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Barcelona, 6th January 2023

Dear Dr. Helbig,

I am submitting the revised version of the manuscript entitled "**Snow sensitivity to climate change during compound cold-hot and wet-dry compound seasons in the Pyrenees**", co-authored by myself, Dr. López-Moreno and Dr. Alonso-González.

We want to express our sincere gratitude for the time you expended reviewing the manuscript, your constructive recommendations and positive feedback.

We would also like to thank the referee's recommendations, which helped to improve the manuscript and added scientific rigor to the research presented here.

The main manuscript modifications are summarized as follows:

(I) We have changed the manuscript according to your suggestions. We have changed Figure 4 absolute for relative values. Also, we have carefully checked that the submitted manuscript corresponds to the track change version of the revised manuscript.

(II) We have included referee 1 methodological correction. We defined the seasonal peak HS date after applying a moving average of 5-days to resolve the issue mentioned.

(III) We have included most of referee 2 suggestions. We have carefully addressed the referee 2 comment about the joint-quantile approach. We have statistical classified the Pyrenean zones, added more data, a new results section and three figures. We have evaluated the snow sensitivity to temperature and precipitation differences due to the Pyrenean sectors, and the number of compound season types recorded during the baseline period. However, no remarkable differences in the relative importance of each compound season type were found between sectors. This is because by applying a joint-quantile approach for each massif and elevation, we are standardizing the climate of the Pyrenean massifs. We are comparing the snow sensitivity to temperature and precipitation change during similar climate seasons, independently where a climate season type was recorded.

A point-by-point answer to reviewer's comments can be found in the following pages.

We expect to fulfil the expectations and we hope the new manuscript version is suitable for publication in **The Cryosphere**. I will be happy to answer any question you might have regarding this work.

Many thanks, kind regards and happy new year,

Josep Bonsoms, on behalf of the co-authors.

Response to second review of "Snow sensitivity to temperature and precipitation change during compound cold-hot and wet-dry seasons in the Pyrenees"

by Josep Bonsoms[1], Juan Ignacio López-Moreno[2] and Esteban Alonso[3]

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Corresponding author: J.I López-Moreno (nlopez@ipe.csic.es)

Response to Reviewer 1.

Reviewer comments are in black and responses in blue.

Thanks to authors for the clear and concise reply. The paper improved a lot. I like the updated Figure 4, but the new Figure 7 sheds light on a problem in Figure 4. The shown seasonal evolution of HS in Figure 4 reflects a false accuracy of model results, which are than translated into the interpretation of e.g. Figure 7, especially for the WW compound extreme. There, a clear later peak HS date only for mid elevation under 1°C or 2°C warming (or the positive sensitivity for the same case in Table 4) make not much sense.

Therefore, I'd suggest applying a smoothing filter to HS evolution in Figure 4 in order to prevent to have several peaks.

Thank you very much for your suggestion.
We have applied your suggestion, and we have changed the peak HS date quantification to prevent several peak HS dates per season. The peak HS date is now determined after applying a 5-day moving average, which resolves the issue you mentioned.
Figure 4 is also changed following editor recommendation.
We have added:
"Some seasons had more than one peak HS; for this reason, peak HS date was determined after applying a moving average of 5 days. All indicators were computed according to massif and elevation range."

Response to second review of "Sensitivity to temperature and precipitation change during compound cold-hot and wet-dry seasons in the Pyrenees"

by Josep Bonsoms[1], Juan Ignacio López-Moreno[2] and Esteban Alonso[3]

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Corresponding author: J.I López-Moreno (nlopez@ipe.csic.es)

Response to Reviewer 2.

Reviewer comments are in black and responses in blue.

Dear authors, dear editor,

The revised version of the manuscript addresses some of the points raised in the first round of review. The method is better detailed, a section on limitations and uncertainty has been added, and the language has been improved. However, I still have some concerns. One is about the vocabulary used, which can easily be corrected, and a more fundamental one about the method, or rather how the results are discussed, which was already raised in the first round of comments and remained unanswered. While this may require more work than was done for the first iteration, I really encourage you to address this issue to increase the robustness of your results.
Best regards,
Adrien Michel

Important note: The updated manuscript does not agree with the track change version! E.g. P2L36 " … and **increasing** surface and air temperature …" in the manuscript, and " … and **reducing** surface and air temperature**s** …." in the track change version. The last sentence of the conclusion also differs (I did not check further). My comments are based on the updated manuscript. The author should carefully check on which version they are working on for further edits.

