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Barcelona, 2nd August 2023

Dear Dr. Haegli,

I am pleased to submit the revised version of the manuscript entitled "**Rain-on-snow responses to a warmer Pyrenees**", co-authored by myself, Dr. Juan Ignacio López-Moreno, Dr. Esteban Alonso-González, Dr. César Deschamps-Berger and Dr. Marc Oliva.

We would like to express our sincere gratitude for your valuable recommendations and feedback. All the referee's recommendations have been carefully considered and have significantly improved the manuscript, enhancing its scientific rigor.

The main manuscript modifications are summarized as follows:

1.- We have followed the reviewer's advice and updated the elevation band names. Additionally, the baseline temporal period is now clearly mentioned from the beginning of the manuscript. We have also included the relevant IPCC quotes.

2.- We have provided an extensive description of the sensitivity analysis conducted in this study and the rationale behind its use. It is important to acknowledge that sensitivity studies and climate projections are distinct types of work. In this study, we focused on evaluating the rain-on-snow sensitivity to temperature and precipitation, which allowed us to understand the non-linear spatiotemporal variations in different sectors and elevations of the Pyrenees. As mentioned by Reviewer 1, representing the results as "change per 1ºC" is advantageous, as it facilitates comparisons with other regions and seasons.

3.- Regarding our decision to use a sensitivity analysis instead of directly use GCMs models, we considered the high uncertainty associated with climate projections for the Pyrenees, particularly concerning precipitation among different models and GHGs emission scenarios presented in previous works (López-Moreno et al., 2008). To address this, we provided temperature and precipitation change values based on already established and latest climate projections for the region. While we acknowledge that this introduces some uncertainty, we consider it is still more reliable than presenting different outputs from model ensembles.

We have included a detailed point-by-point response to the reviewer's comments on the following pages.

We hope that these revisions meet your expectations, and we believe that the new version of the manuscript is now suitable for publication in **Natural Hazards and Earth System Sciences**.

Should you have any further inquiries about this work, please do not hesitate to contact us. We will be happy to answer any question you may have.

Best regards,

Mr. Josep Mª Bonsoms, on-behalf of Dr. Juan Ignacio López-Moreno, Dr. Esteban Alonso-González, Dr. César Deschamps-Berger and Dr. Marc Oliva.

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Reviewer 2: General comments

Review of « Rain-on-snow response to a warmer Pyrenees » by Bonsoms et al.

The manuscript entitled « Rain-on-snow response to a warmer Pyrenees », by Bonsoms et al., is a sensitivity study about the frequency and magnitude of rain-on-snow events in the Pyrenees, under various local temperature change values. The topic is relevant and new knowledge is interesting to have, to better assess the evolution of related risks under climate change. Overall, I did not detect major flaws in the work carried out, however I have some reservations about the novelty and clarity of the methods used and results obtained in this study. I am not convinced that simple « delta change » methods remain an appropriate choice, at a time where regional climate simulations are readily available, especially in European areas. Combined with a lack of connection to a scenario analysis (i.e., under which circumstances a local warming of 1 to 4°C could/would occur in the Pyrenees, compared to the baseline period 1980-2019 ?), this manuscrips lacks some key elements such as an analysis of the uncertainty induced by the approach developed here, compared to alternative approaches. I also find that the graphical representation of the results could be made clearer and more compact, including, for example, results at the scale of the entire mountain range rather than focusing only on 4 subregions. Also, I find that this study quotes a very large number of references (I counted 100 references), and that it would be preferable, I think, to select a subset of targeted references to support the positioning and the discussion of the results, rather than this very long list of references. Ways forward includ, for example quoting the still recent IPCC SROCC « High mountain areas » chapter (Hock et al., 2019), which includes an analysis of the state of knowledge about climate change and rain on snow events (section 2.3.2.1.3 on Floods). It is indicated there that :

« In summary, evidence since AR5 suggests that rain-on snow events have increased over the last decades at high elevations, particularly during transition periods from autumn to winter and winter to spring (medium confidence). The occurrence of rain-on-snow events has decreased over the last decade in low-elevation or low-latitude areas due to a decreasing duration of the snowpack, except for the coldest months of the year (medium confidence). »

And, for future projections :

« In summary, evidence since AR5 suggests that the frequency of rain-on-snow events is projected to increase and occur earlier in spring and later in autumn at higher elevation and to decrease at lower elevation (high confidence). »

We want to express our sincere gratitude for your review.

