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.

> Public discussion: 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.
>
> Revision: We followed the strategy outlined in the public discussion phase, and elaborate on the details below.

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-sectoral 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.

> Public discussion: 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 Mediterrenean 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.
>
> Revision: We changed the title as outlined above.

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.

> Public discussion: We totally agree. This will be added!
>
> Revision: We added a paragraph on this in the Introduction **(see lines 73ff)**

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.

Public discussion: We agree and will revise this sentence accordingly.
Revision: We revised this passage as suggested **(see lines 64ff)**

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 methodological 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.

Public discussion: Very good point, we agree. We particularly like the idea of the three main subheadings, which we will consider while revising the section.
Revision: We revised the Section as suggested.

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.

Public discussion: 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.
Revision: As we motivated above, we decided to keep just one Figure with both the study area and the snow-drought inventory. This was done for concinseness reasons, as the paper already has several figures and it quite long.

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?

Public discussion: BIGBANG is a monthly dataset, interpolated from in situ stations. We will add this to the manuscript.
Revision: We revised this passage as suggested **(see lines 159ff)**.

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

Public discussion: We will add more information about EDII to make the paragraph clearer.
Revision: We moved the reference to EDII after introducing the impact data, which makes it a clearer reference **(see e.g. lines 195ff)**.

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

Public discussion: Agreed, this will be included.
Revision: Done!

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

Public discussion: 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.

Revision: We revised this passage as suggested **(see lines 166ff)**

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.

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

Revision: We revised this passage as suggested **(see lines 269ff)**.

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.

Public discussion: 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.

Revision: We tested boxplots, but still prefer histograms wherever possible, as they display the full variability of the dataset. Also, moving to anomalies as recommended by R2 makes these histograms more impactful. So we decided to keep them in Figure 1 to 3.

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

Public discussion: Agreed!

Revision: Done!

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.

Public discussion: 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.

Revision: See above.

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?

Public discussion: No, and will run a dedicated analysis on this.

Revision: We added a discussion on spring temperature and precipitation in the manuscript **(see lines 392ff)**.

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.

Public discussion: 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.

Revision: We included some more details on the value of the Torgnon analysis in the Methods section **(see lines 313ff)**.

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

Public discussion: Agreed!

Revision: Done!

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.

Public discussion: Agreed!

Revision: We revised the discussion as suggested **(see e.g. lines 443ff or 461ff)**

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

Public discussion: We are inclined to merge Discussion and Conclusions.

Revision: Done!

Reply to R2

In this manuscript, the authors attempt to map snow droughts in Italian mountain regions onto a wide range of impacts (hydrological, ecological, sociological). They use a high-resolution Italian snow reanalysis product to calculate basin-wide snow water equivalent for 38 montane catchments, and identify snow droughts as water years in which the basin-wise December-June average SWE falls below the 30th percentile over the WY 2011-2023 period. They then identify how warm-season runoff and runoff efficiency in their catchments differ between snow drought and non-snow drought years; examine differences in vegetation phenology and productivity derived from remote sensing, as well as two in situ flux towers; and characterize societal impacts using reports of water restrictions and surveys of mountain hut managers. They find reduced runoff and runoff efficiency following snow droughts, modest increased gross primary productivity, and water supply impacts.

Overall, I found the manuscript very interesting, as understanding the impacts of snow droughts in a non-stationary climate is important to informing adaptation in snow-dependent regions. The effort to bring together the multifaceted impacts of snow-drought from a variety of different data sources was impressive. I also found the paper generally well-organized and well-written. I do, however, have a couple significant concerns about some of the methods and framing of results that I feel need to be addressed before I would consider the manuscript ready for publication. I detail each of these points below.

Public discussion: We thank the Reviewer for their extensive review. Revisions are feasible, and we detail our revision strategy below.

Revision: We followed the strategy outlined in the public discussion phase, and elaborate on the details below.

