Responses to Review 1

We thank the Reviewer for the time spent reading our paper and for giving us the opportunity to improve our text. In this review, we are often requested to repeat several times a number of statements. We believe that the concision requirements of scientific paper writing encourage writers to avoid repetitions. When repetitions are recommended by the reviewer, we therefore often choose not to follow such recommendations. Our responses are embedded in the Reviewer's comments, in blue italics. Line numbers refer to those in the tracked changes version.

This study investigates the impact of shrub branches on irradiance by monitoring light levels at specific wavelengths (390±125 nm and >715 nm) within snow-covered shrub areas and adjacent grassland throughout a winter season. Light sensors were deployed at fixed heights within the shrubs and on the grassland. While the research presents interesting findings relevant to snow-related studies, certain areas require improvement to enhance the overall quality of the manuscript.

Major comments:

Q1: The manuscript's Abstract and Introduction currently lack a clear articulation of the research objectives and significance. It's crucial to explicitly state the research gaps addressed in this study compared to previous work. The reader should readily understand the motivation behind this research. Please revise and improve these sections accordingly.

We apologize for this lack of clarity. Regarding the abstract, we indeed did not stress that no data was available to quantify the reduction of photochemical reaction rates caused by shrubs. We added a sentence line 24 stating that "No study is currently available to quantify the reduction in photochemical rates caused by shrubs buried in snow. Here we quantify this effect by monitoring irradiance…".

Regarding the Introduction, we attempted to first make general statements on the impact of snowpack photochemistry, then explain that the role of impurities had been well studied, but that the role of shrubs had received little attention. We then logically proceed with the explanation of our project, intended to fill the gap. We modified line 65 and explain that "To contribute to the understanding of shrub effects on irradiance profiles in snow, and to deduce the resulting impact on photochemical reaction rates in the snowpack and their potential consequences on atmospheric chemistry, we have monitored….". We hope that these modifications will clarify our objectives for readers.

Q2: While Section 3 presents numerous figures and tables, it lacks detailed descriptions and explanations for them. This makes it challenging for readers to understand the results. It's important to guide the readers through the findings and not leave them to guess the story behind the data. Please provide comprehensive descriptions and interpretations for all figures and tables.

Section 3 contains Figures 5 to 11 and Tables 1 to 2. Figure 5 shows vertical profiles of snow physical properties, which are basic snow data familiar to readers of The Cryosphere. Figure 6 shows time series of snow height which are also familiar to readers of The Cryosphere. The Figure is explained by a detailed and lengthy caption of 92 words. Figures 7 and 9 show time series of irradiance signals, explained by a caption of 85 words. In the caption we replaced "CNR4" with "CNR4 radiometer" to clarify the type of instrument. Figure 8 shows time series of irradiance signal during specific days. To clarify the use of these data, we added that "For the February 28th data, the time range 10:30 to 12:30 was used. For the March 6th data, the time range 7:00 to 16:00 was used, with the exclusion of the 11:30-13:00 time range." In the caption. Please note that this is already explained in the text lines 260-262, so we are unsure this addition is necessary. Figures 10 and 11 show the simulations of irradiance profiles in snow. The caption box shows "TARTES" which are simulation profiles using the TARTES software, as explained in the text, but which may not be clear to readers just looking at Figures. To clarify this, we rephrased the caption as follows "Profiles of irradiance in the snowpack at FIELD and SHRUB at 390 nm simulated by the TARTES software. Experimental data points are also shown. The scale…". Similar modifications to the caption of Figure 11 were done. Regarding Table 1, we modified the caption as follows "Canopy and branch characteristics at heights of 325, 485 and 650 mm heights, corresponding to the levels of the three sensors in SHRUB that were buried in snow". Regarding Table 2, we believe the caption adequately describes the Table, as all the variables listed are mentioned in the caption.

