions. We now mention metamorphism as another possible application in the abstract, for which spectral information is relevant. When mentioning photochemistry, we only refer to the 390 nm data. The 760 nm data is useful for quantifying energy absorption, which is relevant to metamorphism. We have restructured the abstract around both these topics so that the use of the 760 nm wavelength hopefully appears useful.

You also need to set up inclusion of the red sensor in the introduction better, it's not until the 3rd paragraph that mention red wavelengths are not relevant for photochemistry - but this paper is about measure blue and red irradiance profiles - so... why?

As mentioned in the abstract, irradiance profiles affect the radiative energy absorption in the snowpack, and hence metamorphism. We now briefly develop this, lines 52-54. We then discuss that radiation absorption in snow is highly wavelength-dependent and that therefore, for metamorphism-related applications, the spectral dependence of shrubs impact must also be studied (lines 73-77).

Please report sensor heights in cm (to align Figure 6) - at the scale of tens of cm, using cm is more suitable

This has been changed throughout. mm are not used anymore for sensor heights.

Figure 6 could be improved for clarity and interpretation - it is busy and using red/orange and green as primary colors on a plot makes challenging for those with the most common form of color blindness to interpret the plot, please update. Don't indicate fieldwork timing that was not included in the study, it is irrelevant. Consider moving the sensor depths on the secondary axis and not overlaid on plot.

Indeed, Figure 6 was busy. We have split it into 2 panels for clarity and changed the colors so that it is color blind-friendly. We have also diversified symbols to facilitate curve identification. The caption has been modified accordingly. We have removed all mentions of the January 6th field work, since the data were not used.

Results

Please include an overview paragraph at the start of the Section Results summarizing the relevant findings and outline how the results will be presented to help guide readers about the results sections. For example, right now it's not clear why snow properties from February 23rd are presented first - there is no context.

We have added an introductory paragraph to explain to the reader which data are presented and the general strategy, lines 233-238. This should make it easier for the reader to follow the structure of the data presentation. We have also made additional modifications later in the text to specify context. For example, line 240, we mention than snow field measurements were performed on three dates, but that we are only showing data from one date in the main text. We have also added context to the caption of Figure 5.

Section 3.2 is not a meaningful results presentation - its primarily justifying conditions and discussing methods. This is basically the focus of the measurement methods - don't assume readers will interpret plots as you expect - what you would you like readers to take away? Please summarize and state results clearly.

Section 3.2 is really about showing irradiance data, as evidenced by the presentation of Figures 7, 8 and 9, which all show such data. There is indeed some justification in the text, but this is in fact explanations of the figures and of the data selection process. This text requires the prior presentation of these data, and therefore these aspects could not be developed in the Methods section. What could be mentioned in Methods was in fact mentioned there, such as the choice of overcast days for simulations. Here we show the actual strategy with data. In fact this was partly in response to the reviewers' earlier request for explanations. Reviewer 1 did not seem to understand how the CNR4 data were used to select overcast days. In any case, we have added a paragraph for more context and clarity, lines 261-267. We have also modified all the Figures of this section to make them color blind-friendly. Moreover, we have modified Figure 9 to show that at 760 nm light transmission is much shallower. The message is that radiation attenuation is much greater in shrubs (Fig. 7a vs 7b and 8ab vs 8cd) and that attenuation is much greater at 760 than at 390 nm (Fig 7 vs Fig 9). This is stressed line 283 "As expected, irradiance signals are lower at SHRUB than at FIELD because of light absorption by shrub branches.", and lines 293-294 "Because of the greater ice absorption at 760 nm than at 390 nm (Warren and Brandt, 2008), radiation penetration is much shallower at 760 than at 390 nm and a signal was detected only for the topmost sensor at the red wavelength.". The actual take-home messages of the paper are in fact in the discussion, the main one being that photochemical rates are divided by 2 in shrubs.

You didn't measure black carbon in snow but it's persistent and variable through both space and time - how would that impacts the analysis?

Indeed, soot is persistent because it is not chemically reactive and it varies with snow layers because of variations in air mass composition. The snow composition reflects that of the air mass where it formed. However, in a given snow layer, soot is not expected to vary over time unless intense melting occurs. We stress this, lines 322-324 "However, the soot concentration of a given layer is not expected to vary significantly over time because soot is not significantly affected by snow metamorphism and by snow chemistry, and soot particles are hydrophobic and little affected by melting events (Festi et al., 2021; Meyer and Wania, 2011)." Our analysis respects this by adjusting soot concentrations for each layer, and then maintaining those concentrations constant over time. For the April 1st simulations, maintaining soot values constant yields unsatisfactory simulations, as mentioned lines 330-331 "Using lower soot values on that last date [April 1st] would allow a perfect fit, but decreasing soot values during melt would not make physical sense.", and visible in Fig. 10, bottom right panel. We mention line 330 that "We reflect on this situation in the discussion.". Subsequently, in the discussion, section 4.1 line 370 explains that this is probably due to the formation of percolation channels, so that simulations using a plane-parallel geometry cannot reproduce the data.