Following we provide some explanations to the reservations shown by the reviewer in some specific questions.

1.- This work focuses on the sensitivity analysis of ROS to temperature and precipitation; we are not performing snow climate projections. We acknowledge that climate projections and sensitivity studies are different types of work, each having distinct scientific objectives and providing insights into different impacts. In snow sensitivity studies, we evaluate the snowpack's response to changes in the forcing variables, specifically atmospheric variables in this case. As explicitly stated in our work, the perturbations performed are based on future climate projections from the latest climate project (CLIMPY) and detailed

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at Amblar-Francés et al. (2020). In the revised paper, we have added a paragraph (section 5.5) where we reinforce the idea of the usefulness of applying a sensitivity analysis. The sensitivity analysis provides easily comparable information with other regions, making it better suited to address the high uncertainty of climate models when projecting precipitation in the Pyrenees (López-Moreno et al., 2008; Amblar-Francés et al., 2020). The range of temperature and precipitation changes used in our study allows for easily interpretable results compared to other regions and seasons. Climate projections would also entail other problems named before.

2.- We have indeed mentioned the IPCC. It is worth noting that the IPCC text cited by the reviewer of *High Mountain Areas* by Hock et al. (2019) was co-authored by one of the authors of our manuscript. In detail, the statement made in that IPCC text was based on studies conducted in other mountain regions, and this specific topic had not been previously addressed in the context of the Pyrenees, especially considering its sectors and elevational bands. Consequently, we firmly believe that our work significantly contributes to filling this gap by providing specific elevation thresholds for the Pyrenees and providing insights into future climatic changes in this mountain range.

While a few studies were published since that time and expend the available body of literature, I think the introduction (and the long list of references quoted there) could be substantially shortened by refering to this critical assessment of the state of knowledge, and positioning the scope and objectives of the current study on this basis. This scientific study targets a scientific audience, I think it is perfectly appropriate to quickly introduce the context and state-of-the-art in this topic and then introduce very early in the manuscript how the challenges are addressed in the study. I think this could save quite a lot of space and avoid quoting an unecessarily large number of references.

We modified the corresponding sections of the manuscript following your suggestion, as far as we could. We want to highlight the relevance of show the potential implications of ROS in the ecosystem. This is because:

1.- The discussion and the interrelationship across natural, social sciences and natural hazards impacts is the scope of Natural Hazards and Earth System Sciences. This was the reason why we decided to send the work to this journal.

2.- From the introduction to the conclusion section, the article has around 7500 words and 18 pages without figures. The Natural Hazards and Earth System Sciences pages limit extension is 24 pages. We are far from the word limit extension required by the Natural Hazards and Earth System Sciences.

3.- We consider that it provides an accurate context of the results found in this work and its ecosystem impacts, which are in line with the scope of the journal.

If the editor considers we should change the manuscript accordingly, of course, we will implement such changes.

I have a series of comments and suggestions, which I provide below :

Page 1, line 8 : While the term is not introduced, I understand that « ROS fr » refers to « ROS frequency ». I strongly suggest that the full word is spelled out, as « ROS frequency », throughout the text. This will increase its readability.

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Done. We have changed ROS fr to ROS frequency.

Page 1, line 17 : I did not understand what is meant by « slow, and non-changes in ROS ablation ». I suggest this is reformulated.

Done.

We have changed: "On the contrary, slow, and non-changes in ROS ablation rates are found for warm and marginal snowpacks"

To

"On the contrary, small differences in ROS ablation are found for warm and marginal snowpacks."

Page 1, line 26 : These introductory statements could be greatly simplified by referring to assessment reports, such as the IPCC ; this would also reduce citations of rather « old » references.¨

Thank you for your recommendation. We have included the IPCC in our work.