We would like to thank your time expended reviewing the manuscript, and your constructive suggestions.

We have followed your suggestions and we have corrected the vocabulary, modified the text and figures. We have also added more data and figures to evaluate the regional differences induced by the joint-quantile approach.

A point by point answer to reviewer 2 recommendations can be found in the following lines.

Vocabulary

There are been many improvements in the usage of the term "sensitivity" compared to the first version. However, in many locations, the word "sensitivity" is still used alone, while "sensitivity of snow to climate change should be used". E.g. section 4.2 is called "snow sensitivity", while it should be "snow sensitivity to climate change", or more precisely "Snow sensitivity to change in air temperature and precipitation".

We have followed Reviewer 2 suggestion; we have changed "sensitivity" alone to snow sensitivity to temperature and precipitation. Also, we have changed "snow sensitivity to climate change" for "snow sensitivity to temperature and precipitation change" through the manuscript, and we have modified the manuscript title.

Abstract:

"…Here, we perform a snow sensitivity to temperature and precipitation change analysis of the Pyrenean snowpack (1980 – 2019 period) using five key snow-climatological indicators.".

Introduction:

"…Therefore, the main objective of this research is to quantify snow (accumulation, ablation, and timing) sensitivity to temperature and precipitation change during compound temperature and precipitation seasons in the Pyrenees."

Method:

In this method, air temperature and precipitation were perturbed for each massif and elevation range based the historical period (1980–2019). Air temperature was increased from 1 to 4ºC at 1ºC, assuming an increase of $\mathrm{LW_{inc}}$ accordingly.

In the text, you mainly use "extreme compounds seasons", while in the title you use "compound cold-hot and wet-dry seasons" only. While the term "extreme" could be disputable here (based on percentile 60), this is not unique in the literature (despite usually higher percentiles are used), so it is fine. However, for consistency you should maybe also use “extreme” in the title.

We have followed your suggestion and we have included “compound cold-hot and wet-dry seasons” in the title. We have deleted the word “extreme” according to your suggestion.

The term “ablation” is often used alone, while I think you are most of the time referring to “ablation rate”, this should be corrected. Also, it is not clear if you talk about absolute ablation rate (cm/day) or relative ablation rate (in %/day or %/°C). Both of them are relevant, but it should be clarified to which one you refer to.

We have defined in the methodological section snow ablation : snow ablation is the difference between the maximum daily HS recorded on two consecutive days.

In the text: “…daily average snow ablation per season (snow ablation, hereafter)”. Subsequently, we have applied the term “snow ablation” through the manuscript accordingly.

Analysis description

You added some details on P6L170-178. However, we still do not know exactly which model setup you used. If I want now to reproduce your work, I need the exact name of the models’ parameters. I Imagine these parameters are chosen at compilation time form the compile.sh file (https://github.com/RichardEssery/FSM2/blob/master/compil.sh). A table in supplementary with the exact names (and maybe the exact version of the model, i.e. git commit number) is necessary for reproducibility.

Thank you for your suggestion.

We have followed your recommendation and added a table in supplementary material with the physical configuration and compile numbers.

In the methodological section we have added: “…. Snowpack was modelled using a physical-based snow model, the Flexible Snow Model (FSM2; Essery, 2015). This model resolves the SEB and mass balance to simulate the state of the snowpack. FSM2 is open access and available at https://github.com/RichardEssery/FSM2 (last access 16 December 2022). Previous studies tested the FSM2 (Krinner et al., 2018), and its application in different forest environments (Mazzoti et al., 2021), and hydro-climatological mountain zones such the Andes (Urrutia et al., 2019), Alps (Mazzoti et al., 2020), Colorado (Smyth et al., 2022), Himalayas (Pritchard et al., 2020), Iberian Peninsula Mountains (Alonso-González et al., 2020a; Alonso-González et al., 2022), Lebanese mountains (Alonso-González et al., 2021), providing confidential results. The FSM2 requires forcing data of precipitation, air temperature, relative humidity, surface atmospheric pressure, wind speed, incoming shortwave radiation (SW_{inc}), and incoming long wave radiation (LW_{inc}). We have evaluated different FSM2 model configurations (not shown) without remarkable differences in the accuracy and performance metrics. The FSM2 configuration included in this work estimated snow cover fraction based on a linear function of HS and albedo based on a prognostic function, with increases due to snowfall and decreases due to snow age. Atmospheric stability is calculated as function of the Richardson number. Snow density is calculated as a function of viscous compaction by overburden and thermal metamorphism. Snow hydrology is estimated by gravitational drainage, including internal snowpack processes, runoff, refreeze rates, and thermal conductivity. Table S1 summarizes the FSM2 configuration and the FSM2 compile numbers."

