

Reply to R1

In the manuscript, “Multi-sectorial impacts of Mediterranean snow droughts on mountain socio-ecohydrology,” the authors characterised snow droughts across Italy for about the last decade and analysed the cascade impacts on the mountain landscape, that is, snow-vegetation-streamflow-people. I enjoyed reading the manuscript and think that the approach, which tries to tackle drought as a whole across the mountain system, is extremely valuable and much needed. However, the manuscript’s storyline is sometimes unclear, and in some sections, the analyses seem somewhat independent. In addition, different periods and locations are used for each analysis, which also contributes to that slightly confusion. My comments aim to go in this direction: keep an integrated story using the propagation cascade you showed in Fig. 9 as the common thread from the beginning of the paper.

We thank the Reviewer for their helpful comments. They are feasible and will improve the quality of the manuscript. In the following, we comment on our revision strategy.

I feel the title is a bit confusing. To me, it is not clear what the cause-and-effect relationship is. That is, snow drought impacts different sectors, and these sectors condition the mountain socio-ecohydrology, or are the different sectors conditioning snow droughts and then impacting mountain socio-ecohydrology? What are these multi-sectorial impacts? Is hydrology one of these sectors? I also think it would be nice to specify in the title that the work is carried out across the Italian mountains as a representative case of the Mediterranean mountains.

Thanks for this comment. We are not aiming to imply a cause effect in our title. Our aim is instead to explain that we will discuss impacts of snow droughts on the various sectors of socio-ecohydrology in Mediterranean mountains. We will simplify the title as “Impacts of Mediterranean snow droughts on mountain socio-ecohydrology”. We are already discussing the role of Italy as a representative case of the Mediterranean mountains in the Introduction, and we would like to keep the Title concise.

Introduction: Since the Mediterranean mountains are the target area of the study, I suggest adding a paragraph explaining a bit about the particularities of the Mediterranean mountain systems (snow-vegetation-streamflow-people). Among them, I think it is important to mention the roles of snow evaporation and snow sublimation in the water balance.

We totally agree. This will be added!

Introduction: I miss a sentence with the main aim of the paper. Line 73 points in this direction; however, it is followed by a dataset description, which masks it. I suggest rewriting this objective, moving the scientific questions (lines 80-83) closer to it, and sorting them according to the previously suggested cascade.

We agree and will revise this sentence accordingly.

Methods: The current headings’ structure is not clear; the use of the word impact in both data and analysis is confusing, at least to me. Maybe an explanatory summary methodolog-

ical figure would help. Another option, maybe also complementary, would be to keep just three main subheadings: 2.1 Study region, 2.2 Data, and 2.3 Analysis, and to change those under data to the type of data (for instance, meteorological, hydrological, vegetation, and societal) and those under analysis to the analysis carried out following the same structure.

Very good point, we agree. We particularly like the idea of the three main subheadings, which we will consider while revising the section.

Figure 1 mixes study site data with results. I would keep only panel (a) from the current figure, and I would add some of the information written in the description of the study site here: meteorological context, land cover distribution and watershed districts. I would also zoom in on the selected catchments and draw their borders.

In principle, we agree that this figure mixes a little bit of data with some results. On the other hand, we already have several figures in this manuscript, and the results reported here are an initial inventory of snow droughts, rather than a collection of more in-depth insights. Thus we are inclined to keep this figure as it is. We will, nonetheless, also consider the idea of adding some more information about meteorology and land cover.

Line 161, please add more detail about the BIGBANG dataset, for instance, is it a monthly or daily dataset? Is it a reanalysis product or an interpolated product from ground observations?

BIGBANG is a monthly dataset, interpolated from in situ stations. We will add this to the manuscript.

Lines 164-169, please contextualise this paragraph. I do not understand it.

We will add more information about EDII to make the paragraph clearer.

Table 1, I suggest adding the data's spatiotemporal scales to this table.

Agreed, this will be included.

Line 176, please add some uncertainty information for the PML V2 product.

The PML V2 products demonstrate strong performance when compared to observations at 95 flux tower sites worldwide, and are comparable to or outperform other widely used ET and GPP products (Zhang et al., 2019). Reported evaluation metrics indicate good agreement with in situ observations, although uncertainties remain due to model structure, input data quality, and spatial scaling from site-level measurements to gridded products. In particular, uncertainties in PML V2 GPP arise from the representation of canopy processes, meteorological forcing, and remote sensing inputs (leaf area index, albedo and emissivity), as well as from mismatches between flux tower footprints and pixel-scale estimates. However, at the spatial scale considered in this study, i.e., aggregated over multiple mountain catchments across Italy, such uncertainties are expected to be partially mitigated through spatial averaging, reducing the influence of local-scale errors. While these uncertainties may affect the absolute magnitude of GPP, they are expected to be relatively consistent in space and time, and therefore are unlikely to bias the relative comparisons between drought and non-drought years that are

the focus of this study. We will add the above to the manuscript.

Line 250, if I have understood correctly, the paper defines a snow drought on an annual basis, that is, for each hydrological year, just a snow drought can be defined, and in the case this snow drought exists, the year is classified as a snow drought year. Please add a sentence to clarify that.

Yes, this is correct. We will include a sentence in the manuscript.

Results: I would include in section 3.1 a new figure with Fig 1 panels (b) to (e). In addition, I suggest replacing the probabilistic density function graphs with boxplots, following the same design used in Figs. 4 and 5.

Regarding Figure 1, see our previous comment. Regarding density plots, we have originally preferred them over boxplots to show the full variability in these metrics. Nonetheless, we see the point and we will give boxplots a try. We will decide on what is the best option when submitting the revised manuscript.

Results: As stated in the previous section, I suggested splitting snow from streamflow sections.

Agreed!

Results: 2 and 3, following the previous comment, I think boxplots are a better option for representing differences between snow drought and non-snow drought years.

See above: we see the point and we will give boxplots a try. We will decide on what is the best option when submitting the revised manuscript.

Results: Section 3.3, in the analysis at the different catchments, have the authors assessed the possible impact that spring precipitation and temperature could have on these results?

No, and will run a dedicated analysis on this.

Results: Lines 371-382, I found this analysis very interesting; however, I proposed to remove it or include it in an Appendix since I see it as a complementary analysis.

We see the point, especially as the manuscript is already quite long. The value of this analysis beyond the remote sensing data of GPP is that Torgnon data contextualize GPP with other variables that enabled us to improve process understanding of why GPP increases during snow droughts. So we are inclined to keep this analysis in the main text. However, we understand from your comment that we did not do a good job in conveying this added value in our manuscript. Without lengthening the paper too much, we will more thoroughly explain its relevance to the main findings.

Results: Figure 5, change km to m in the x-thick mark to be consistent with the rest of the figures.

Agreed!

Discussion: I would emphasise and contextualise the discussion more in the Mediterranean mountain context, since the study is carried out in this type of environment.

Agreed!

Conclusions: Conclusions are short to have the entity of a proper section. I would elaborate further or merging with section 4. Discussion.

We are inclined to merge Discussion and Conclusions.