Page 2, line 31 : « leading in some cases to ROS events ». To me this is incorrect. A ROS event occurs when rainfall falls on a a snow-covered ground. Such a definition is lacking from the manuscript until section 4.1, I think this should really be provided earlier. Also, ROS have always occurred in mountain regions, but climate change is modifying their frequency and elevation distribution. Climate change does not « lead » to the existence of ROS in mountains, but modifies their patterns. This needs to be clarified, and I strongly suggest that a definition of what a ROS is should/could be added.

We have changed the manuscript accordingly: "leading in some cases to ROS events" to "leading in some cases to ROS events **in snow covered areas"**

Regarding the ROS definition, in the methodological section we provide a definition of ROS. We prefer to not repeat more times the information in the introduction.

Page 2, line 33 : « Mountain elevation-dependent warming ». I think this deserves some clarifications here. Elevation dependent warming (EDW) refers to the fact that, in some cases, the magnitude of the climate trend is not the same depending on elevation. This is debated and the evidence is not unequivocal. However, there is no need to invoke EDW to state that snow cover changes (including ROS) depend on elevation. Indeed, climate conditions depend on elevation, such as the mean snowfall fraction, so that a similar change in temperature would have different consequences depending on the elevation. This shows that there can be elevation dependent changes without necessarily elevation depending warming. I think this could/should be clarified in the introduction here, as this is a confusion which is often made, and this manuscript could offer an opportunity to clarify this, especially in a context where the « delta change » approach applies a uniform warming level to all elevations considered, i.e. it ignores EDW in its very design.

Thanks, we agree. We have changed "mountain-dependent warming" to "warming in mountain regions" in order to avoid confusions. We have also delated the word "delta-change" in our work since our methodological conceptualization is different from previous "delta-change" definitions.

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Page 2, line 43 : I think the various SEB components could/should be more precisely described, rather than quoting previous references. There is a common misconception that rainfall is directly causing snow melt during ROS events, and the introduction does not explicitly allude to the processes responsible for the influence of ROS events. Again, no need to quote dozens of references, but a few clear statements on the physical processes related to ROS events and their consequences would be useful.

Thank you for your recommendation. We agree, rainfall is not the main driver of snow ablation during ROS events (manuscript first version; L462 to L473). Given that this information is presented in the discussion section of the manuscript, we have now added: "further works should analyze the SEB controls during ROS events within the mountain range, and its response to climate warming".

Page 3, line 73. I have some questions about the concept « ROS drivers ». But before, I think the manuscript lacks a clear definition of what a ROS is (see above), and how is it computed. A ROS occurs then rainfall occurs over a snow-covered ground, hence it recquires an analysis of the simultaneity between two variables (non zero snow cover and non zero rainfall). What is the threshold (i) in terms of snow depth or SWE and (ii) in terms of rainfall amount (daily ?) used to state whether a given day is a « ROS day » ? This should be quickly introduced here in the introduction, and with more details in the Methods section. In this sens, « snow depth / height of snow » and « snowfall fraction » are not individual drivers of ROS, but ROS stems from their combined time series at daily or subdaily time resolution. The analysis cannot be done independently, or, if so, reasons must be given what this is relevant.

Thanks, we have delated "ROS drivers" according to your suggestion. We consider that in the introduction the reader should know about the uncertainties, relevant ROS literature, rather than methodological details.

Therefore, the information (i) and (ii) that Reviewer 2 makes references is included in the methodological section:

"Data and methods", "3.5 HS, Sf and ROS climate indicators", in particular.

We have changed the name of the section to gain visibility:

"3.5 ROS definition and indicators".