Table S1. FSM2 configuration implemented.

Physics and driving data options	FSM2 Configuration	FSM2 Compile number
Albedo	Prognostic age function	2
Snow conductivity	Function of density	1
Snow density	Function of overburden	2
Turbulent exchange	Richardson number atmospheric stability adjustment	1
Snow hydrology	Gravitational drainage	2
Snow cover fraction	Linear function of snow depth	1

There is no "data and code availability" indicated in the paper. I highly encourage you to publicly share your data.

We have modified the data and methods section. Also, we have added a data availability section following your suggestion.

Data and methods :

"…FSM2 is open access and available at https://github.com/RichardEssery/FSM2 (last access 16 December 2022)."

"…We forced the FSM2 with the open access SAFRAN climate reanalysis dataset described by Vernay et al. (2021)"

Data availability:

Snow model (FSM2) is open access and available at https://github.com/RichardEssery/FSM2 (last access 16 December 2022). Meteorological Forcing data is described by Vernay et al. (2021), through AERIS (https://www.aeris-data.fr/landing-page/?uuid=865730e8-edeb-4c6b-ae58-80f95166509b#v2020.2; last access 16 December 2022). Data of this work is available upon request (contact: josepbonsoms5@ub.edu).

Impact study, determining factors, uncertainty

On P11 L284-285 you say: "The results show a non-linear response between seasonal HS loss and temperature increase." Which is clear from figure 4 and an interesting result. However, later in the analysis, you mainly use linear indicators (in %/°C): e.g. P11-12 L292- 319, Tables 2-3. I think these numbers do not add anything to the analysis and they contradict the non-linearity you found and emphasis in your abstract (saying that the greater relative change is for +1°C). I would recommend to remove the above-mentioned lines and tables. This will make this Section easier to read, without any loss of information. The same information is obtained by commenting the boxplots on Figs 5-6-7. As I already mentioned in the first review round, there are many numbers listed in the text, which are all visible from figures, and not really useful later on in the analysis.

Thank you for your recommendation.

We want to show both (I) the average sensitivity to temperature and precipitation change (% per ºC) and (II) the seasonal decreases by increments of temperature, elevation, season type and regions because:

1. The reader is informed in the abstract and the main text about the seasonal HS non-linearity evolution when temperature is progressively increased:

"When the temperature increased progressively at 1ºC intervals, the largest seasonal HS decreases from the baseline were at +1ºC (47% at low elevation, 48% at mid-elevation, and 25% at high elevation)."

And:

"Our results suggest that warming had a non-linear effect on snowpack reduction. Our largest snow losses were for seasonal HS when the temperature increased by 1ºC above baseline."

2. We used the average sensitivity to temperature and precipitation change (% per ºC) in benefit of the results interpretation. We consider that it is easier to understand for the reader. Details (i.e.,

seasonal losses by increment of each temperature, season type, elevation, etc) can be consulted in the figures.

3. The mean value is comparable between the compound seasons, elevation, and sectors.

4. The average snow sensitivity to changes in meteorological forcing variables (expressed in %/ºC) has been applied and validated in snow hydrology (i.e., Pomeroy et al., 2015; Brown and Mote, 2009; Musselman et al., 2017a; Esteban-Alonso et al., 2020; López-Moreno et al., 2021, etc.). The inclusion of the average allows us to compare to those previous works and make our results comparable in the future. The average (expressed in %/ºC) to changes in model forcing data has been used in other cryosphere topics. Just to name a few:

Anderson, B., and A. Mackintosh (2012), Controls on mass balance sensitivity of maritime glaciers in the Southern Alps, New Zealand: The role of debris cover, J. Geophys. Res., 117, F01003, doi:10.1029/2011JF002064.

Pomeroy J, Fang X, Ellis C. 2012. Sensitivity of snowmelt hydrology in Marmot Creek, Alberta, to forest cover disturbance. Hydrological Processes 26: 1892-1905. doi:10.1002/hyp.9248.

