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Dear Dr. Haegli,

I am submitting the revised version of the manuscript entitled "**Rain-on-snow responses to a warmer Pyrenees: a sensitivity analysis using a physically-based hydrological model**", co-authored by myself, Dr. Juan Ignacio López-Moreno, Dr. Esteban Alonso-González, Dr. César Deschamps-Berger and Dr. Marc Oliva.

We want to express our sincere gratitude for your recommendations and feedback.

We have corrected the manuscript following Reviewer 2 suggestions. Please, find a point-by-point answer to Reviewer 2 comments in the following pages.

We expect to fulfil the expectations and we hope the new manuscript version is suitable for publication in **Natural Hazards and Earth System Sciences.**

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 1: General Comments

There is a confusion here. The work presented is interesting and original, but it could be introduced and placed in context in a much more concise way, for example using the IPCC assessment as a starting point. I'm disapointed that the authors still quote over 100 references for this work. This makes the reading of the papier very cumbersome, and diverts the attention of the reader to the true novelty of the results.

Yet the introduction remains full of citations, and the key results from the IPCC are in fact not referred to.

We have changed the introduction following reviewer 2 suggestion.

Now the introduction has a (1) paragraph about ROS definition and its natural and hazards impacts (2) ROS past and future evolution (including a quote to IPCC, 2019) and why we consider it is important to analyze ROS patterns (3) objectives of this work and its structure.

According to Reviewer 2, we have removed the following paragraph to avoid divert the attention of the reader:

"…To date, research has been focused on the ROS predictability (Corripio and López-Moreno, 2017), detection and validation methods through remote sensing (Bartsch et al., 2010) and models (Serreze et al., 2021). Several works have examined ROSfrequency from the climatological point of view, by analyzing ROS spatial-temporal patterns for Alaska (Crawford et al., 2020), Japan (Ohba and Kawase, 2020), Norway (Pall et al., 2019; Mooney and Li, 2021) or the Iberian Peninsula mountains (Morán-Tejeda et al., 2019). ROS events have also been linked with Northern-Hemisphere and Arctic low-frequency climate modes of variability (Rennert et al., 2009; Cohen et al., 2015) as well as synoptic weather types (Ohba and Kawase, 2020). Further, several works in mountain catchments of Switzerland (Würzer et al., 2016), Germany (Garvelmann et al., 2014a), United-States (Marks et al., 1992), Canadian Rockies (Pomeroy et al., 2016) or Spain (Corripio and López-Moreno, 2017), have portioned the contribution of Surface Energy Balance (SEB) components during ROS events…"

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Manuscript first round of review:

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"***

I don't agree.Indeed, climate change can lead in some cases to ROS in situations where it did not occur before, but also change the pattern of ROS occurrence where it already occured.

The change implemented does not clarify this. the mention "where it did not occur before" should be added, if this is what the authors mean here implicitly.

We have changed the manuscript accordingly:

"…Climate warming, however, is modifying mountain snowfall patterns (IPCC, 2022), through temperature-induced precipitation changes from snowfall to rainfall (Lynn et al., 2020), leading in some cases to rain-on-snow (ROS) events in snow covered areas where it did not occur before."

the term "ROS drivers" was still found in the manuscript. Please ensure that it is fully deleted if that is the intent.

We have deleted this term.

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Manuscript first round of review:

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).

Not everywhere, please check again.

Sorry for the error.

We have carefully checked again the manuscript to ensure that the historical climate period (1980 – 2019) is expressed in each figure caption. We mentioned the historical climate period years for the first time in the introduction and then in the conclusion section.

Manuscript first round of review:

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 (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.

yet some references remain in the revised manuscript, related to SAFRAN-France

We have changed the manuscript following your suggestion.

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Still it is very surprizing that they are not equally spaced. This induces some bias. In this case, if the authors want to keep 1500, 1800 and 2400, I suggest they add 2100 too, which would provide a more continuous range of elevation bands, without "jump".

We preferred to show a large range of elevations at the expense of the elevation continuity in order to represent most of the ROS variability, while keeping it concise and ease the reader's interpretation.
Since we analyzed the results based on many variables (three elevation bands, four sectors for three indicators, depending on seasons and increments of temperatures), we fear that including another elevation band would make it even more difficult to understand all the information we provide.

