Interactive comment on "Snow sensitivity to climate change during compound cold-hot and wet-dry seasons in the Pyrenees"

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Response to Reviewer 2. Comment posted on 06 October 2022.
Reviewer comments are in bold and responses in blue.

Dear authors, dear editor,

The paper submitted discusses the impact of climate change on snow cover in the Pyrenean for different air temperature and precipitation pathways, and for different seasonal conditions.In general, the paper is clear and shows clean figures. As I detail below, there are some important points to be addressed, mainly enhancing clarity of the description of the method and analysis (to allow reproducibility), and focusing more the analysis on the main question.

I have no doubt that these points can be clarified and/or enhanced by the authors and that a reviewed version will fit for a publication in TC. Indeed, if the authors are able to re-focus the analysis on the main point of the paper (i.e. the difference between compound cold-hot and wet-dry seasons), this work will brin some valuable contribution for the community.

We would like to express our sincere gratitude to Dr. Michel for their extensive constructive suggestions and comments. All the recommendations suggested by the reviewers were carefully taken into consideration and have improved the rigor and clarity to our findings presented in this paper.

Major comments:

Use of "climate sensitivity" term

Throughout the introduction (and the rest of the paper), the term climate sensitivity is used several times, mostly in the form "climate sensitivity of snow". Climate sensitivity is defined as: "*Climate sensitivity refers to the change in the annual global mean surface temperature in response to a change in the atmospheric CO_2 concentration or other radiative forcing."* [IPCC glossary[1]]. In your case it is rather used to describe the response of snowpack to climate change. E.g. lines 123-124: "*[…] suggest the existence of a wide variety of climate sensitivitiesof snow depending on elevation and spatial factors.",* where you mean "a wide range of responses to climate change". I'd recommend to reformulate all the instances of "climate sensitivity" throughout the manuscript since in the climate change language this correspondsto something really specific. You should use "climate change impact", which is in my opinion the correct word, or at least stick to "sensitivity of snow to climate change".

Thank you for your suggestion. Accordingly to comments from reviewer 1, we have changed “climate sensitivity” for “sensitivity of snow to climate change” and “snow sensitivity” depending on the context.

Validation process

The whole validation process is not clearly described. In P6 you say *“In this work, the FSM2 model configuration was selected on a trial-and-error basis (not shown here), validated by in-situ snow records of four automatic weather stations (AWS) placed at high elevation areas ofthe Pyrenees. Then, the FSM2 was forced with the SAFRAN reanalysis dataset for the entiremountain range (see Section 3.2).”* and finally you describe some corrections of the data fromAWS.

We have changed Section 3.2 and split the information into two sections:

Section 3.2, Snow model: where we describe the FSM2 configuration.

Section 3.3, Snow validation: where we provide a description of the snow model validation.

Did you run at stations with SAFRAN data of with AWS data for the validation? If run with AWS, when then do you validate the model with SAFRAN data? This is acrucial step.

We run the FSM2 with meteorological AWS data and compared the accuracy against HS records. It is not possible to compare the AWS between the AWS records and the SAFRAN system due to:

1. The different resolution and elevation bands. The SAFRAN system provides data by homogeneous (around 1000 km2) meteorological and topographical mountain massifs every 300 m, from 0 to 3600 m (Durand et al., 1999; Vernay et al., 2021), that do not coincide with the AWS elevation used for validating the FSM2.

2. The SAFRAN dataset that we used in this work was data-assimilated with in-situ meteorological observations of the mountain range. We cannot validate in-situ records that were previously data-assimilated by the SAFRAN system. In addition, the SAFRAN system has been extensively validated before our work.

Did you use the AWS for the mentioned trial-and-error setup? In this case, this is a calibration, not a validation. It should then be validated at stations not used to calibrate the parameters

We have validated the FSM2 against in-situ (AWS) snow simulations. We have evaluated different configurations, but no significant differences were observed in the accuracy and performance metrics.

We have added (also in response to reviewer 1)

“We have evaluated different FSM2 model configurations (not shown) without significant differences in the accuracy and performance metrics. Therefore, we selected the most complex FSM2 configuration, except for the snow cover fraction estimation, that is based on a linear function of HS. In detail, albedo is calculated 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"

I think Section 3.1 should only describe the model (and here you should add few lines giving some details about the main model physical principle, assumptions, and parameters), and then a new Section 3.2 should describe accurately the calibration/validation procedure. The final model parameters need also to be available in order to allow the reproducibility of the study.

We have added the model configuration. We also have added a chapter (5.5 Limitations and uncertainty) where we detailed the limitations of the input, model and method used.

<u>Analysis description</u>

In line with the lack of details mentioned above, the actual simulations performed is not really well described. In Section 3.3 you say: *"Temperature and precipitation are perturbed for each massif and elevation range based the historical period"*, but never clearly say: *"The model is run for XXX regions, YYY years, etc."*.