Rasouli K, Pomeroy JW, Marks, DG. 2015. Snowpack sensitivity to perturbed climate in a cool mid-latitude mountain catchment. Hydrological Processes 29: 3925–3940. doi: 10.1002/hyp.10587.

Some of them have been included in The Cryosphere, for instance:

Burke, E. J., Zhang, Y., and Krinner, G.: Evaluating permafrost physics in the Coupled Model Intercomparison Project 6 (CMIP6) models and their sensitivity to climate change, The Cryosphere, 14, 3155–3174. Doi:10.5194/tc-14-3155- 2020, 2020.

Ebrahimi, S., & Marshall, S. J. (2016). Surface energy balance sensitivity to meteorological variability on Haig Glacier, Canadian Rocky Mountains. The Cryosphere, 10, 2799–2819. Doi:10.5194/tc-10-2799-2016

van Pelt, W. J. J., Oerlemans, J., Reijmer, C. H., Pohjola, V. A., Pettersson, R., and van Angelen, J. H.: Simulating melt, runoff and refreezing on Nordenskiöldbreen, Svalbard, using a coupled snow and energy balance model, The Cryosphere, 6, 641–659, doi:10.5194/tc-6-641-2012, 2012.

Reviewer suggestion was not implemented but if the editor considers that we should modify it, we will change the manuscript accordingly.

In the first revision round, I raised this important concern:

“‘I have one concern about the method itself. As far as I understand, seasons “classes” (WW,CW,etc.) are determined for each subregion and elevation range separately (Figure S1). And thus, figures like 4 are obtained by averaging all the regions together for each elevation band and season class. **My problem is that from Figure S1 we see that some classes of season are manly dominated by some regions (e.g. cold wet is dominated by south-west regions).**

So, when comparing the different season class, we do not really know if the difference is due to the meteorological input, or due to some other aspects differing between regions.

In addition, the season class is (maybe?) determined for each region separately (see my comment above), so a CW in one region might not be CW in another region. **As a consequence, because of the approach chosen, I do not think the differences observed between compound seasons is only due to the specific weather of the seasons. This is probably the dominant factor, but the spatial difference would add some uncertainty there. This should at least be discussed**.” Many points have been answered and clarified, but not what is in bold font in the paragraph above (you just answered “We are not comparing season types between massifs”). I'll reformulate here this concern. In Figures 4 to 7, and in most of the analysis, you split the data per elevation range and per season type. In Figure S1, we see that some seasons types occur mor often in some regions (e.g. some regions in the south have no warm-wet, some regions have a total of ~80 extremes seasons, while some have only ~40 in total). As a consequence, when looking at one class of season and one elevation band, the different regions are not represented equally, on some seasons type signal will thus be influenced by the dominant regions in the sub-ensemble (e.g. col-wet mid altitude are dominated by the western regions). In Figures 9-10 you show that the response to climate change is different between region. So, when in the end you assess the change for a season type and an elevation band (e.g. col-wet mid altitude), we do not know if the response is more dependent on the season type, or on the region which is dominating this subset.

Looking at the spread in the boxplots of Figure 6, I'm not sure that for all case we have a proper statistical difference between low and mid elevation band (this can be statistically tested). This can be explained by the fact that going from low to mid altitude the representation of the regions in the sub-ensemble considered in not the same. In other words, we cannot with certainty attribute the difference to the season type as you do in the analysis. Note that you can do some statistical tests to see if the different response to climate change between region, elevation band, and season-type are significant, excluding the two other parameters (note: using only one variable you would not have this problem, because by definition all region will have the same number of extreme, 40% of the seasons with the percentiles you use). This imbalance between regions, induced by the joint quantile approach used, should be discussed in the text (note: it indeed totally makes sense to compute extreme per regions). This is not an insurmountable problem, but this is a clear drawback of the method used (as every method has). A clear example is the following sentence in the conclusion: “In particular, snowpack losses were greatest during WW seasons at low and mid-elevations and were greatest during WD seasons at high elevations”. At mid-elevation, the eastern region has more WW event that the western one (Figure S1), at the same

time, HS is more sensitive to climate change in the eastern regions (Figure 9). Now the question is: Is eastern more sensitive because of the local conditions (e.g. closer to isothermal conditions in the baseline simulation), and thus since it has more WW season, the WW signal will appear more sensitive simply because it contains more seasons from this region?
Or is it the opposite: WW season are for some reason more sensitive to climate change, and eastern region having more WW season compared to the other regions, it appears to be more sensitive to climate change? Are the local conditions or the season types dominant here? The fact is that with this analysis we cannot answer this question. We can see some correlation between season and sensitivity to climate change, yes, but we cannot attribute the observed different sensitivity to the season type, this is a major difference.