Manuscript first round of review:

The role of precipitation variability in the seasonal HS evolution is moderate to low (Figure S2 to S4).

This should be double checked. I agree it is a good idea to refer to it in each figure caption.

We have added:

"For this reason, precipitation was excluded from further analysis, and the ROS sensitivity analysis is evaluated for the average change of precipitation"

Manuscript first round of review:

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.

this was changed for some figures, but not all. Figure 4 is still affected by the problem.

We have changed the figures according to your suggestion.

yet the conclusions section states that "During the baseline climate period, annual ROS frequencytotals on average 10, 12 and 10 day/season for 1500 553 m,1800 m and2400 m

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elevations. " this is exactly the kind of information that could be provided in the Results section (also for other indicators), before of course focusing on the results for subregions.

Done. We have moved this information to the results section:

"..During the historical climate period (1980 – 2019), annual ROS frequency totals on average 10, 12 and 10 day/season for 1500 m, 1800 m and 2400 m elevations. However, there are large differences depending on the sector."

Manuscript first round of review:

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.

the palette remains divergent, although a positive variable is plotted. This therefore remains inadequate. A sequential palette is needed.

Done. We have used a sequential palette (PuBu from ColorBrewer)

Manuscript first round of review:

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.

This is incorrect and at odds with the fundamentals of graphical visualization.

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A palette can either be divergent or sequential. See e.g. Figures 5 and FIgure 6 Crameri et al., (Nature, 2020). Here it should be sequential, but this does not mean that it has to use discrete values (continuous values are plotted).

Done. We have used a sequential palette (OrRd from ColorBrewer) in this version.

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.

Unfortunately, the term" contradiction" remains in the revised manuscript;

Done. We have deleted this word.

Unfortunately, the terme "counterintuitive" remains in the revised manuscript.

Done. We have deleted this word.

Typos:

Abstract

“…based snow model forced with reanalysis climate data perturbed following 21st century climate projections for 6 this mountain range.”

Changed to “ using a range of values of temperature and precipitation changes consistent with 21st century climate projections”.

“..this mountain range. ROS patterns are **characteritzed** by their”

Changed for **characterized**

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"lasting snow cover exists until late spring. Similarly, warming **triggers fast** ROS ablation(+ 10% per ºC)"

Changed for "induces greater"

the introduction is still full of (too) many references, and does not follow the recommendation by the reviewer to concisely introduce the topic, using IPCC state of the art as a starting point.

I still counted above 100 references for this article, I don't think this is a reasonable number.

We have changed the introduction following reviewer advise:

"…is analyzed using a physically based snow model, forced with reanalysis climate data perturbed according to 21stcentury climate **projections spread for range** (Amblar-Francés et al., 2020)"

Suggested to be reformulated.

Changed to : "using a range of values of temperature and precipitation changes consistent with 21st century climate projections"

"…we analyze height of snow (HS) and snowfall fraction (Sf) responses to temperature and precipitation since these are the main drivers of ROS".

Suggested to be reformulated without the term "drivers"

Changed to:

Done, changed to: "First, we analyze height of snow (HS) and snowfall fraction (Sf) responses to temperature and precipitation since these are the main variables that control ROS events."

L80: average over what space and time domain ?

We have modified the paragraph where this information is found:

Here we examine the ROS sensitivity to temperature and precipitation change for low (1500 m), mid (1800 m) and high (2400 m) elevations of the Pyrenees. ROS responses to temperature and precipitation is analyzed using a physically based snow model, forced with reanalysis climate **data (1980 to 2019 period)** perturbed according to a range of temperature and precipitation

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changes consistent with 21st century climate projections for the mountain range (Amblar-Francés et al., 2020).

Generally, snow ablation starts in February **inlow** elevations and in May at high elevation

"at low"

Changed.

Figure 1.(a) Pyrenean massifs sectors (colors) for 1500 m,1800 m and 2400 m elevation. (b) Principle Component Analysis (PCA)scores of each massif for 1500 m,1800 m and 2400 m elevation.

I don't understand what this figure represents. Based on what is the PCA calculated ? Temperature ? PRecipitation ? Snow depth ? Other ? Over what time period ? Figures should be almost self supporting in terms of content, and a concise yet precise caption is required.