We are sorry for the misunderstanding. We have changed: "The data includes flat slopes at low, mid and high elevation ranges and Pyrenean massifs (Figure 1) at hourly resolution" for:

"The FSM2 was run at an hourly resolution for each massif, each elevation range, and each climate perturbation scenario from 1980 to 2019".

Moreover, for all the first part of the analysis, the spatialpatterns are not discussed, and the difference in massifs only appears in the discussion). As a reader I was confused until reaching the bottom of page 15 to know whether the model wasreally run for different locations, or only for different elevation bands.

Thank you for your suggestion. The spatial patterns were already included in the results section (manuscript first version, L368 paragraph: … Snow climate sensitivity shows remarkable spatial contrasts… etc), not only on the discussion section.

The model was run at hourly resolution for each massif, elevation band and climate perturbed scenario (it is mentioned in the methodological section, and it can be observed at Figure 9 and 10).

The procedure should be really explained (see my minor comment about a missing global study description at the end of the intro, which can help). Naming the massifs in Figure 1 andhaving a table briefly describing each massif (e.g. with min/mean/max elevation) would be useful for the analysis and help to clarify that the model is indeed run per massif. Another unclear point for me is the elevation used. Did you run only three elevations of some groups of elevation based on the 300m discretization of SAFRAN?

Thank you for your suggestion. It does not exist different elevations (min/mean/max) for each massif, given that SAFRAN system provides data every 300 m, from 0 to 3600 m. We defined the low, mid, and high elevation bands that we used: Low, mid and high elevation corresponds to 1500, 1800 and 2400 m, respectively, specific elevation bands. The model was run at hourly resolution per each massif, elevation band and climate (baseline and perturbed) scenario.

In Section 4.2, you should clearly state that all massifs are grouped together for the present analysis (and that the spatial analysis is performed later on). As far as I understand, Figure 4 shows the average across all massif. This should be clearly stated.

Figure 4 is the average for each elevation band. We have modified the figure and figure caption:

"Figure 4. Average daily values for season type, baseline climate and different temperature increases at (a) high (b) mid and (c) low elevation."

Also, in the whole Section 4.2 changes in precipitation are not mentioned (except in the caption of Figure 5), and only shown in Supplementary Figures. However, the fact that precipitation (+10%) could contract a 1°C is presented as one of your key results. The corresponding Figures should thus be properly shown, introduced, and described in the main text and in the Results Section (now Supplementary figures are just mentioned in the Discussion section).

Thank you for your suggestion. We have rearranged the information and figures. We have added Figure 7 (previous Figure S4), following your suggestion to show the influence of precipitation in the snowpack evolution. Our results have been focused on seasonal snow-related changes due to increments of temperature, elevation, interannual variability – season type –, and spatial differences. These are the key factors that ruled the snowpack variation. We prefer to not add more details about precipitation given that precipitation only can counterbalance warming at high elevations, during the core months of winter, and if temperature do not exceed > 1ºC with respect of the 1980-2019 climate.

<u>Impact study, determining factors, uncertainty</u>

I have the feeling that Section 4.2 is a long list of numbers a bit hard to follow, and in many cases the text repeats numbers shown in the Figures and Tables. Moreover, I feel a significantpart of the numbers mentioned in Section 4.2 are nor really useful to support the latter analysis. In addition, this study inspects many aspects: different temperature and precipitation pathways, different kinds of compound seasons, and many sub-regions. In addition, they areanalysed using 5 indicators, resulting on hundreds of different "numbers" to discuss. In the discussion, it is hard to really see the direction. Indeed, while the title suggests a focus on compound seasons, this is not really present in some part of the discussion (i.e., 5.4, which summarizes well known impacts and is in my opinion not necessary here, or 5.2.1, winch basically say that if we have more solid precipitation, we have higher snowpack). I would encourage to maybe reduce and reorganize the discussion and to only focus on few points (e.g. compound season and spatial distribution). A large amount of data has been produced for this study and it can be tempting to discuss every aspects of the data obtained from the model, but this makes it harder to read, and hide what is

really the novelty of this work. Note that the plots about spatial distribution are introduced in the discussion, while in my opinion they belong to the results Section.

We are grateful of the reviewer comment, but we consider that we have followed a chronological order to discuss the results. We have focused the results and discussion on snow accumulation, ablation, season type differences, spatial patterns, environmental impacts, limitations, and uncertainties of the work. As far as we could, we have avoided to express numbers in the text. We have discussed the main results, and unfortunately there is not many more research that analyze the links between compound extremes seasons and snowpack evolution.

We agree with the reviewer and Figure 9 and 10 and associated text have been moved from the discussion to the results section.