The extreme compound season is really emphasised in your abstract/title/conclusion, but:

1. The analysis suffers from the problem discussed above. We cannot do a proper attribution.
2. Is not that much discussed in the text in the end. Indeed, only one paragraph (Section 5.3) discusses it in the whole discussion. Finally, most of the discussion is based on more general results.

I think here lies my main problem with the current status of the manuscript. Either the focus is kept on the compound seasons (then maybe the general discussion on well know impacts of climate change on snow (Section 5.1 and 5.2) should be highly shortened), and the strength of the analysis on the difference in the signal between seasons should be improved (by using proper statistical test showing that season type is significant despite the imbalance in regions) and the problems mentioned above arising from the season type construction need to be discussed (which I encourage you to do); or you decide to be more generalist (as you are now in some parts of the discussion), and then you remove most of the emphasis on the compound season.

Thank you for your recommendation.

We have modified the manuscript accordingly. We have added more data and figures to evaluate the results obtained by using the joint-quantile approach and answer reviewer 2 comments. We proceed with the analysis suggested by the reviewer, and we have analyzed sensitivity to temperature and precipitation change by sectors of the range. We statistical classified the massifs by applying a PCA, a broadly applied technique in snow climatology.

Comparison between sectors reveal almost the same relative importance of each compound season type in the sensitivity to temperature and precipitation change for the entire range than for Pyrenean sectors. Thus, our results confirm that although there are different number of season types by sector (Figure S1 and S3), no differences in the relative importance of each compound season type on the sensitivity to temperature and precipitation change are found between Pyrenean regions. For instance, for most indicators, maximum to minimum snow sensitivities to temperature and precipitation ranges from WW to CW, independently of the sector and the number of season types recorded by massif. Thus, we can split the data by sectors (with different number of season types recorded during the baseline period by each massif, as shown in Figure

S1 and S3) or by massifs (it was shown at first version manuscript Figure 9 and 10), but the relative importance of each compound season type is similar than if we do the average for the entire range and by elevation bands. This is because by applying the join-quantile approach (season types are defined by each massif and elevation temperature and precipitation percentiles historical records) we are comparing similar climate seasons, independently of the sector where the season type was recorded. In the end, we are standardizing the climate of the Pyrenean massifs. The maximum sensitivity to temperature and precipitation change (absolute values) is always reached in the southern-eastern Pyrenees, independently of the season type. This is because this leeward side is exposed to higher turbulent and radiative heat fluxes, and this sector is closer to the 0ºC isotherm.

Results section 4.2 are focused on the sensitivity to temperature and precipitation change analysis due to increments of temperature, elevation, and compound season type. These variables have larger influence in the sensitivity to temperature and precipitation change than spatial differences, because the relative importance of the latter is reduced by applying the joint-quantile approach. Spatial differences in the sensitivity to temperature and precipitation change (absolute values) were already examined in our manuscript first version, but now we included a title to the results paragraph (section 4.3), and we provide an analysis by sectors defined by a PCA. Finally, in the discussion section, we explain the reason why the number of seasons by massif does not influence in the results obtained.

In this case, we prefer not reducing section 5.1 and 5.2, especially when we are within the word limit proposed by The Cryosphere. We consider that this information should be included to better understand our results. Without an accurate contextualization the numbers provided in the results section do not have any meaning, and for this reason we consider important to not reducing results discussion. We compare our work with general snow sensitivity studies and snow climate projections because of its similarities. As we state in the introduction, there are not many works comparing snow sensitivities to temperature and precipitation and compound season types (or dry/ wet seasons), which limits the discussion of our results with this literature but provides evidence of the novelty of our findings.

If the editor considers that we should modify it, of course, we will implement such changes.

We have modified the methodological section, results and discussion of the manuscript and added figures according to your comments and our findings:

Method

We have added an accurate description of the standardizing procedure that we are doing by applying the joint-quantile approach:

"…Note that the number of compound season type is different depending on the Pyrenees massif (Figure S1). However, by applying the joint-quantile approach described, we are comparing the snow sensitivity to temperature and precipitation change between similar climate conditions, independently where each compound season type was recorded."