Q3: The study simulates the influence of shrub branches using a "soot-equivalent" approach. However, figures like Fig. 2, Fig. 5, and Fig. S1-S3 highlight the variability of snowpack properties. Deep snow and high specific surface area (SSA) can significantly impact irradiance. The current analysis doesn't seem to account for these snowpack properties, leading to potentially inaccurate simulation results. This is particularly evident in Section 3.4, where the lack of consideration for snowpack properties results in convoluted and confusing explanations. That means your conclusion in Abstract and Conclusion section would be modified. Please address this issue.

We respectfully disagree with the reviewer. Table 2, which occupies a whole page (page 15) details our careful consideration of snowpack density and SSA profiles in simulations. The values used are based on our field measurements shown in great detail in Figures 5, S2 and S3. The variability on snowpack properties has therefore been at the core of our reasoning and simulations. Line 185, we explain regarding simulations that "In TARTES, input data are the thickness, density and SSA of each snow layer.", implying we do consider snowpack properties. Lines 286-287 we further state "An irradiance profile can be simulated if the physical properties (SSA and density) and impurity concentrations of the snow layers are known", again implying we do consider these properties. Subsequent lines stress even further than these properties are at the core of our simulations.

Q4. Branch density is a crucial factor, yet it's only briefly mentioned in Section 4.3. I recommend including comparative tests in Section 3 to explore its influence.

Branch density is indeed a crucial factor which manifests by the amount of light absorbed. We explain in detail that we make the hypothesis that branches are considered as a homogeneous absorber like soot, despite the fact that they are discrete absorbers. This is mentioned, necessarily briefly, in the abstract line 29. In Methods, we now elaborate on this and clarify this point. Lines 197-201, we now write "What TARTES uses is an absorption coefficient, which can be translated into a concentration of any impurity provided that the mass absorption coefficient of that impurity is known. It may also eventually be translated into a branch density. Here, we translate for simplicity the absorption coefficient used in TARTES into a soot concentration, because soot is a common and highly absorbing impurity in snow (Hansen and Nazarenko, 2004; Chylek et al., 1983; Warren and Clarke, 1990). The soot optical properties used are those reported in (Bond and Bergstrom, 2006)". This, combined with the following lines 203-203, should make it clear that we consider branch density a crucial factor.

Branch density is also mentioned in the discussion lines 383, 418-423 and in the conclusion line 446. The requested comparative test was already made in Table 4.

Minor comments:

Abstract

Lines 23-24: The sentence needs clarification and rephrasing.

The abstract should clearly highlight the research gap this study aims to fill.

This repeats Q1, which has been addressed above.

Introduction

Line 38-41: Please provide more information on this physical process.

We replaced "Snowpack photochemistry modifies the snow composition and produces…" with "Chemical reactions in the snowpack lead to the production of numerous species which are released in snowpack interstitial air. Produced species include NO and NO_2 ….", lines 39-41

Line 46-47: Explain the focus on the 300-450nm wavelength range. And comment on the use of 760 nm in this study.

The focus on the 300-450 nm wavelength range is because "Most snowpack photochemical reactions are triggered by radiation in the 300 to 450 nm wavelength range (Grannas et al.,2007; Wang, 2021)", as explained lines 49-50. We also write in the abstract, lines 31-32: "Noting that photochemically active radiation is mostly in the near UV and blue…". Furthermore, we write line 68 "The 390 nm wavelength is within the most photoactive wavelength range…". Regarding the 760 nm wavelength range, we write lines 66-68: "At 760 nm, photochemistry is not known to be active for most molecules. However, at this wavelength, the ice absorption coefficient is about 120 times greater than at 390 nm (Picard et al., 2016), so that investigating this longer wavelength informs us on the impact of shrubs under more absorbing ice conditions." We believe these explanations are sufficient.

Line 54-55: Expand the introduction of previous studies, detailing their measurement methods and identifying research gaps they left unaddressed.

Thank you for raising this point. In fact, these 3 studies measured the impact of shrubs protruding above the snow on irradiance above the snow. It is therefore not relevant to our study, focused on irradiance within the snowpack. Mentioning them adds confusion without any added value for our purpose. We will therefore delete the mention to these 3 studies. Lines 56-61 have been deleted.

Line 62: Specify the species of shrub studied.