I've 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, of 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 theapproach 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. Note that there is no discussion about uncertainty and limitation, this should be added.

Thank you for your comment. The information about the season type classification was detailed in the methodological section: "Compound temperature and precipitation extreme season (season type) is performed using a joint quantile approach (Beniston and Goyette, 2007; Beniston, 2009; López-Moreno et al., 2011a), for each massif and elevation ranges".

We have changed that for: "Compound temperature and precipitation extreme season (season type) is performed based on each massif and elevation historical climate record (1980-2019), using a joint quantile approach (Beniston and Goyette, 2007; Beniston, 2009; López-Moreno et al., 2011a). Season types are classified based on the massif and elevation historical record. We are not comparing season types between massifs. If we classify the season types based on the entire range percentiles, some extreme season types, such as CW, will be significantly reduced in the driest zones.

In the methodological section we have already mentioned that snow is modeled for flat slopes. We consider that we are already presenting the spatial differences for each season type in the results and discussion. Differences between regions (Figure 9 and 10) are due to meteorological input data. The massifs of the Eastern area are exposed to higher rates of radiative and turbulent heat fluxes and the snowpack is near to the isothermal conditions during the season shoulders. Therefore, a small increase of temperature leads to higher snow losses, especially during WW seasons.

Limitations and uncertainties

We have followed the reviewer suggestion and we have included a limitation and uncertainty section:

"**5.5 Limitations and uncertainties**

The meteorological input data that we used to model snow were estimated for flat slopes and the regionalization system we used was based on the SAFRAN system. According to this system, a mountain range is divided into massifs with homogeneous topography. The SAFRAN system has negative biases in shortwave radiation, a temperature precision of about 1 K, and biases in the accumulated monthly precipitation of about 20 kg/m^2 (Vernay et al., 2021). The snow model used in this work (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; however, a more complex model does not necessarily provide better performance in terms of snowpack and runoff estimation (Magnusson et al., 2015). Biases in the SAFRAN system and biases related to the FSM were minimal because we quantified relative changes between a modeled snow scenario (climate baseline) and several perturbed scenarios. Finally, our estimates of snow sensitivity were based on the delta-approach, which considers changes in temperature and precipitation based on climate projections for the Pyrenees (Amblar-Francés et al., 2020), but assumes that the snow patterns of the reference climate period will be constant over time."

Minor comments:

Abstract: Please do not use abbreviations in abstract, only full words.

Changed.

P2 L38-39, L44, …: Please sort citation in ascending order by year (throughout the whole manuscript).

Done.

P3 L67-68: I do not understand what “*coincides*” with “*low solar radiation periods*”.

The snow ablation onset occurs earlier in the season, coinciding with low solar radiation periods.

“...However, warming can slow the early snow ablation rate on the season (Pomeroy et al., 2015; Rasouli et al., 2015; Jennings and Molotch, 2020; Bonsoms et al., 2022; Sanmiguel-Vallelado et al., 2022) because of the earlier HS and SWE peak dates (Alonso-González et al., 2022), which coincide with periods of low solar radiation (Pomeroy et al., 2015; Musselman et al., 2017a)…”

P4 L95-96: What do you mean by “mid-end 21st century”?

Changed: “mid-end 21st century” to “for the next decades”

P4 L107: “.” Missing

Done.

P5 L112-113: “*To date, some studies pointed out different climate sensitivities on wet or dry years*”. Can you please explain in one sentence the different results found.

We prefer to simplify this section since we already discuss these studies in the 5.3 section.

P4 L126-128: Here I would briefly describe the main steps used to achieve this objective

The main steps (input data and model) are already presented in the abstract, data and methodology and conclusions.

P5 L139: Which “lapse-rate”? Elevation lapse rate of precipitation?

We have changed this paragraph: “Precipitation is mostly driven by large-scale circulation patterns (i.e., Zappa et al., 2015; Borgli et al., 2019), the jet-stream oscillation during winter (e.g., Hurell, 1995) and land-sea temperature differences (Tuel and Eltahir, 2020)”

P5 L142: “being ~ 1000” change to “being on average …”. Please clarify in the rest of the paragraph where “~” means “around” and where it means “on average”.

Thank you. Done

P6 L177-178: You should provide the final retained configuration for reproducibility.

Thank you. Done:

“We have evaluated different FSM2 model configurations (not shown) without significant differences in the accuracy and performance metrics. Therefore, we selected the most complex FSM2 configuration. In detail, albedo is calculated 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. Snow cover fraction is based on a linear function of HS."

P7 Table 1: Seems coordinates are in lat/lon °, not in UTM. Units are missing for the two "distance" column. "Reference period" is never explained in the text (see also major comments on the calibration/validation description).