The average HS and Sf sensitivity to temperature and precipitation (expressed in % per ºC) is the average seasonal HS and Sf anomalies under the baseline climate and divided by degree of warming. In this work we used previous ROS days classification; in particular, days are are classified as ROS days when daily rainfall amount was >= 10 mm and HS >= 0.1 m, according to previous works (Musselman et al., 2018; López-Moreno et al., 2021). ROS frequency are the number of ROS days. ROS rain is the average daily rainfall (mm) during a ROS day. ROS ablation is the average daily snow ablation (cm) during a ROS day. The average daily snow ablation is the daily average HS difference between two consecutive days (Musselman et al., 2017a). Only the days when a negative HS difference occured were selected. ROS exposure is the relation between ROS rain (y-axis) and ROS frequency (x-axis) differences from the baseline climate scenario for the massifs were ROS frequency is recorded for all increments of temeperature.

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Page 4, line 105 : « February (May) in low (high) elevations ». This is not correct grammatically, and should be rephrased for better clarity. See https://eos.org/opinions/parentheses-are-are-not-for-references-and-clarification-saving-space

Thanks, changed.

Page 4, Figure 1 : « low », « mid » and « high » should be defined in the caption (not defined at this stage in the text, and worth making clear in the caption). Also, the time period used for the analysis should be explicitly stated (1980-2019 ?).

Thanks for your suggestion. We have changed low, mid and high for the elevation in meters. Regarding the time period used for the analysis, we stated in L123 "….baseline climate (1980 – 2019)". The temporal period is selected according to the reference period used in the climate projections of the CLIMPY project (Amblar-Francés et al., 2020).

Page 4, line 117 : While I have no problem with using FSM2, I wonder what the Crocus model results, driven by SAFRAN, where not used at least to compare with the FSM2 results. These simulations are also provided on the AERIS data portal. Also, there are climate projections available for all the massifs in the Pyrenees using the adjustment method ADAMONT applied to an ensemble of EURO-CORDEX regional climate models driven by several CMIP5 GCMs, with the same geometry as the SAFRAN reanalysis. The method and type of results is described in Verfaillie et al. (2018, The Cryosphere), and the dataset (atmospheric and snow cover) dataset for climate projections is freely available on the Drias climate data portal (https://www.drias-climat.fr/accompagnement/sections/215). It is thus suprising that a simple delta change method has been applied here, without any comparison to other approaches and using other snow cover simulations. Combining the results obtained here would enhance the robustness of the analysis, by adding several ways to explore and quantify the uncertainty related to changes in ROS frequency and characteristics under climate change.

Thank you for the suggestion. If our primary objective was to characterize the spatiotemporal variability of ROS, the dataset you mentioned would have been suitable and straightforward for us to use.

However, since we aimed to conduct a sensitivity analysis (and to the best of our knowledge, the methodology followed in this sensitivity analysis is the most commonly used approach), along with analyzing the response of different ROS characteristics, we found it necessary to run our own snowpack simulations. Therefore, we opted to utilize the widely used and computationally efficient FSM2 model. As demonstrated by the presented validation and references provided, there is evidence that FSM2 provides robust results.

Page 5, line 140 : I suggest referring to « flat terrain »

Done.

Page 5, line 143 : « homogenized » is to be deleted.

Done.

Page 5, line 144 to 146 : this part of the sentence is not accurate and is misleading. Indeed, there are two implementations of SAFRAN in France : the original configuration of SAFRAN operates in mountain areas

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(Durand et al., Vernay et al.), and an another implementation was developed for the entire country, and referred to as « SAFRAN-France », providing results on a 8kmx8km grid. I think it is better to not mix references to these two systems. In this sense, the references to SAFRAN-France implementations (Habets et al., 2008, Quintana-Segui et al., 2008), would be better left out.

Thanks, we have changed the manuscript following your suggestion.

Page 5, line 161 : The elevation bands chosen are 1500, 1800 and 2400. It is not clear why these bands were chosen, and in particular whay there are not equally spaced. In this context, I suggest that throughout the manuscript the elevations are explicitly provided instead of « low », « mid » and « high » elevation, to avoid misunderstanding or overinterpreting trends at these three elevations.

Thanks for your suggestion. We have changed low, mid and high for the elevation in meters.

We selected these three bands since they are representative for three different elevations and are consistent with previous analysis within the range that show different snow-climate trends depending on the elevation (López-Moreno 2005; López-Moreno et al., 2007 and 2020; Alonso-Gonzalez et al., 2020).