Added (bold):

Figure 1. (a) Pyrenean massifs sectors (colors) for 1500 m, 1800 m and 2400 m elevation. **Massifs were classified according to a Principal Component Analysis (PCA) applied over monthly HS data of each massif and elevation range for all months and years of the baseline climate.** (b) PCA scores of each massif for 1500 m, 1800 m and 2400 m elevation. The black numbers are the SAFRAN massif's identity numbers defined by Vernay et al. (2022). Note that **2400 m elevation** does not include massif number 64 since this massif does not reach **that elevation range.**

Snowpack is modeled using the energy and mass balance snow model FSM2 (Essery, 2015).

"The".

Added: "…The snowpack is modeled using the energy and mass balance"

The FSM2was forced at hourly resolution for each massif and elevation range (c.f. Sect. 3.3) for the baseline climate (1980 –2019) according to climate projections (c.f. Sect. 3.4).

I don't understand the sentence here. No climate projection was used, as far as I understand.

Changed to :

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"(c.f. Sect. 3.3) for the baseline climate (1980 – 2019) and perturbed using a range of values of temperature and precipitation changes consistent with 21st century climate projections (c.f. Sect. 3.4)"

Snow cover is calculated by a linear function of snow depth, snow albedo is estimated based on a prognostic function with the new snowfall

I don't understand. What does "snow cover" means ? "Snow cover fraction" ? Other ?

Added: "Snow cover fraction"

The detailed FSM2 physical parameters and Fortran compilation numbers are shown in Table S1.

what does this mean ?

Changed for: "The FSM2 configuration selected shown in Table S1."

SAFRAN system was firstly designed for avalanche monitoring (Durand et al., 1999, 2009)

"hazard forecasting"

Added.

"…results obtained enhanced the diffusion of the meteorological system and its integration in the French hydrometeorological modelling system by the local weather service, Metéo-France (Habets et al., 2008).

In my review comments I explicitly mentioned that this is not the same implementation of SAFRAN. Further, I don't see the point of providing such information given the scope of this manuscript.

Removed.

A PCA was applied over HS data for all months and years of the baseline climate. Massifs were this information should be provided in the caption of FIgure 1

Added.

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"ROS sensitivity to precipitation, Ta, 176 increasing incoming longwave radiation (Lwin) accordingly. This method…"

Which method ?

Changed to:

Sf, HS and ROS sensitivity to air temperature and precipitation is analyzed by perturbing climate data (López-Moreno et al., 2013; Pomeroy et al., 2015; Marty et al., 2017; Musselman et al., 2017b; Rasouli et al., 2019; Alonso-González et al., 2020a; López-Moreno et al., 2021).Specifically, SAFRAN reanalysis climate data was perturbed according to Spanish Meteorological Agency air temperature and precipitation projections for the 21st century in the Pyrenees (Amblar-Francés et al., 2020).

SAFRAN reanalysis climate data was perturbed according to Spanish Meteorological Agency climate change scenarios projected for the **21st Century** in the Pyrenees

Changed to:

SAFRAN reanalysis climate data was perturbed according to Spanish Meteorological Agency climate change scenarios projected for the **21st century** in the Pyrenees.

Ta (ºC) was perturbed from +1ºC to +4ºC by +1ºC.

"Steps of "

Added: "Ta (ºC) was perturbed from +1ºC to +4ºC, by steps of +1ºC.

A temperature increase of 1ºC can be interpreted as an **optimistic projection** for the region, while 2ºC and 4ºC would represent projections for mid and high emission scenarios, respectively (Pons et al., 2015).

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"optimistic" is not an adequate term for referring to climate projections. Those are driven by emissions, not by chance.

Changed to: "…A temperature increase of 1ºC can be interpreted as a low emission scenario for the region, while 2ºC and 4ºC would represent projections for mid and high emission scenarios, respectively (Pons et al., 2015).."

The average HS and Sf sensitivity to temperature and precipitation (**expressed in % per ºC**)

"… of local warming

Added : "..HS and Sf sensitivity to temperature and precipitation (**expressed in % per ºC of local warming**)

ROS exposure is the relation between ROS rainfall amount(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 temperature.

I don't understand the term "exposure" here. Is this relationship expressed as a ratio ? How is it quantified ?