"3.7 Spatial regionalization

We have examined spatial differences in the sensitivity to temperature and precipitation change by compound season types. Massifs were grouped into four sectors by applying a Principal Component Analysis (PCA) of HS data (i.e., López-Moreno et al., 2020b; Matiu et al., 2020) and for each elevation depending on PC1 and PC2 scores. PCA scores are shown at Figure S2, the number of season types per sector are shown at Figure S3 and the spatial regionalization is presented at Figure 1."

Results

We have added in the first line of results:

"4. Results

We validated the FSM2 at Section 4.1. Subsequently, we analyzed the sensitivity to temperature and precipitation change based on five snow climate indicators, namely the seasonal HS, peak HS max, peak HS date, snow duration and snow ablation. Compound season types show similar relative importance on the sensitivity to temperature and precipitation change regardless of the Pyrenean sector. For this reason, our results have been focused on seasonal snow changes due to increments of temperature, elevation, and compound season type. These are the key factors that ruled the sensitivity to temperature and precipitation change and an accurate analysis is provided in Section 4.2. Spatial differences on the sensitivity to temperature and precipitation change during compound season types are examined at Section 4.3."

4.3 Spatial patterns

PCA analysis reveals four Pyrenean sectors, namely northern-western (NW), northern-eastern (NE), southern-western (SW), and southern-eastern (SE). No differences between sectors are found in the relative importance of each compound season type in the sensitivity to temperature and precipitation change (Figure 8). Snow sensitivity to temperature and precipitation change absolute values are generally lower at northern slopes (NW and NE) than at the southern slopes (SW and SE) (Figure S7 and Figure S8). In detail, seasonal HS ranged from −26%/°C during CD (NW) to −36%/°C during WW (SE) . Similarly, the maximum peak HS max sensitivity to temperature and precipitation was at SE during WW seasons (25%/ºC) and the minimum was during CD seasons at NW (15%/ºC). The snow duration sensitivity to temperature and precipitation increased during WW seasons, and the maximum changes were at SE sector (−16%/ºC); in contraposition, the lowest sensitivity to temperature and precipitation are found at NW sector, during CD and CW seasons (−8%/ºC, in both seasons). Snow ablation sensitivity to temperature and precipitation increases towards the eastern Pyrenees, particularly during WD seasons (14%/ºC and 13%/ºC for NE and SE, respectively). Finally, no remarkable peak HS date differences are observed between sectors and maximum values are found during CD and CW seasons, when the peak HS date is anticipated >= 5 per ºC for all sectors."

We have modified "5.3 Spatial and elevation factors controlling sensitivity to temperature and precipitation change" section:

"5.3 Spatial and elevation factors controlling sensitivity to temperature and precipitation change

Comparison between Pyrenean sectors (Figure 8) reveals no remarkable differences in the relative importance of each compound season type in the sensitivity to temperature and precipitation change. This is because by applying a joint-quantile approach for each massif and elevation, we are comparing similar climate seasons between sectors, regardless of the number of compound season types recorded in each massif during the baseline period (Figure S1 and S3). The highest absolute sensitivity to temperature and precipitation change values is found in the SE Pyrenees. This is consistent with the snow accumulation and ablation patterns previously reported in this region (Lopez-Moreno, 2005; Navarro-Serrano et al., 2018; Alonso-González et al., 2020a; Bonsoms et al., 2021a; Bonsoms et al., 2021b; Bonsoms et al., 2022). The Atlantic climate has less of an influence in the SE sector, and in situ observations indicated there was about half of the seasonal snow accumulation amounts as in northern and western areas at the same elevation (>2000 m; Bonsoms et al., 2021a). The snow in the SE Pyrenees is more sensitive to temperature and precipitation because these massifs are exposed to higher turbulence and radiative heat fluxes (Bonsoms et al., 2022). Similar conclusions are found for low elevations, where the results show an upward displacement of the snow line due to warming. Previous studies described the sensitivity of the snow pattern to elevation at specific stations of the central Pyrenees (López-Moreno et al., 2013; 2017), Iberian Peninsula mountains (Alonso-González et al., 2020a), and other ranges such as the Cascades (Jefferson, 2011; Sproles et al., 2013), the Alps (Marty et al., 2017), and western USA (Pierce et al., 2013; Musselman et al., 2017b). In these regions, the models suggest larger snowpack reductions due to warming at subalpine sites than at alpine sites (Jennings and Molotch, 2020) due to closer isothermal conditions (Brown and Mote, 2009; Lopez-Moreno et al., 2017; Mote et al., 2018)."