Page 6, line 179 : I think that it would be appropriate to explain how the LWin was increased according to changes in temperature (I noticed the last sentence of the paragraph on the topic, better combine at the same place and provide more information such as an equation and/or a reference to the method employed).

Thank you for your recommendation, we have included the atmospheric emissivity equation:

" Ta was perturbed from +1ºC to +4ºC by +1ºC. LWin was increased due to warming, by applying the Stefan-Boltzmann law, using the Stefan-Botzmann constant ($\sigma; 5.670373x10^{-8}Wm^{-2}K^{-4}$), and the hourly atmospheric emissivity (ϵ_t) derived from SAFRAN Ta and LWin :

$$\epsilon_t = \frac{LW_{in}}{\sigma(Ta+273.15)^4}$$ "

Page 6, line 184 : « Delta-change » is a method that was developed and primarily employed a time when regional climate projections were not available or not usable, or for locations where this is still the case. While I understand that such approach may bear some relevance for sensitivity analyses, I think it should be stated clearly that such methods undersample some climate change effects, such as changes in the variability of meteorological conditions, which only climate modelling methods can approach. I think this should be clearly stated here and also recalled in the dicussion and conclusion. Also, I would strongly suggest to provide some context about the values used for the local warming level (1° to 4°C), i.e. how do they connect to global warming levels and/or climate change scenarios. Otherwise, the results here stand disconnected from the analysis of climate change impacts relevant to stakeholders and policy-makers, and other scientific studies based on scenarios and climate models.

We realized that Reviewer 2 "delta-change" conceptualization differs from the methodological approach we are performing in our work. This term has been used in different works for different objectives. We have removed this term in order to avoid confusions.

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In addition, following Reviewer 2 suggestion, we have added a paragraph in Section 3.4 where we inform about the global warming levels (please, see our response to Reviewer 1).

Page 6, line 187 : The reference time period should be clearly stated here. Is it 1980-2019 ?

Thanks, we state it in the very beginning of the manuscript, when it is mentioned: L123 "….baseline climate (1980 – 2019)". We have now added the temporal period in each figure description.

Page 6, line 195 : I don't understand why there is no reference to the change according to the change in precipitation amount (+ or – 10%) but only temperature. Could this be clarifier ?

This information is detailed in the first part of the results:

"Seasonal HS and Sf variability is mostly controlled by the increment of temperature, season, elevation, and spatial sector (Figure 2). The role of precipitation variability in the seasonal HS evolution is moderate to low (Figure S2 to S4). Only in high elevation an upward trend of precipitation (at least > 10%) can counterbalance small increments of temperature (< 1ºC, over the baseline climate) from December to February (Figure S4). For this reason, precipitation was excluded to further analysis". As we state in the manuscript, the reader can consult further information in the supplementary materials (Figure S2 to S4).

We have added Figure S1 to the main manuscript following Reviewer 2 suggestions.

Page 6, line 196 : Here is the much needed information about the definition for a ROS day and a number of other terms used in the manuscript but not introduced before, unfortunately. This should be clarified much earlier in the manuscript.

Thanks, the definition of ROS day, including the threshold mentioned, is detailed in the corresponding methodological section "Data and methods", and "3.5 HS, Sf and ROS climate indicators", in particular.

Note that we have changed the name of the section to gain visibility:

The new section name is: **"3.5 ROS definition and indicators".**

Page 6, line 198 : What is the motivation for defining ROS ablation based on the change in snow depth/height of snow : SWE is a much more appropriate variable to infer changes in snow quantity, because changes in snow depth/height of snow can be due to compaction. I'm certain that FSM2 can provide SWE output. More information should be given about the motivation for such a choice, and, if possible focus rather on SWE than snow depth/height of snow.

We analyzed this indicator since it has been extensively used in snow hydrology (section 3.5 and references therein). Therefore, it is easy to compare with previous works focused on this variable.

Following your suggestion, we have added:

"Further works should analyze the SEB controls during ROS events within the entire mountain range, as well as the ROS hydrological responses to climate warming".