Deleted to avoid confusions.

it is not a record (observation) but the result of a simulation. I suggest reformulating, perhaps replacing here by "analyzed"

Changed.

"…ROS frequency is recorded for all increments of **temperature".**

Sentence deleted.

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"….We provide an analysis of ROS drivers, near-present ROS patterns and their response to warming".

please state simply what these are here, it will be easier to read.

We have deleted the term drivers everywhere.

Changed to: "….We provide an analysis of Sf, HS, and ROS patterns response to temperature and precipitation change".

"…ROS spatio-temporal dynamics are analyzed **by** frequency, rainfall quantity and snow ablation".

in terms of

Changed to: "….ROS spatio-temporal dynamics are analyzed in terms of frequency, rainfall quantity and snow ablation."

"…intuitive **ROS sensitvity** to temperature"

Changed: "ROS sensitivity"

"…ROS indicators values are shown for each increment of temperature, grouped by elevation and sectors, namely SW, SE, NW and NE."

as a function of the change in temperature and precipitation amounts.

Changed.

"…The role of precipitation variability in the seasonal HS evolution is moderate to1500 m (Figure S1to S3).

please reformulate

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"….The role of precipitation variability in the seasonal HS evolution is moderate to low (Figure S1 to S3)."

We have added the information in each Figure caption:

"Data are the average of the simulated precipitation change (from -10% to 10%, by steps of 10%)."

"…Only **in 2400** m elevation an upward trend of precipitation (at least > 10%) can counterbalance small increments of"

Only at
Changed.
consistent with previous results (e.g. Martin et al., 1994).
Thanks. Added in the discussion section.

L433: "…..Similarly, Sf decreases by 29 %, 22 %, and 12 % per ºC for 1500 m, 1800 m, and 2400 m elevations. These results provide evidence of an elevation-dependent snow sensitivity to temperature change and are consistent with snow sensitivity to climate works in near alpine sectors, such as the Alps (e.g., Martin et al., 1994)…."

Martin, E., Brun, E., and Durand, Y. (1994). Sensitivity of the French Alps snow cover to the variation of climatic variables. Annales Geophysicae 12, 469–477.

"…elevation an upward trend of precipitation (at least > 10%) can counterbalance small increments of temperature (<1ºC, over the baseline climate) from December to February. Previously it is stated that the sensitivity analysis is performed by steps of 1°C. HOw is it possible to analyze changes for differences strictly less than 1°C ?

We did not consider analyzing the sensitivity to changes lower than 1ºC. We have modified the manuscript L198 (" Air temperature (ºC) was perturbed between +1ºC and +4ºC by steps of +1ºC")

"…Snow **in 1500 m** and1800 m elevations during summer is rarely"

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Snow at 1500 m

Changed.

"… season (autumn and spring), **coinciding with the time when ROS events are more frequent**

At this stage, results about ROS are not provided yet. I suggest keeping this to a later stage of the provision of the results."

Sentence deleted.

"…are modeled for spring when temperature is + 1ºC. The greatest HS decreases in2400 m elevation areas are **modeled"**

Simulated

Changed.

"…4 ºC), seasonal HS is reduced 92%, 89%, and 79 % for **low,** 1800 m, and 2400 m elevations, respectively (Figure S4)."

Changed: "…4 ºC), seasonal HS is reduced **by** 92%, 89%, and 79 % for **1500 m,** 1800 m, and 2400 m elevations,

"…respectively **of the range considered** (Figure S4).".

We consider respectively (without *of the range considered*) is correct.

"…Figure 2. Height of snow (HS) (lines) and Snowfall fraction (Sf) (bars) monthly variation for baseline climate scenario and different increments of temperature (colors) grouped by elevation (rows) and sectors (columns).

the authors insist that a sensitivity experiment is a different exercise than climate projections, hence the term "scenario" should be left out altogether, otherwise this induces confusion. Furthermore, the baseline is produced based on a reanalysis, not a climate model output.

We have deleted the word "scenario" everywhere following your suggestion.

We have changed "baseline climate" and "baseline scenario" to "historical climate period" following your suggestion.

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The caption says nothing about the meaning of the -10%, 0 and +10%. I guess this relates to the change in precipitation, but this shoud not be left for the reader to guess.
The caption should also warn the reader that the y-axis is inverted for the snowfall fraction, compared to that of the snow depth.