Major Comment 1: Choice of snowpack measure. The authors characterize the winter snowpack by taking the average basin-wide value over the December-June period, including no snow days. My concern is that this measure does not capture just snowpack accumulation, but also aliases the length of the accumulation and melt seasons. For instance, a basin could reach the same peak SWE in year 1 and year 2, but if the snowpack begins accumulating at different points (say 1 December versus 1 January), the measure the authors use would report a very different value, even though the following warm season would likely be very similar hydrologically, at least from a snowpack perspective. Additionally, even if a fixed window were appropriate, the period seems much too wide. Looking at the histograms in Figure 2, the overwhelming majority of basin-years have snow durations, much less than 200 days, except at the highest elevations, which means that this window includes large chunks of time outside the snow season, especially at the lower elevations. I would strongly encourage the authors to use a more commonly, hydrologically-relevant measure such as basin-scale peak SWE to characterize the snow season. At the very least, the authors would need to validate their chosen measure by examining its relationship to warm-season streamflow vis-a-vis other snowpack metrics like peak SWE to justify its use as a hydrologically-relevant variable.

Public discussion: This is a very good point. In the revised manuscript, we will also consider peak SWE and then decide on whether to use that or mean SWE based on the explanatory power vs. streamflow and other impacts. Thank you!

Revision: We conducted a dedicated analysis on this point and found that using peak SWE instead of mean SWE produces very similar results in our setting, as the two measures are highly correlated. As suggested, we have included a discussion of this in the manuscript **(see lines 256ff)**, which we believe has strengthened the robustness of the paper. Thank you!

Major Comment 2: Snow drought definition. The authors define snow droughts as years in which their snowpack measure falls below the 30th percentile over the 2011-2023 period. Essentially, this choice imposes a fixed number of snow droughts (4) for each basin. There are numerous ways to define snow droughts, and the choice of method necessarily affects the set of events identified and thus the set of impacts one might ascribe to them (Gottlieb & Mankin, 2022). I would encourage the authors to consider a broader range of methods from the literature for identifying snow droughts and make sure that their results are robust to that analytical choice.

Public discussion: Thanks for this comment. We agree that the definition of snow drought has implications in terms of how we interpret impacts. Our definition and the resulting snow droughts align very well with our knowledge of what years were characterized by low snow accumulation across the Italian mountains, which was an indirect validation of our choice (see, e.g., `https://essd.copernicus.org/articles/15/639/2023/` or `https://iopscience.iop.org/article/10.1088/1748-9326/acdb88`). We will mention this as well as review other methods, at least in the Discussion section.

Revision: We have included a discussion of this in the manuscript **(see lines 256ff)**, which we believe has strengthened the robustness of the paper. Thank you!

Major Comment 3: Visualization of distributions and use of statistics. In Figures 1 and 2, it would be far more informative to show the distributions of local basin-year anomalies of temperature, precipitation, streamflow, etc., rather than the absolute values, as the current distributions largely show the differences in climatology across stations rather than the difference between snow drought and non-snow drought years. By calculating the departures from normal (e.g., degrees for temperature and percentages of normal for hydrologic quantities) and plotting those distributions, it would make those differences much more salient and easier to interpret. It would also make for a more appropriate statistical test, as it would compare within-basin anomalies in snow drought versus non-snow drought years, whereas now it also includes between-basin differences in climatology, which doesn't provide much information about the snow drought effect. Finally, the numbers the authors report about differences between snow drought and non-snow drought years (e.g., -50% length of snow season, -50% summer runoff, etc.) should reflect these local anomalies (if they do not do so already; I cannot tell where those values are coming from), rather than the pooled distribution.

Public discussion: We agree with this comment, and we will recompute results in Figure 1 and 2 based on anomalies rather than absolute values.

Revision: We replaced absolute values with anomalies, and results became much clearer. We would like to thank the reviewer for this very important comment.

Major Comment 4: Overinterpretation of GPP results. This is somewhat related to the last point, but the authors do not, as far as I can tell, apply any statistical tests to assess whether

the vegetation metrics they examine are significantly different between snow drought and non-snow drought years. My guess is that they are not, as the authors show quite a conservative uncertainty range (inter-quartile range in Figures 5 and 6), and the differences between snow drought and non-snow drought years appear quite modest. Even in the cases with the largest departures above 1500m, the median in snow drought years seems to sit at about the 60th percentile of the non-snow drought distribution, which hardly seems like meaningful support for the claim that GPP is higher following snow droughts. This section either needs to be supported by strong statistical evidence if it exists, or the authors need to report this accurately as a null result — to be clear, it would be a very interesting null result! — if that is indeed the case.

