

Review of “Snow sensitivity to climate change during compound cold-hot and wet-dry seasons in the Pyrenees”

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 bring some valuable contribution for the community.

Do not hesitate to contact me for further discussions.

Best regards,

Adrien Michel (adrien.michel@epfl.ch)

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 CO₂ concentration or other radiative forcing.*” [IPCC glossary¹]. 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 sensitivities of 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 corresponds to 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”.

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 of the Pyrenees. Then, the FSM2 was forced with the SAFRAN reanalysis dataset for the entire mountain range (see Section 3.2).*” and finally you describe some corrections of the data from AWS

1. Did you run at stations with SAFRAN data or with AWS data for the validation?
2. If run with AWS, when then do you validate the model with SAFRAN data? This is a crucial step.

¹ https://www.ipcc.ch/site/assets/uploads/2018/11/sr15_glossary.pdf

3. 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

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.

Analysis description

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.”*. Moreover, for all the first part of the analysis, the spatial patterns 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 was really run for different locations, or only for different elevation bands.

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 and having 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? In Section 3.4, you do not explain if the quantiles analysis is done per massif (i.e. looking at each season in comparison to other seasons in this given massif), or globally (i.e. looking at each season in comparison to the whole set of seasons for all massifs).

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. 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).

Impact study, determining factors, uncertainty

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 significant part 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 are analysed 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, which 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.

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 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. Note that there is no discussion about uncertainty and limitation, this should be added.

Minor comments:

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

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

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

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

P4 L107: "." Missing

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.

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

P5 L139: Which "lapse-rate"? Elevation lapse rate of precipitation?

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".

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

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).

P8 L208: Please provide a reference for the implementation

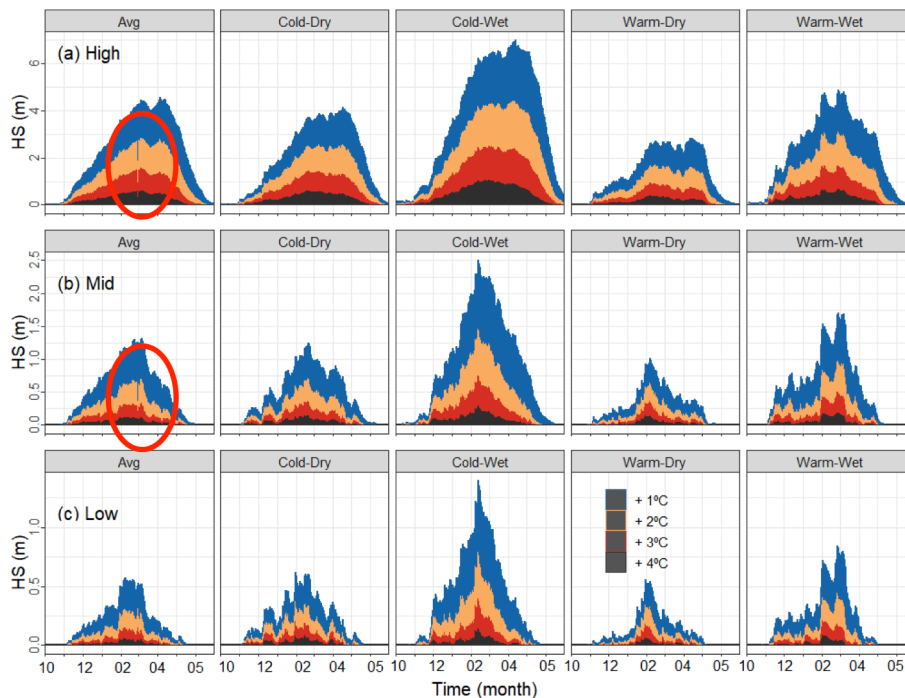
P8 L217: "in" section

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

P9 Section 4.1: R2 should be R²

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

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).



P13 Figure 5: how are the boxes constructed? Different seasons (i.e different years) + change in 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.

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

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 $HS=0$)?

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

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

P17 Figure 8, P20 Figure 9: Units missing

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

P18 L438-440: Something is missing in this sentence, e.g. “The higher average [...]”

P18 Section 5.2.2: This is really interesting. In my work on hydrology, I found that on a warmer world discharge peak from snowmelt will occur earlier, but also be “flatter” (see Michel, 2022²). I never went deeper in the analysis of the cause of the flattening. Your analysis on slower melt rate seems really relevant to answer this question.

P19 L464: “if” → “in”

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

P19 L467: A reference is needed here.

² <https://hess.copernicus.org/articles/26/1063/2022/hess-26-1063-2022.html>