Lastly, since we stress metamorphism as an extra field of application of our data in the abstract and introduction, we also added section 4.6 in the discussion to review the implications of our results for this subject. We also briefly mention metamorphism in the conclusion.

Report 1 by Reviewer 2

Thank the authors to resolve my comments. I just have few comments for the authors' consideration as below:

1. The authors replied that an image analysis software which determined the snow level on the striped poles was used to determine snow height from photographs taken by a time-lapse camera. Please provide the software name and the associated citation.

We now mention line 166 that "The image analysis software Fiji (https://imagej.net/software/fiji/ last accessed on 27 February 2025) was used to determine the snow level on the striped poles"

2. Figure 3 caption: PWR; Power -> PWR: Power

Thank you, changed.

3. Figure 1 caption: Fig. 1 to Figure. 1.

Thank you, changed

4. The current figure quality need to be improved in terms of numbering, color and styles.

As detailed in the response to the Editor's comments, Figures 6, 7, 8, 9 and 11 were modified.

5. There are too many very small tables with just 1 line data, which can be improved

We are surprised by this comment. Should a Table necessarily have many lines? Table 2 is a full page long, because it needs to. Table 5 just has one line of data, because that is all that needs to be shown. The small Tables are all different and cannot be regrouped.

Report 2 by Reviewer 1

The authors have made only minor modifications to improve their manuscript. However, significant work remains before the manuscript can be considered suitable for acceptance.

As I and another reviewer have recommended, Section 3 is overly simplistic and requires more detailed information about the data and figures. The authors' response, stating that they "want to keep this simple and concise," is not a valid justification. If simplicity is the goal, they could simply list the figures and tables without any explanatory text within their manuscript. However, without sufficient description and interpretation of the data and results, the manuscript's quality does not meet the standards required for publication in The Cryosphere.

Thank you, but this comment is not very helpful for lack of specificity. Addressing this comment would require constructive details or suggestions, without which no useful modification can be

made. The Editor has made some specific, clear and useful comments that we have done our best to address. We hope the resulting changes will satisfy the reviewer.

Additionally, the term “snow depth” is the correct and professional terminology in this field. The authors MUST replace all instances of “snow height” with “snow depth”. As defined by the National Snow and Ice Data Center (NSIDC), snow depth refers to “the combined total depth of both old and new snow on the ground” (source: NSIDC Cryosphere Glossary). This correction is essential for accuracy and consistency with established scientific terminology.

Snow depth is a wonderful variable, when the snow surface is used as a reference. When we discuss radiation penetration, then of course the reference is the snow surface and we then use snow depth (e.g., lines 194, 212, 347, 389, 407, 416, 437). However, given that the sensors are at fixed height and certainly not at fixed depth, snow depth to discuss irradiance at the sensors' level is a totally useless variable in this case, and snow height obviously has to be used. We are shocked by the reviewer's insistence and especially by the inacceptable tone, which only shows the reviewer's ignorance of a large fraction of the snow literature, in particular the snow physics field, where snow height is often used for good reasons.

For the reviewer's information, if there is an internationally recognized snow terminology for snow physics work, it is "The International classification for seasonal snow on the ground" (Fierz et al., 2009), written by an international team of experts from many institutions, including Richard Armstrong from NSIDC, and not some glossary by one institution, which I have never seen, as there are anyway many such glossaries. (Fierz et al., 2009) mention the use of both snow height and snow depth, depending on the application. For snow pit work, snow height is often used, because the reference is the ground. Snow modelers also often use snow height to track a layer coordinate, rather than snow depth, for obvious reasons.

In summary, snow depth and snow height have to be used with judgement and not in a rigid and dogmatic manner. When we use snow height, it is because this is the sensible variable. We consider this discussion definitely closed.

Reference cited

Fierz, C., Armstrong, R. L., Durand, Y., Etchevers, P., Greene, E., McClung, D. M., Nishimura, K., Satyawali, P. K., and Sokratov, S. A.: The International classification for seasonal snow on the ground UNESCO-IHP, ParisIACS Contribution N°1, 80 pp., 2009.

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