Changed to :"Height of snow (HS) (lines) and Snowfall fraction (Sf) (bars) monthly variation for the historical climate period (1980 – 2019) and different increments of temperature (colors) grouped by precipitation change (columns) and elevation (rows). Note that Sf values (y-axis) are inverted"

by 29%, 22 %, and 12 % per ºC for **low,** 1800 m, and 2400 m elevations, respectively

1500 m

Changed to: "by 29%, 22 %, and 12 % per ºC for **1500 m,** 1800 m, and 2400 m elevations, respectively.

An increase of 4ºC **supposes** Sf reductions of …

Leads to

Changed to : "An increase of 4ºC **lead to** Sf reductions of …"

Low elevation annual ROS frequency for the baseline climate is 17, 8, 10 and 7 days/year …

1500 m

Changed to : "1500 m annual ROS frequency for the baseline climate is 17, 8, 10 and 7 days/year …"

it should be reminded once in a while in the paper, what this "baseline climate" is, for better clarity.

Changed "Baseline climate" to "historical climate period (1980 - 2019)"

L266 : "…which contrast with the **modeled** values for SW (2 and 3 days/month, for1500 m and1800 m elevations, respectively)."

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Changed to "…which contrast with the **simulated** values for SW (2 and 3 days/month, for1500 m and1800 m elevations, respectively)."

Figure 4:

this remains problematic in terms of visualization. Please ensure that the size of the bars remains the same regardless of the number of bars. "0" values seem to not be handled adequately for the production of this graph. This should be fixed.

Corrected.

L279: ROS tends to disappear in October for 1500 m elevation except in SW (Figure 4and 5). The

for which level of warming ?

Changed to "ROS tends to disappear in October for 1500 m elevation for + 1ºC, except in SW (Figure 4 and 5)."

Figure 5 this scale is fully inadequate.
Sorry, but it does not make any sense to use a diverging color palette ranging from 0 to 4, centered on "2".

Changed to a sequential scale following reviewer suggestion.

" …(around 40mm/day in 1500 m and 1800 m elevations."

Corrected: "(around 40mm/day in 1500 m and 1800 m elevations**)** "

Figure 6:
why are the colors different to Figure 4, although they show the same thing (degree of warming) ? Please homogenize, this will be clearer for the reader.

Changed following reviewer suggestion.

"…Figure 6. ROS rainfall amount(mm) temporal evolution for baseline climate (1980-2019) and increment of warming (colors), grouped by elevation (columns) and sector (rows).

Annual mean

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Response: Changed to “Average ROS rainfall amount (mm/day) for each month of the season. Data are shown for the historical climate period (1980 – 2019) and different increments of temperature (colors), grouped by month (x-axis), elevation (columns) and sector (rows).”

L315: ROS rainfall amount progressevly increases.

Progressively

Corrected

due to warming (4%, 4%, and 5% per ºC for low, 1800 m, and 2400 m elevations, respectively; Table S2).

Corrected: “due to warming (4%, 4%, and 5% per ºC for **1500 m**, 1800 m, and 2400 m elevations, respectively; Table S2).”

Modeled

Simulated

Corrected.

“ROS exposure”

the term is unclear, and does not correspond to the meaning of the term "exposure" in climate sense (see IPCC glossary, for example).

We have delated the term “exposure” and was changed for ROS rainfall amount and frequency

Nonetheless, **remarckable** spatial and seasonal differences are found.

Corrected

On the contrary, small changes in frequency are detected in SW and NW, despite ROS rainfall amount is expected to increase (<10mm/day).

what is the meaning of the "<" sign here ?

Less than

Figure 8: it would make a lot more sense if the elevation were arranged in the other direction (1500 m at the bottom, 2400 at the top).

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Note that this is the case for Figure 2, hence I don't understand why a different choice was made here. This is very confusing for the reader.

Corrected for all the figures.

Figure 8. Average ROS exposure. Again, what does it mean.

In the text, there are mentios of "increasing ROS expsure". But what is the equation defining the concept ? And again, the term is at odds with standard use of the term (see IPCC glossary).

We have delated the term “exposure” and was changed for ROS rainfall amount and frequency

Figure 9: again, ordering of elevations should match that of Figure 2 (lower elevation at the bottom, higher elevation at the top).