Minor comments

P1 L35: Should be "increasing" the albedo.

Changed.

P2 L36: Should be "decreasing surface and air temperature". And it is not absolutely true that snow decreases surface temperature. During winter, the snow/soil interface will mostly remain at 0°C if the soil is snow covered, while if the soil is snow free but the air temperature is cold, the soil surface temperature will further decrease (snow is a really good insulator for the soil). In spring, it is true that snow cover will keep the soil colder. However, I would keep only “decreasing air temperature”.

Changed for “modulating surface and air temperature”

P2 L59: “on snowpack duration” to “on snow cover duration”

Done

P3 L85-86: “and the different mountain exposure to the main air masses”. Which “main air masses”?

Changed “main air masses” to “Atlantic air masses”.

P4 L95: What is “mid-late”?

Changed: “21st century climate projections”

P4 L105: “Sensitivity” to what? (Same at lines 107, 108, 110, Sections 3.4 and 4.2 names, etc). Should always be “sensitivity to climate change”, this is indeed a bit heavier in the text, but correct (see comment above).

Done.

P4 L115: What does “these” refer two? Long sentence with many commas, hard to follow, consider splitting in two sentences.

We refer to warm season.

We have changed

“on warm seasons in the Mediterranean basin (Vogel et al., 2019; De Luca et al., 2020) because these are likely to increase in the future (e.g., Meng et al., 2022)”.

to:

“…Warm seasons in the Mediterranean basin require special attention because these are likely to increase in the future (e.g., Vogel et al., 2019; De Luca et al., 2020; Meng et al., 2022”

P9 L259: Do you mean “ablation rate”?

Snow ablation is described in the methodological section:

"…(v) daily average snow ablation per season (snow ablation, hereafter)."

Snow ablation sensitivity to temperature and precipitation change is expressed in % per ºC.

P9 L262: Why isn't snow duration also an accumulation indicator?

It can be also. We have deleted this phrase to avoid misunderstandings .

P11 L287-288: "High elevation areas had lower season-to-season snow variability than low elevations for all season types (Figure 4)". I don't see any information about season-to-season variability in Figure 4.

We have changed:

"High elevation areas had lower season-to-season snow variability than low elevations for all season types (Figure 4)".

To:

"High elevation areas had lower seasonal HS variability between season types than low elevations (Figure 4)".

In Figure 4 it is clearly shown that seasonal HS differences between season types are greater in low elevation areas than at high elevation, which is in accordance with previous works as it is mentioned in the introduction section.

P11 L 289: Avoid using the word "significantly" if not in the context of a proper statistical analysis (and thus a statistical "significance").

Done.

We have deleted "significantly" and "significant" where needed. We have changed "significantly" to "clear", "remarkable" and "important" depending on the context.

P11 L289-291: "All the snowpack-perturbed scenarios indicated that snowpack decreased at low and mid elevations under warming climate scenario". This is also the case at high elevation (Figure 4).

We have changed:

"All the snowpack-perturbed scenarios indicated that snowpack decreased at low and mid elevations under warming climate scenario".

To

“All the snowpack-perturbed scenarios indicated that snowpack decreased for all elevations under warming climate scenario”.

P13 L331: “the peak HS date per °C was earlier by 9 days”. This does not mean anything to me, should be: “the peak HS was anticipated by 9 days per °C” (but see my comment on linear indicators).

Changed.

P13 L337: “and because”: Remove “and”

Done.

P13-14 Figures 5-6: Why not having only one figure with three rows of boxplot?

Changed.

P14 L350: “At low elevations, the snow ablation in all four extreme seasons was 12%/ºC”. Something is missing here. Should be “snow ablation rate increase”. Same for the rest of the paragraph, should be ablation rate change/increase/decrease.

We have changed all the text according with snow ablation definition provided in the methodological section “…(v) daily average snow ablation per season (snow ablation, hereafter).”

In this case, we have changed:

“At low elevations, the snow ablation in all four extreme seasons was 12%/ºC”
To
“At low elevations, the snow ablation sensitivity to temperature and precipitation in all four extreme seasons was 12%/ºC”

P16 Figure 8: I think the y-axis units should be cm/day. Explain the numbers in the caption, e.g. “the numbers in the plot show the difference in ablation rate compared to the previous degree”. Or maybe I don’t understand the figure, and this just are absolute values of ablation rate.