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Page 7, line 213 : I'm not convinced by the term « ROS drivers », mostly because these are not independent drivers and that ROS compounds the state of the snow cover with the occurrence of rainfall. I would be more comfortable with simply stating that this is an analysis of the change in mean seasonal/monthly snow depth/height of snow and snowfall fraction.

Done.

We have changed:

"First, we analyze ROS drivers, namely height of snow (HS) and snowfall fraction (Sf) (López-Moreno et al., 2021), sensitivity to temperature and precipitation"

To

"First, we analyze ROS hydrological conditionings, namely height of snow (HS) and snowfall fraction (Sf) (López-Moreno et al., 2021), sensitivity to temperature and precipitation"

We have changed the term "ROS drivers" to "HS and Sf" according to your suggestion.

Page 7, line 214 : I think at least one figure showing the influence of the change in precipitation should be provided in the main manuscript and not only in the Supplement. That temperature plays a much stronger role than precipitation change was found as early as in the 1990s (also using delta change approaches, see Martin et al., 1994, Annales Geophysicae).

Thanks, we have moved Figure S1 to the manuscript following your suggestion.

Page 8, line 232. « baseline climate » should be provided explicitly in each figure caption. Also, we don't find the reference to the warming level in the legend of the figure, which is further unclear because the changes are indicated as % per °C. The content of the figure needs to be clarified, perhaps it is simply too complicated. As indicated above, « low », « mid » and « high » needs to be explained in the caption, especially in a context where the corresponding elevation bands are not equally spaced.

Thanks, we state it in the very beginning when it is mentioned: L123 "….baseline climate (1980 – 2019)". We have now added the temporal period in each figure description.

Page 8, line 247 : I think it would be good to always state that the values provided are for a given time period (1980-2019 ?) and also provide some information about the variation about the mean (standard deviation ? quantiles ?).

Thanks, we state it in the very beginning when it is mentioned: L123 "….baseline climate (1980 – 2019)". We have now added the temporal period in each figure description.

The variation around the mean is shown in the error bars of Figure 2: "Seasonal (a) HS and (b) Sf anomalies over the baseline climate. Data are shown by elevation (colors), season (x-axis) and sectors (boxes). Points represent the average seasonal HS and Sf anomalies grouped by month of the season and increment of temperature (from 1ºC to 4ºC). The black diamond point indicates the mean, whereas the upper and lower error bars show the Gaussian confidence based on the normal distribution."

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Page 9, line 259. There is a problem with the graphics, which shows spurious « wider » bars for panels with less bars. This should be fixed so that bars all have the same width, and the graphical processing account for the lack of value (or 0 values ?).

Done, we have changed the figures according to your suggestion.

I also suggest that some information about the variation around the mean is provided, especially because the rounding seems to have quite a large influence on the display of the results (in fact, why are the results rounded to the nearest integer ? In fact, I see no reason for this, there is no problem to refer to the mean number of days with ROS as non-integer value. My suggestion would be to remove this rounding, and include a representation of the variability around the mean (standard deviation ? quantiles ?). I also think that such figure would benefit from an overall graph showing the entire mountain range, with a sub-regional focus for a more in-depth analysis, given that many results seem rather comparable depending on the subregion.

Thank you for your recommendation, we have delated the rounding.

Unfortunately, we can not average the results for the entire mountain range as suggested by Reviewer 2. We are observing different ROS responses to temperature depending on the season and sector (Figure 2 to 10). One of the key findings of the work is the different ROS responses (under the same changes) depending on the sector. Average the values for the entire range are strongly not recommended since it reduces the variability between sectors. This is why we performed the PCA analysis described at 3.3 section, and the reason why we show the spatial figures.

The figure that Reviewer 2 is proposing is very similar to Figure 7.

Page 10, line 278. Figure 4 : the color palette is inadequate. It uses a diverging color palette although continuous, increasing values are shown. Maybe the baseline could be provided using a continuous/increasing color palette, and then the change compared to the reference could be displayed as a deviation from the reference (using a diverging palette, then).