Done

Figure 10: Snow ablation is expressed in cm in the text, please homogenize the units (I suggest using cm throughout, to avoid, as much as possible confusion with changes in SWE, which would be expressed in mm w.e. typically)

Corrected. The units are cm/day.

“…The contradiction between rainfall ratio increases and snowpack 391 reductions, as well as the 2400 m spatial and monthly differences found, explain the complex ROS response 392 to warming. HS decrease by 39 %, 37 % and 28 % per ºC, for..”

This is not a contradiction, as mentioned in the previous review (and that the authors agreed in their reponse). However these are opposed trends, whose result may not be unequivocal, as shown in the paper. But this is not a contradiction (apparent inconsistency)

Corrected. Sentence removed.

“…. Maximum ROS frequency for a season are found in SW and NW because of larger snow magnitudes in this sector …”

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what is a "snow magnitude" ? Please clarify.

Changed to “snowpack magnitude.”

“…The generally ROS rainfall amount increase reported in this work(independently of the increment of temperature and elevation)is explained by the Sf reduction expected for all sectors (Figure 3).”

and the fact that this increase supersedes the effect of the snow cover reduction (i.e., even with snow cover reductions, the snow cover does not fully disappear hence ROS increases due to increases in rainy days).

We added this information:

“…..The generally ROS rainfall amount increase reported in this work (independently of the increment of temperature and elevation) is explained by the Sf reduction expected for all sectors (Figure 3). Large increments of warming decreases ROS frequency due to snow cover depletion in early autumn and late spring (Figure 2). However, for the rest of the seasons even with snow cover reductions, the snowpack does not fully disappear leading to ROS frequency increases due to more rainy days…”

“….In the latter sectors, ROS rainfall amounttends to dissapear in Octuber under large (>. In the latter sectors, ROS rainfall amounttends to **dissappear** in Octuber under large…”

Changed to disappear.

2400 m elevation **show** the largest variation over the baseline climate as well as

Shows

Corrected.

at a time where a snow cover may have already developed at sufficiently high elevation.

Added.: “..when snow cover may have already developed at sufficiently high elevation.”

Title: 5.2 ROS temporal evolution

the title of this section could rather read as "COmparison with other studies"

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Changed

"….trend ROS due to warming, which is consistent with the different ROS rainfall amountand frequency responses **depending on** the increment of temperature detected in our work"

Depending in

Corrected.

is already near to the isothermal conditions. These results go in line with results **modelled** for cold and warm

Obtained

Changed

"….Nevertheless, data show 1500 m or decreases in ROS ablation in SE and spring since the snowpack is already near to the isothermal conditions."

Please, reformulate.

Changed to: "…Nevertheless, data show no-changes or decreases in ROS ablation in SE and spring since the snowpack is already near to the isothermal conditions."

"…Therefore lower ROS ablation is **direclty** affected by lower HS magnitudes. "

Changed.

"…The expected decreases in **Sf and HS** due to climate warming will likely change ROS "

these should be spelled out once in a while, for better clarity. Especially at the beginning of the conclusion section.

Changed to "The expected decreases in snowfall fraction (**Sf) and height of snow (HS)** due to climate warming will likely change ROS"

During the baseline climate period, annual ROS frequency totals on average 10, 12 and 10 day/season for 1500 553 m,1800 m and2400 m elevations.

is this for the full Pyrenees ? If so, this is a result, that is interesting, but should be introduced in the Results section (this is what I suggested in the previous review, actually).

We have added this information in the results section following your suggestion:

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"During the historical climate period (1980 – 2019), annual ROS frequency totals on average 10, 12 and 10 day/season for 1500 m, 1800 m and 2400 m elevations. However, there are large differences depending on the sector."

"….ROS frequency is highlty sensitive to warming in"

Corrected.

when **counterintuitive** factors play a key role.

why counterintuitive ?
I agree these are "contrasted", "diverging", "opposed" factors, but not "counterintuitive".

Corrected to "diverging"

"…On the one hand, **maximum** Sf decreases are **modeled** for spring, leading to rainfall increases…

Changed "maximum" to "largest" and "modeled" to "simulated"

"…Increases in cold snowpacks, such as those modeled in 2400 m"

Changed to "increases in cold snowpacks, such as those **simulated** in 2400 m"