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.

This paper is intended for readers of The Cryosphere, who are expected to be familiar with basic concepts of snow studies. Again for the sake of concision, recommendations by the reviewer to detail such concepts are not followed. Our responses are embedded in the Reviewer's comments, in blue italics.

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 propose to add 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 propose to modify line 62 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, familiar to readers of The Cryosphere. Figure 6 shows time series of snow height which are also familiar to readers of The Cryosphere. It also shows the dates of field work and the dates for which simulations were performed, and lastly the height of sensors placed in snow which probably. All this 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 propose to replace "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 propose to add 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. Part of this is however already explained in the text lines 242-243. It may be better to add some text to avoid duplicating the text in Figure captions. 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 propose to rephrase 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 are proposed. Regarding Table 1, we propose modify 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 265-266 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. As far as our work is concerned, branch density manifests by the amount of light absorbed. We explain in detail that branches are considered as a homogeneous absorber like soot, despite the fact that they are discrete absorbers. This is mentioned in the abstract line 28, in methods line 191, in the discussion lines 352, 397 and 404 and in the conclusion line 426. Therefore, in our approach variations in branch density will have the same effect as variation in soot concentrations. We propose to stress and clarify this by adding in the Methods section, line 193: "We therefore expect a higher branch density to manifest itself by requiring the use of a higher soot concentration in simulations".

The validity of this approach is challenged at 760 nm, and we therefore already present comparative tests in this case in section 3, as detailed in Figure 11 and lines 315-317: "We therefore performed simulations at 760 nm using the same soot concentrations for SHRUB 315 as at 390 nm. To investigate the impact of bark optical properties that would vary with wavelength differently to soot, we also performed simulations with concentration multiplied by 0.33 and by 3. Results of simulations at 760 nm are shown in Fig. 11"

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.

Indeed, we have made a suggestion in our reply to Q1.

Introduction

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

We propose to replace "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 *…."*

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 46-47. We also write in the abstract, lines 30-31: "Noting that photochemically active radiation is mostly in the near UV and blue…". Furthermore, we write line 64 "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."

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. We will delete lines 54-58.

Line 62: Specify the species of shrub studied.

We will add Alnus incana, as already mentioned in the abstract (line 25) and in methods, line 100.

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

We will add the references (Grannas et al., 2007; Wang, 2021), already cited lines 46-47.

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 head 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.

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 apologize for the lack of clarity. We propose to replace "For the SHRUB sensors, $(I_{r,i}/I_0)_c$ *therefore takes into account the shading by shrub branches." With "When shrubs are present, branches protruding above the snow reduce the radiation incident on the snow surface, and this reduction appears in* $(I_{r,i}/I_0)_c$*. However, since we are interested in the extinction within the snowpack, this does not affect our data analysis."*

In fact, we realize that this initially confusing statement adds no useful information. It is probably even better to simply delete it, which is our preferred option.

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

We could complement "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." However, this appears very clearly when results are detailed, so this addition may not be useful at this stage.

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.

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 for photochemistry. This simplifies the problem "we consider for simplicity," (line 187) that is otherwise intractable without spectrometer measurements. Dust is certainly present, as well as numerous other absorbers, but all we need is an impurity absorption coefficient accounting for all absorbers. We could also treat it as dust, and come up with a dust equivalent. However, soot is often the main absorber in snow, and presents less diversity and has a flat absorption spectrum compared to any other components which make it more suitable for an equivalent. See for instance Tuzet et al. 2019.

Tuzet, F., Dumont, M., Arnaud, L., Voisin, D., Lamare, M., Larue, F., Revuelto, J., and Picard, G.: Influence of light-absorbing particles on snow spectral irradiance profiles, The Cryosphere*, 3, 2169–2187, doi:10.5194/tc-13-2169-2019, 2019*

To clarify this, we propose to add line 193: "Any other type of impurity could be used to simulate absorption. The important parameters are the optical constant and the concentration of the impurity, which determine absorption."

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

We propose to replace lines 193-194: "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}).

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.

These Figures are intended for readers of The Cryosphere, who have at least minimal familiarity with snow studies. Figure 5 shows vertical profiles of snow physical properties, among the most basic data shown in snow field studies. Likewise, Figure 6 is a time series of snow height, again a basic concept in snow field studies.

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

This paper was written with the understanding that readers had minimal knowledge regarding a time series of solar irradiance during daytime under clear-sky conditions. 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 will be obvious to readers of The Cryosphere. 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 expect this to be known by readers. Line 233 we could add "solar" before "shortwave irradiance" and "radiometer" after "CNR4" if that would help. Regarding line 244, 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 236, we will add "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 177, 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 will nevertheless add line 245 "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 177 and as just stated, we will repeat it here.

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 67 that ice absorbs much more at red wavelengths than at blue wavelengths, and we repeat this line 251. Lower signals are expected, especially at depth. We believe this is understandable for readers of The Cryosphere.

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 will change "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." We will change "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 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 propose to change "For February 2nd and 23rd, we used the physical data obtained on those very days during our snowpit measurements." With "For February 2nd and 23rd, we used the snow density and specific surface area values obtained on those very days during our snowpit measurements."

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 explained in the methods section, lines 187-188, which reads "Here, we consider for simplicity that all absorbing impurities are soot, with properties reported in Bond and Bergstrom, (2006),"

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 described lines 272-273 and we will not repeat it here.

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.