Done, we have changed the figure according to Reviewer 2 suggestion.

Page 11, line 292, Figure 5 : same general comments for Figure 5 as for Figure 3.

Done, we have changed the figure according to Reviewer 2 suggestion.

Page 12, line 306, Figure 6 : same general comments for Figure 6 as for Figure 4.

Thank you for your suggestion. In this case, we believe that the colors used in Figure 6 (Figure 7 in this version) are intuitive and accurately represent the data. Implementing a sequential scale could reduce the visual interpretability of the data variability in the spatial plots. Therefore, it is essential to include a scale between two contrasting colors (e.g., black to red, as it is currently).

If the editor considers that we should modify this figure, of course, we will change it.

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Page 14, line 339, Figure 8 : same general comments for Figure 8 as for Figure 3.

Done, we have changed the figure according to Reviewer 2 suggestion.

Page 15, line 344, Figure 9 : same general comments for Figure 9 as for Figure 4.

Thank you for your suggestion. In this case, we believe that the colors used in Figure 9 (Figure 10 in this version) are intuitive and accurately represent the data. Implementing a sequential scale could reduce the visual interpretability of the data variability in the spatial plots. Therefore, it is essential to include a scale between two contrasting colors (e.g., black to red, as it is currently).

If the editor considers that we should modify this figure, of course, we will change it.

Page 16, line 356 : The sentence on climate projections is largely insufficient. More information should be provided here on the scenario considered, and the reference period used from which temperature and precipitation changes are reported. Here the statement on the temperature increase could be provided in a way that makes it possible to contextualize the temperature increase values (since 1980-2019 ?) used in this study.

Thanks. This information is included in the discussion section “5.2 ROS temporal evolution”.

We state it in the very beginning when it is mentioned: L123 “….baseline climate (1980 – 2019)”. We have now added the temporal period in each figure description.

Page 16, line 365 : « The contradiction between rainfall ratio increase and snowpack reductions ». I see no contradiction here at all, both the rainfall ratio (note that the manuscript refers rather to the snowfall fraction) and snow cover decrease are driven by the temperature increase in a consistent way. I suggest that this is reformulated, because, indeed, the increase in rainfall ratio and the decrease on snow depth, induce potentially divergent effects on ROS days.

We agree, and we have delated the word “contradiction” according to reviewer 2 suggestion.

Page 16, line 368 : « elevation dependent snow sensitivity to temperature change ». This is not a new result, there are multiple reports or publications addressing this issue (e.g. Hock et al., 2019, and Kotlarski et al., 2022, for the European Alps). In fact, this also shows that there is no need for an elevation dependent warming to see elevation dependent changes in snow conditions, as discussed earlier in this review.

Changed. We refer to “closer isothermal conditions”.

Page 16, line 360 : A Discussion section generally introduces a discussion of the limitations of the method used for the study. This is currently lacking from the Discussion section, and I think this should be addressed. Examples of topics for discussion include the relevance of the delta change method, compared to methods directly using climate change projections from regional climate model experiments (again, the corresponding data has been made available for the Pyrenees on the Drias climate data portal, see above). The discussion could also refer to the influence of the snow cover model used for the analysis.

Thanks, we have added the following text where we discuss the limitations of the reanalysis dataset, climate projections and sesntivity studies, providing answer to Reviewer 2 questions:

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5.5 Limitations

This study evaluates the sensitivity of ROS responses to climate change, enabling a better understanding of the non-linear ROS spatiotemporal variations in different sectors and elevations of the Pyrenees. Instead of presenting diverse outputs from climate model ensembles (López-Moreno et al., 2010), we provide ROS sensitivity values per 1ºC, making them comparable to other regions and seasons. The temperature and precipitation change values used in this sensitivity analysis are based on established climate projections for the region (Amblar-Francés et al., 2020). However, precipitation projections in the Pyrenees exhibit high uncertainties among different models, GHGs emission scenarios, and temporal periods (López-Moreno et al., 2008).