Public discussion: We thank the reviewer for this important comment. To formally assess differences between snow drought and non-snow drought years, we conducted additional statistical analyses based on annual aggregates for each catchment and elevation class. Specifically, we performed paired comparisons across catchments and computed effect sizes and confidence intervals using a t-based approach for both GPP and green-up timing. For GPP, results indicate a clear elevation-dependent pattern. At lower elevations (0–1000 m), differences between drought and non-drought years were small and not statistically significant (e.g., 0–500 m: mean difference = 3.4 [gCm-2], 95% CI [13.0, 6.2], p = 0.54). At intermediate elevations (1000–1500 m), differences were modest but statistically significant (mean difference = 10.7, 95% CI [2.1, 19.3], p = 0.019). At higher elevations (>1500 m), GPP was consistently and significantly higher in drought years, with larger effect ranges (e.g., 2000–2500 m: mean difference = 33.4, 95% CI [25.8, 41.0], p < 0.001).

In contrast, green-up timing showed a consistent and statistically significant response across all elevation ranges. Green-up occurred earlier in drought years at all elevations, with mean advances of approximately 2–3 days at low and mid elevations (e.g., 0–500 m: 2.4 days, 95% CI [4.2, 0.7], p = 0.016) and larger advances at higher elevations (e.g., >2500 m: 6.7 days, 95% CI [10.7, 2.6], p < 0.001). The magnitude of this effect increased with elevation.

Together, these results clarify the patterns shown in Figures 4 and 5. While the visual differences in GPP are modest and not statistically significant at lower elevations, the statistical analysis confirms a significant increase in GPP following snow drought at higher elevations. At the same time, the consistent advancement of green-up across all elevations suggests that phenological responses to snow drought are more widespread, whereas productivity responses are more strongly constrained by elevation.

We will revise the manuscript to include these statistical results and to more accurately reflect the strength and elevation dependence of the observed effects.

Revision: We added the above to the manuscript **(see lines 400ff)**, which we believe has strengthened the clarity of the paper on the topic of GPP. Thank you!

The title is a bit awkwardly worded. I would drop the "Multisectorial impacts of" part, as mountain socio-ecohydrology already captures the fact that the authors are looking at a diverse set of impacts.

Public discussion: Based also on comments from Reviewer 1, We will simplify the title as "Impacts of Mediterranean snow droughts on mountain socio-ecohydrology".

Revision: Done!

l. 107-108: It would be useful context to provide the snowfall fractions of your mountain

catchments, or report values from the literature.

Public discussion: Yes! We will take these from the literature, as computing them ourselves would require processing significant additional data.

Revision: Determining such figures for the Italian Alps proved challenging; however, we were able to provide some data on total snowfall and its variation with elevation **(see lines 104ff)**.

l. 154-155: Are these error values for peak SWE/snow depth? They seem quite large. More detail would be useful here. What are those errors as a percentage of average? How do they vary across elevations?

Public discussion: Thanks for this comment. While they might appear large, we should always bear in mind that IT-SNOW is a national-scale, operational modeling chain at 200/500 m resolution. This necessarily degrades performance compared to a point-scale application of a snow model. As we discussed in Avanzi et al., 2023, this performance is in line with other similar large-scale reanalyses. We will add this to the manuscript.

Revision: We added the above to the manuscript **(see lines 153ff)**.

l. 159: A bit more information about the BIGBANG dataset would be useful. Is it a reanalysis? Interpolated station data?

Public discussion: We will add this information in the paper. It is an interpolation of in-situ observations.

Revision: We added the above to the manuscript **(see lines 159ff)**.

l. 240: I believe the Hatchett and McEvoy citation is incorrect. In that paper, they identify warm snow droughts based upon 1 April accumulated P $>$ 100% and SWE $<$ 100% of normal and dry snow droughts using 1 April accumulated P $<$ 100% and SWE $<$ 100% of normal. This is a very different snow drought definition than the one the authors use.