We added Alnus incana line 67, as already mentioned in the abstract (line 26) and in methods, line 105.

Line 66: Add a reference to support your statement made.

We added the references (Grannas et al., 2007; Wang, 2021).

2.2 Sensor deployment and site description

Figure 2: Include images to illustrate sensor deployment both before and after snow cover, showing how measurements are taken.

Figure 2 shows such pictures before sensor heads burial and after its burial. Figure S1 also provides an extra 4 pictures detailing the setup with views of the sensors before burial. After burial, sensors are not visible anymore, as shown in Figure 2. We believe these images are sufficient.

Section 2.4: change all instances of "snow heigh" to "snow depth"

When the snow surface is used as a reference, snow depth is adequate. When the ground is used as a reference, snow height is more appropriate. Snow height is commonly used and we use it as required.

Line 172: Provide an explanation for the statement made.

We realize that this statement can be confusing. Furthermore, it adds no useful information. We deleted it, line 180.

Line 187: Specify the number of snow layers considered and describe how snow depth is divided into these layers.

We complemented "In TARTES, input data are the thickness, density and SSA of each snow layer." With "In TARTES, input data are the thickness, density and SSA of each snow layer, as determined from observations.", line 197. This also appears very clearly when results are detailed, as well as the division into snow layers.

Lines 187-188 & 190-191: Justify the assumption that all absorbing impurities are soot-like and explain why other elements like dust are not considered.

This comment is similar to Q4 above. We do not assume that all absorbing impurities are soot or even soot-like but we seek a soot equivalent concentration in the range of wavelength of interest here. To clarify this, we have added lines 197-201 "What TARTES uses is an absorption coefficient, which can be translated into a concentration of any impurity provided that the mass absorption coefficient of that impurity is known. It may also eventually be translated into a branch density. Here, we translate for simplicity them absorption coefficient used in TARTES into a soot concentration, because soot is a common and highly absorbing impurity in snow (Hansen and Nazarenko, 2004; Chylek et al., 1983; Warren and Clarke, 1990). The soot optical properties used are those reported in (Bond and Bergstrom, 2006)"

Line 195 and 202: Explain how the values "~29 cm" and "8.2 cm" were derived.

We replaced lines 194-195: "At 390 nm, we calculate that for typical snow encountered during this study (density=200 kg m-3, SSA=25 m2 kg-1, soot=25 ng g-1), irradiance is reduced by a factor of 10 over a distance of 29 cm." with "At 390 nm, we calculate using TARTES that irradiance is reduced by a factor of 10 at a depth of 29 cm for typical snow encountered during this study (density=200 kg m^{-3}, SSA=25 m^2 kg^{-1}, soot=25 ng g^{-1}). Lines 207-209.

Section 3:

Line 214-215 & 221-222: Provide more detailed explanations for Fig. 5 and Fig. 6, guiding the reader through the snowpack properties evolution and the significance of the figures.

Figure 5 just shows vertical profiles of snow physical properties. These are common plots seen in numerous papers appearing in The Cryosphere. Perhaps the reviewer is suggesting to comment variations, metamorphism, etc. This will not serve our purpose and again, for concision, we will just *focus on our topic: the role of shrubs on irradiance profiles. Likewise, Figure 6 is just time series of snow height, very common in papers in The Cryosphere. There is again no need to detail the precipitation and melting events at this stage. We do comment melting events when required, e.g., section 4.1. However, here, extra comments are not useful to our purpose and we do want to remain as concise as possible.*

Line 232-234 & 244: Clarify the conclusions drawn and specify the variables or evidence supporting them.

There are no conclusions here, just a factual description of the data. We explain, referring to Figure 7, that the data coming from the monitoring of downwelling solar radiation on March 3rd is characteristic of clear-sky conditions, which we feel is pretty clear. On other days, plots differ from this shape. Irregular variations in irradiance indicate variable cloudiness while days with permanent low irradiance indicate continuous cloudiness. We feel these are simple facts. To make sure there is no confusion, line 249 we added "solar" before "shortwave irradiance" and "radiometer" after "CNR4". Regarding line 244 (now 261), we feel that the current text "As expected, irradiance signals are lower at SHRUB than at FIELD because of light absorption by shrub branches" will be understood by readers because we already discuss this at length in the Introduction and in Methods.