Figure 8 (now Figure 7) shows the average daily snow ablation (cm/day) for the baseline climate and by each increment of temperature.

So then why stacking them on top of each other and why having a y-axis? Shouldn’t it be like figure 7? Also, I can’t reconciliate what I shown in this figure with the numbers in Table 4 for the ablation columns. In the text and Table 4, you have ablation in %/°C, and here in cm/day.

We have changed Figure 7 type.

We show absolute (cm/day) and relative values (%/ºC) because:

1. Relative values are easier to compare with past and future works in other geographical areas.

2. Absolute values are specially interesting to compare between elevation band (i.e., analyze slow snowmelt rates in marginal snowpacks).

3. We must include absolute and relative values for consistency, because we have done the same for the other snow climate indicators.

We have added in the description:

"Figure 7. Absolute snow ablation values (cm/day) at three different elevations during four different compound temperature and precipitation seasons for different temperature increases above baseline (gray)."

If the editor considers that we should modify it, we will implement such changes.

P18L398: "The sensitivity of snow to different spatial patterns of climate change that we identified here […]". You do not study different patterns of climate change; you apply the same delta everywhere. Please correct.

Changed: "The different sensitivity to temperature and precipitation change spatial patterns that we identified here (Figures 9 and 10)"

P21L504-505: "Our maximum snow ablation and peak HS date occurred during dry seasons …]". Are you talking about change or about absolute value in the baseline simulation?

Changed:" Our maximum snow ablation relative change over the baseline scenario…"

P21L508-510: "The temperature in the Pyrenees is still cold enough to allow snowfall at high elevations during WW seasons, and for this reason we found maximal sensitivities during WD seasons." I don't understand, temperature is almost the same in WW and in WD, so why is sensitivity to climate change greater in WD?

This is because larger precipitation in wet seasons will allow more snowfall and a slight increase of temperature will not affect snowpack evolution at high elevation and in the coldest months of the season (Figure S9).

P23L562-564: “however, a more complex model does not necessarily provide better performance in terms of snowpack and runoff estimation (Magnusson et al., 2015)”. But it also can, especially for climate change study (see Carletti et al, 2022, doi.org/10.5194/hess-26- 3447-2022)

Thank you for your suggestion and many congratulations for your work.

We have included your reference in our manuscript. We have added:

“In this work we used a physical-based snow model since it provides better results for future snow climate change estimations than degree-day models (Carletti et al., 2022)”

We have modified this paragraph:
“…The FSM2 is a physics-based model of intermediate complexity, and the estimates of snow densification are simpler than those from more complex models of snowpack. The FSM2 configuration implemented in this work includes snow meltwater retention, snowpack refreezing and snow albedo based on snow age, which are the physical parameters included in the best-performing snow models according to Essery et al. (2013). Snow model sensitivity studies reveal that intermediate complexity models exhibit similar SWE accuracies than most complex snow models, as well as robust performances across seasons (Terzago et al., 2020).”

Regarding the robustness of the snow model, the reader is informed in the methodological section:

“…and its application in different forest environments (Mazzoti et al., 2021), and hydro-climatological mountain zones such the Andes (Urrutia et al., 2019), Alps (Mazzoti et al., 2020), Colorado (Smyth et al., 2022), Himalayas (Pritchard et al., 2020), Iberian Peninsula Mountains (Alonso-González et al., 2020a; Alonso-González et al., 2022), Lebanese mountains (Alonso-González et al., 2021), providing confidential results.”

If the editor considers that we should change the snow model or add more FSM2 uncertainties, we will proceed accordingly.

P564-566: “Biases in the SAFRAN system and biases related to the FSM2 were minimal because we quantified relative changes between a modeled snow scenario (climate baseline) and several perturbed scenarios”. This assumes constant biases. Snow cover involves different variables, non-linear processes, and will accumulate errors along the season, we cannot be that certain.

We have deleted this phrase.

P23L568-569: “[…] but assumes that the snow patterns of the reference climate period will be constant over time.” Don’t you mean meteorological patterns? (E.g. you don’t capture change in precipitation regime, you just scale the intensity of each events). Please clarify.

Thank you for your suggestion, we have changed “climate” for “meteorological”.