Public discussion: We apologize for this mistake, and we will fix this passage in the manuscript.

Revision: We fixed this, we were simply pointing to the wrong Hatchett reference **(see line 247)**.

l. 341. It is unclear where the number of a $\sim$100% increase in melt-out events comes from, given that the increase at each individual elevation band is $<$100%.

Public discussion: We agree with this comment. As we wrote in the Abstract, we meant "up to $\sim$+100%" (precisely, 96% between 2500 and 3000 m). We will fix this.

Revision: We decided to remove these percentages, as they were in the end the result of quite small absolute differences and so they were a bit misleading. Now we mention absolute changes **(see lines 361ff)**.

ll. 348-349: It is also possible that warmer summertime temperatures in snow drought years (to which the snow drought itself may or may not causally contribute) drive increased evapotranspiration and thus lower streamflow and runoff efficiency (Goulden & Bales, 2014)

Public discussion: We totally agree and will add this to the manuscript.
Revision: We added the above to the manuscript **(see lines 504ff)**.

Figures 5 and 6: shading the interquartile range (i.e., the middle 50% of the data, or 6-7 years) is an overly narrow portrayal of the variability in these measures that makes differences appear significant, even when they may not be. I would encourage the authors to either show all 13 individual years (thin and light) with the median heavier and darker, or more of the full range of the data. This would put the snow drought vs. non-snow drought differences in their fully context of the range of variability.

Public discussion: See our main comment above. Differences are indeed significant for intermediate - high elevation & GPP, and significant for all elevations when looking at greening date. In the revised manuscript, we will present the data in a way that more clearly highlights the differences between drought and non-drought years.
Revision: We added the above to the manuscript **(see lines 400ff)**.

Figure 7: I don't believe a boxplot of elevations is appropriate here. Isn't the number of restrictions more relevant? A clustered or stacked bar plot showing the number of restrictions in different elevation bands would be more appropriate.

Public discussion: We agree and will revise this as suggested!
Revision: We changed Figure 7 as recommended.

Figure 9: Again, I am not convinced that the results support the GPP/growing season length conclusions.

Public discussion: See our extensive reply above.
Revision: We added the above to the manuscript **(see lines 400ff)**.

ll. 437-438: Your findings run counter to the Trujillo et al. (2012) paper, which finds increased vegetation productivity after snowier winters. Overall, this discussion section could better position your findings in the context of the literature and attempt to explain possible discrepancies between them.

Public discussion: We agree, and we will incorporate this point into the Discussion. Broadly speaking, both winters with abundant snowfall and winters with snow drought can enhance the following summer's productivity, but through different mechanisms. Snow-rich winters increase water availability beyond typical levels, which will likely boost productivity, particularly in elevation bands that are otherwise water-limited. In contrast, snow-drought winters can enhance productivity at higher elevations that are usually energy-limited and do not require additional water, but instead gain from a shorter-than-normal snow season. Therefore, our findings and those of Trujillo are not necessarily inconsistent. We will add this clarification to the manuscript.
Revision: We added the above to the manuscript **(see lines 489ff)**.

l. 441 Zhao et al., (2022) also show widespread increases in ET during droughts

Public discussion: Noted, we will add this!
Revision: We added the above to the manuscript **(see line 474)**.

l. 445: Pulliainen et al., (2017) also show that early snowmelt advances spring green-up across the northern hemisphere and enhances springtime carbon uptake, which feels like an important part of the literature to cite.

Public discussion: Noted, we will add this!
Revision: We added the above to the manuscript **(see line 471)**.

ll. 456-464: Much of the difference between the grassland and forested site likely has to do with the fact that soil moisture was measured at 30cm, while trees typically have much, much deeper rooting depths that allow them to access deeper soil water and/or groundwater or water in fractured bedrock, which respond more slowly to hydroclimatic variability (Baldocchi et al., 2021; McCormick et al., 2021)

Public discussion: We will include a comment on this in our discussion.
Revision: We added the above to the manuscript **(see lines 499ff)**.