Line 236: Address the potential uncertainty error in the simulation due to direct radiation on March 6th

Line 252, we added "Periods with direct radiations were removed from the analysis."

Line 245: Explain the selection of specific days for analysis and clarify the statements made in relation to Section 3.1.

Line 186, we state clearly that we "limit our data analysis to overcast conditions, when incident light was diffuse, similar to the conditions of the sensors buried in the snow.". Therefore, the days selected were overcast. We nevertheless repeated line 263 "because overcast conditions were observed".

Line 245: why did you select these four days "February 2nd, 3rd and 23rd and April 1st" for analysis? If you think the following sentence is the reason, it is still unclear. You didn't give the explanation in Section 3.1

The explanation was given line 185-187 "it was simpler and probably less error-prone to limit our data analysis to overcast conditions, when incident light was diffuse, similar to the conditions of the sensors buried in the snow" and as just stated, has been repeated line 263.

Line 245-246: Explain the statement made here.

We understand the reviewer is referring to the statement ". Only one to two sensors were then buried, as visible in Fig. 6." We believe that a cursory look at Figure 6, which shows the time series of snow height and the height of the sensors, will convince the attentive reader that on the days discussed, indeed one to 2 sensors were buried.

Line 253: Provide additional explanation and comment for Fig. 9.

This is similar to Figure 7, but for the red radiation. We explain line 71 that ice absorbs much more at red wavelengths than at blue wavelengths, and we repeat this line 269. Lower signals are expected, especially at depth.

Section 3.3: "A... reported in Table 1. B.... is shown in Fig. S4. ...". Clarify the purpose of the two sentences and Table 1 in this section.

We changed "The mean diameter and number of branches of the shrub canopy in a representative shrub at the level of the S325, S485 and S650 sensors are reported in Table 1." To "The mean diameter and number of branches of the shrub canopy in a representative shrub at heights of 325, 485, and 650 mm, which correspond to the heights of the S325, S485 and S650 sensors are reported in Table 1." (line 277). We changed "The distribution of branch diameters at these same levels is shown in Fig. S4" to "The distributions of branch diameters at these three heights are shown in Fig. S4". (line 279).

Line 266: Explain the selection of these specific days for analysis "February 2nd, 3rd, 23rd and 28th, March 6th and April 1st."

These were overcast days, as explained twice in the text above.

Line 268-271: Rephrase the sentence to improve clarity on the simulation parameters used.

We replaced "For February 2^{nd} and 23^{rd}, we used the physical data obtained on those very days during our snowpit measurements." with "For February 2^{nd} and 23^{rd}, we used the snow density and specific surface area values obtained on those very days during our snowpit measurements (Figure 5 and Figure S2)." (Line 288).

Line 272-273: Provide references or evidence to support the idea presented.

Our text reads: "The concentration of impurities in the snow, treated as soot-equivalent, was not measured and was used as an adjustable variable". This is a methodological choice, as now explained in great detail in the methods section, lines 197-202, as discussed earlier.

Table 2: Clarify if the soot density information is derived from the simulation, based on the description in Lines 272-273.

Yes, as the Reviewer mentions, this has already been clearly mentioned lines 292-293. "The concentration of impurities in the snow, treated as soot-equivalent, was not measured and was used as an adjustable variable". and we will not repeat it here. A Table heading has to be kept short and a Table has to be read with the corresponding text.

Fig. 10: Provide further descriptions and comments to guide the reader's understanding.

We addressed this comment in the Reviewer's Q2 comment and will not repeat this here.

Line 356: "Figs. 9 and 10 illustrate that irradiance decreases faster with depth at SHRUB than at FIELD." Acknowledge that the faster decrease in irradiance with depth at SHRUB compared to FIELD also suggests the influence of snow properties on irradiance reduction

We are not sure to understand this comment. Perhaps the Reviewer is suggesting that irradiance reduction is also caused by snow, as a function of its density and specific surface area. This basic snow physics concept has been alluded to many times in the text and need not be repeated here. Furthermore, we are only discussing the comparison between SHRUB and FIELD, so we do not feel this comment is relevant to this part of the discussion.