We have added:"Lat/Lon º" and Reference period for "Validation period (years)"

P8 L208: Please provide a reference for the implementation

It is mentioned in the manuscript first version L171 (Essery, 2015). Also, we have added the snow model configuration.

P8 L217: "in" section

Changed for "Precipitation type was classified following the threshold approach used for the model validation" (according to reviewer 1).

P9 L266: What do you mean by "by massif"?

Each snow-climatological indicator is calculated for each massif and elevation band.

P9 Section 4.1: R2 should be R^2

Done.

P11 L292: Refer to Figure 5 at the end of the sentence. What does "Here" refer to?

We refer to low elevation (Figure 4).

We have changed "Here" for "At low elevation".

P11 Figure 4: For comparison, you should also show the reference simulation (+0°C) in the Figure. There are some strange drops in snow height (see below).

Thank you for your suggestion. We have added the baseline climate snow profile in Figure 4 and resolved the error (one day was missed when plotting the results).

P13 Figure5: how are the boxes constructed? Different seasons (i.e different years) + changein precipitation + different massifs? Or do you have only one point averaging across all seasons for a given massif? You should explain how are the boxes (1 and 3 quartiles?), whiskers, and outliers are defined.

Thank you for your recommendation. We have added in each boxplot:

“The solid black lines within each boxplot are the average. Lower and upper hinges correspond to the 25th and 75th percentiles, respectively. The whisker is a horizontal line at 1.5 interquartile range of the upper quartile and lower quartile, respectively. Dots are outliers. Data is grouped by season, season type, increment of temperature, precipitation variation, elevation, and massif”.

P14 Figure 6: Why not using a boxplot here as in Figure 5?

Thank you. Wehave added a boxplot following your suggestion.

P15 figure 7: How is this exactly computed? By “season” you mean the exact length of the ablation season (i.e. time between HS_{max} and HS=0)?

We detailed in the methodological section how snow ablation is calculated (average daily snow ablation for a snow ablation day).

P16 L383-386: This kind of statement should be in the Introduction section:

Thank you for your comment. It is mentioned in the introduction (first manuscript version L21), however, we intentionally repeated our statement since it is crucial to introduce the reader to the discussion section and reinforce the relevance and novelty of our work.

P16 L393-398: I do not really see the added value of this information here

Thank you for your suggestion. We cannot remove this information since it is necessary to understand the spatial patterns of snow in the mountain range, as well as the different spatial responses to warming that we have detected.

P17 Figure 8, P20 Figure 9: Units missing

Added.

P17 L418-420: You should show plots supporting it (e.g. a plot of precipitation phase)

We are grateful of your suggestion. In this sentence we refer to the changes in the snow dynamics reported by scientific literature. However, we have added Figure S4 where the snowfall fraction shifts due to warming can be found.

P18 L438-440: Something is missing in this sentence, e.g. “The <u>higher</u>

average […]"

Thank you. Changed.

P18 Section 5.2.2: This is really interesting. In my work on hydrology, I found that on a warmerworld discharge peak from snowmelt will occur earlier, but also be "flatter" (see Michel, 2022).I never went deeper in the analysis of the cause of the flattening. Your analysis on slower meltrate seems really relevant to answer this question.

We appreciate your comment in this active research topic. We have provided a plausible explanation based on our work and previous studies:

"Climate warming leads to a cascade of physical changes in the SEB that increase snow ablation near the 0ºC isotherm. On overall, the average daily snow ablation showed moderate to low changes due to warming. Comparison between low and high elevations indicated slightly faster snow ablation at high elevations (Figure 8). This higher rate of snow ablation per season at high elevations (which have deeper snowpacks) are probably because the snow there lasts until late spring, when more energy is available for snow ablation (Bonsoms et al., 2022). Temperature increase does not imply significant changes in the daily snow ablation rate per season because warming decreases the magnitude of the snowpack (seasonal HS and peak HS max) and triggers an earlier onset of snowmelt (Wu et al., 2018). The earlier peak HS date at low and mid elevations (Table 4 and Figure 7) implies lower rates of net shortwave radiation, because snow melting starts earlier in warmer climates (Pomeroy et al., 2015), coinciding with the shorter days and lower solar zenith angle (Lundquist et al., 2013; Sanmiguel-Vallelado et al., 2022). Our results agree with the slow snow melt rates reported in the Northern Hemisphere from 1980 to 2017 (Wu et al., 2018). The results of previous studies were similar for subarctic Canada (Rasouli et al., 2014) and western USA snowpacks (Musselman et al., 2017b), but Arctic sites had faster melt rates (Krogh and Pomeroy, 2019)."

P19 L464: "if" □ "in"

Done.

P19 L464-466: With all the uncertainty involved, I would say "is similar"

Done.

P19 L467: A reference is needed here.

Done.

Thank you for your constructive suggestions.