The SAFRAN meteorological system used in this work relies on a topographical spatial division and exhibit and accuracy of around 1 ºC in Ta and around 20 mm in the monthly cumulative precipitation, with largest uncertainities found at high elevations (Vernay et al., 2022). Precipitation phase partitioning methods are also subject to uncertainties under close-to-isothermal conditions (Harder et al., 2010). Finally, the FSM2 is a multiphysics snowpack model that has been implemented and validated previously in the Pyrenees (Bonsoms et al., 2023) and compared against different snowpack models (Krinner et al., 2018), providing evidence of its robustness.

Page 19, line 463 : The section on ROS socio-environmental impacts and hazards provided interesting context, but does not discuss the results of this specific study. I suggest providing this information in a condensed way, rather in the Introduction, because it provides context and motivation for the study, than in the Discussion, because it does not build on the results of this particular study.

We strongly believe that it is crucial to mention the impacts on the ecosystem, socio-economic aspects, and natural hazards because of the scope of *Natural Hazards and Earth System Sciences.* Please refer to our previous response provided at the beginning of the review.

Page 20, line 501 : Again, please indicate what « low », « mid » and « high » elevation refer to.

We have changed the manuscript following your suggestion.

Page 20, line 509 : I don't see any counterintuitive factor in the study. It is quite obvious, as indicated above, that rainfall fraction and snow cover evolve in different directions, and it is relevant to assess changes in ROS, which is indeed a compound of snow cover state and rainfall. But this is not counterintuitive. Here some of the results are provided in general terms for the entire mountain range, which supports the suggestion before that some results could also be provided for the entire mountain range, in addition to the sub-regional analysis.

We have delated the word "counterintuitive" according to Reviewer 2 comment.

One of the key findings of this study is the variation in ROS depending on the sector and month. The sensitivity of ROS to climate warming exhibits a skewed distribution: ROS frequency increases for small increments but decreases thereafter. It is therefore not recommended to average the values for the entire range, as doing so would reduce most of the statistical variability.

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Page 20, line 518 : It should be discussed here that the increase in ROS rainfall amount (I suggest, btw, changing ROS rain to ROS rainfall amount, this will be clearer) is not due to any change in climate conditions such as Clausius-Clapeyron effect on precipitation amount, but is only a direct consequence of the influence of temperature on the precipitation phase (what is the threshold used, btw ?), which leads to more cases of rainfall corresponding to previous cases of snowfall under a colder (reference) climate, at potentially different periods of the year. This is another point, which could be discussed in the Discussion section, as it is a limitation of the delta change approach with respect to the topic addressed in this study.

1.- The revised version includes the increase of precipitation that Reviewer 2 mentioned, and the Figure S1 that Reviewer 2 and described at the very beginning of the results section:

"HS and Sf response to temperature and precipitation is shown in Figure 2. Seasonal HS and Sf variability is mostly controlled by the increment of temperature, season, elevation, and spatial sector (Figure S1). The role of precipitation variability in the seasonal HS evolution is moderate to low (Figure S2 to S4). Only in high elevation an upward trend of precipitation (at least > 10%) can counterbalance small increments of temperature (< 1ºC, over the baseline climate) from December to February (Figure S4). For this reason, precipitation was excluded to further analysis".

2.- We have added a section of limitations (please, see Reviewer 1 response).

3.- We have changed "ROS rain" for "ROS rainfall amount" following your suggestion

4.- The threshold used is defined in the "Data and methods" section:

"…..baseline climate (1980 – 2019) and several climate perturbed scenarios (c.f. Sect. 3.4**). Sf was quantified using a threshold-approach. Precipitation was snowfall when temperature was < 1 ºC according to previous ROS research in the study zone (Corripio and López-Moreno, 2017) and the average rain-snow temperature threshold for the Pyrenees** (Jennings et al., 2018)**.** Snow cover is calculated by a linear function of snow depth, snow albedo is estimated based on a prognostic…."

Typos : I noticed some typos in the text, they can be identified by running a proofreading software through the text.

Thank you for your recommendation, we have carefully checked and corrected the found typos across the manuscript.