Responses to Review 2

We thank the Reviewer for the time spent reading our paper and for providing useful comments. Our responses are embedded in the Reviewer's comments, in blue italics. Line numbers refer to those in the tracked changes version.

Like soot, shrub branches can have large impacts on the vertical irradiance distribution in the snowpack. This study used the comparative measurements of irradiance in snow with and without shrub branches to show the significant impacts of shrub branches in radiative transfer processes in snowpack, which is interesting and promising. My most comments are related to the technical clarifications and in-depth discussion. Please see below for my specific comments.

Major concerns:

1. Section 2.4: Please provide more details on the snow height estimation. How did the authors use camera photos to estimate snow height? Please also provide more details on the uncertainty of 1.5 cm.

We changed "The snow height was determined from camera photos taken on site" to "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". Lines 159-160. Regarding the 1.5 cm uncertainty, we changed "Considering spatial variations, we estimate the uncertainty on snow height to be 1.5 cm." to "Spatial variations between the various striped poles indicate an uncertainty on snow height at the sensors of interest of 1.5 cm." Lines 163-164.

2. Line 170-174: How did the authors acquire the gain correction factor as well as the detection limit?

Line 178, we changed "The analysis of data when no sensor was buried" to "The comparison of signals from all sensors when no sensor was buried". Regarding the detection limit, we changed "The detection limit for $I_{r,i}/I_0$ was found to be 0.002" to "The detection limit for $I_{r,i}/I_0$ was taken as the signal greater than 3 times the noise, and found to be 0.002." Line 180.

3. Line 175-194: How did the authors determine the sky conditions as the overcast?

Line 191-192, we changed "Overcast conditions were determined from time lapse images and from irradiance values" to "Overcast conditions were determined from time lapse images, which revealed the presence of clouds and the lack of shadows, and from the daily time series of solar irradiance, which deviated from the typical clear-sky plots". Subsequently, Figure 7 illustrates the various sky conditions.

4. Line 187-190: Please clarify how the authors considered the soot in the simulations, considering no direct measurements of soot concentration? How about dust? Black carbon, brown carbon and dust can have very distinct impacts on snowpack.

We now explain lines 197-202 that all we need in the simulations is an absorption coefficient regardless of the type of impurities: "What TARTES uses is an absorption coefficient, which can be translated into a concentration of any impurity provided that the mass absorption coefficient of that impurity is known. It may also eventually be translated into a branch density. Here, we translate for simplicity the absorption coefficient used in TARTES into a soot concentration, because soot is a common and highly absorbing impurity in snow (Hansen and Nazarenko, 2004; Chylek et al., 1983; Warren and Clarke, 1990). The soot optical properties used are those reported in (Bond and Bergstrom, 2006)".

Subsequently, lines 292-293 explain "The concentration of impurities in the snow, treated as soot-equivalent, was not measured and was used as an adjustable variable to optimize the agreement between measured and simulated irradiance profiles."

5. Line 190: Please clarify why did the authors can use soot to simulate the impacts of branches? Their optical properties can be very different.

We believe our addition lines 197-202 also addresses adequately this question.

6. Line 196-208: There are many numbers in this paragraph. Please clarify where these numbers are from and what is the major points from these numbers?

To clarify our objective, we now start this paragraph with "Figure 4 sums up the optical properties of the constituents considered here: ice, soot and bark". Line 211.

7. Section 3.1 & 3.2, 4.2: Please add the statistical tests to check whether the differences are significant among SHRUB and FIELD. Besides, the introduction in section 3.1 is too simple.

The objective of this work is to determine the soot equivalent of shrub branches. Table 2 shows rather clearly that the soot values of SHRUB are always greater than those of FIELD. Statistics do not appear as a necessary addition. For each line the soot value at SHRUB is greater than that of FIELD. The average soot value of SHRUB is 111±54 ppb while at FIELD it is 23± 9 ppb. Discussing this further does not seem essential.

In section 3.1, we just factually report snow physical measurements, the methods of which have been detailed in section 2.5. The methods are standard, very common and well established in snow field studies. We do not think extra details are required here.

8. Section 3.3 is too simple. Did the authors use these data for the analysis and explanations?

Yes indeed, we do want to keep this simple and concise. The only objective of this section is to simply present data on branch numbers and diameters. This must remain simple. These data are used in the discussion, section 4.4.

9. Line 278-280: Please explain why the soot concentration of a given layer is not expected to vary significantly over time.

We added line 299 "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)."

10. Section 4.1: Can the authors provide some field-based evidences for this?

The only true field evidence would have been to dig a snowpit at the very spots involved, but this destructive action is not compatible with our monitoring activity. Moreover, it would have required analyzing the data before the data were obtained. We do however provide field evidence at a nearby site. Fig. 6 of Bouchard et al. (2024) is a clear demonstration of the reality of the process invoked, as mentioned line 350. Percolation channels are very common in temperate snowpacks and are abundantly described in the literature (e.g. Sturm et al, 1995, https://doi.org/10.1175/1520-0442(1995)008<1261:ASSCCS>2.0.CO;2).

11. Many parts of the discussion should be moved to results section.

Indeed, this is debatable. Our choice however was to have all these parts in the discussion, because they are not derived from primary data analysis, but secondary data analysis. There are several possible strategies to write a paper.

12. How the site-scale findings in this study can be extended and incorporated into Earth system models deserves more discussion.

We feel that section 4.5 "Impact of branches on snowpack photochemistry", is already quite lengthy and we already mention line 424 that "We therefore speculate…", indicating that we already integrate a certain degree of speculation. We conclude this section by writing line 442 "The reduction of snow photochemical rates by shrub expansion may thus lead to numerous chemical and climatic effects that may deserve further quantification." We added at the end "using coupled models of snow and atmospheric chemistry. (Toyota et al., 2014; Zatko et al., 2016)." Going beyond that and discussing Earth system model would be overspeculation.

Minor concerns:

1. Figure 3: Please provide the full names of some abbreviations in the caption.

We added: PWR; Power; SSH: Secure Shell protocol; RSSH: Reverse SSH protocol; SFTP: SSH File Transfer Protocol.

2. Figure 4: How about the bark absorption?

The data available in the literature is the bark reflectance. With the assumption that the bark is not transparent i.e., thick enough: 1-reflectance is equal to absorption. We changed the legend to 'bark absorptivity" and added in the caption: "The absorptivity of Alnus incana bark is calculated as 1 – bark reflectivity (Juola et al., 2022b), assuming bark is thick enough to be fully opaque".

3. Table 2 & 4: How did the authors determine these values?

Table 2: these values are discussed at length in the text. Lines 287-294 explain: "For February 2nd and 23rd, we used the snow density and specific surface area values obtained on those very days during our snowpit measurements (Figure 5 and Figure S2). For the other days, snow physical properties were estimated from the snowpit data, the literature (Domine et al., 2007; Taillandier et al., 2007) which helps in estimating the time-evolution of snow physical properties and the SSA-density correlation, and above all from our experience of snow physical properties and their evolution at the Montmorency Forest (Bouchard et al., 2023; Bouchard et al., 2022). The concentration of impurities in the snow, treated as soot-equivalent, was not measured and was used as an adjustable variable to optimize the agreement between measured and simulated irradiance profiles." This text appears just before Table 2.

Table 4: Lines 393-395 read: "Based on photographs of the S325 and S485 sensors taken during installation (Fig. S1) and also 370 on the data of Fig. S4 in the Supplement, we attempt to estimate the number and mean diameter of branches within two e-folding depths of each sensor, for both wavelengths studied. These estimates are reported in Table 4." This text appears just before Table 4.

4. Section 4.4: This section belongs to results.

We have addressed this comment in our response to major comment 11 above.