Comments to Editor – HESS-2023-1911

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December 31st, 2023

Dr. Anne Van Loon

Handling Editor

Hydrology and Earth System Sciences

Dear Editor,

We extend our sincere appreciation for your efficient handling of our manuscript and for providing insightful and concise feedback following our response to the reviewers. In the subsequent paragraphs, we provide a summary of the changes implemented in the manuscript based on your valuable suggestions. Changes addressed in the response to reviewers document, not explicitly mentioned here, have also been incorporated into the revised manuscript. Thank you for your support and for helping us improving the manuscript :)

Additionally, we would also like to sincerely thank you for handling the manuscript in such a prompt and efficient manner.

Sincerely,

Oscar Manuel Baez-Villanueva

(on behalf of all authors)

Editor comments

E–C1: SSI (reviewer 1 & 2): Two of the reviewers commented on the use of the time scale for SSI. I agree with your rebuttal that there is no need to accumulate SSI because it reflects streamflow and therefore integrates catchment processes (including groundwater discharge and snow accumulation and melt). I do suggest to make the suggested changes in the manuscript to clarify this also for the reader (including the additional references).

All changes described in the response to the reviewers (R1–C2, R2–C2, R2–C3) have now been added to the revised manuscript.

E–C2: Zero lag (reviewer 1): I do not agree with your explanation that the lack of lag is related to a rapid catchment response. Catchment storage delays are already included in the accumulation period, which is a better indicator for rapid or slow response. Only processes that cause a strong temporal shift in response (like snow accumulation and melt) will show a clear lag. An optimal lag of zero was also found in other studies, which shows that shifts generally do not occur, even in slowly-responding catchments. Please reformulate your suggested revision of the text.

Thanks for the clarification! The text in the manuscript has been reformulated in L567–570 to:

In general terms, the cross-correlation and event coincidence analysis values decreased gradually from a lag of zero months to a lag of 12 months. This could be explained by the fact that only processes that cause a strong temporal shift in response (like snow accumulation and snowmelt) will show a clear lag response. An optimal lag of zero months was found by (Peña-Gallardo et al., 2019), which indicates that these shifts generally do not occur, even in slowly-responding catchments.

E–C3: SWE (reviewer 1 & 3): thanks for the explanation. I agree with this, but I have a fundamental issue here. The SWEI indicates a lack of snow ACCUMULATION, but what is important for streamflow is snow MELT. The effects of low snow accumulation are only noticeable in streamflow during the melt season, so low correlations between SWEI and SSI are expected. I would therefore suggest to use the Standardized Snow Melt and Rain Index (SMRI) by Staudinger et al. (2014) instead of the SWEI. Using SMRI might also change the conclusion that snow is not so important (reviewer 3).

Thank you for your comment. Indeed, the SWEI does not explicitly consider snowmelt, and we acknowledge that the SMRI could provide insights into how precipitation and snowmelt contribute to streamflow droughts. However, we believe that incorporating it might potentially alter the article's scope, including its objectives and overall narrative. Currently, we recommend indices and timescales that could be applied to ungauged catchments since precipitation (P), potential evaporation (PE), snow water equivalent (SWE), and soil moisture data can be obtained from openly available gridded datasets. On the contrary, the SMRI proposed by Staudinger et al. (2014) relies on a snowmelt model that requires a calibration strategy in each basin, making its application challenging over data-scarce settings.

To be transparent with our readers, we have included the following paragraph in L576–583:

The SWEI standardises the time series of SWE, indicating a lack of snow accumulation. The low correlation values and event coincidence rates observed in the evaluation of the SWEI may be also attributed to the fact that the SWEI does not explicitly account for snowmelt and its impact on streamflow conditions. The effects of low snow accumulation become noticeable in streamflow only during the melt season; hence, lower correlations between the SWEI and SSI are expected. For future studies aiming to assess the influence of snowmelt on streamflow drought, we recommend using the Standardised Snow Melt and Rain Index (SMRI; Staudinger et al., 2014) instead of the SWEI. The SMRI requires the implementation and calibration of a snow model capable of representing snow processes in the target catchments.

E–C4: Thresholds (reviewer 1 & 2): I agree with you that a sensitivity analysis of thresholds is beyond the scope of the study. But, as you mentioned in the rebuttal, it is helpful to discuss the uncertainty related to thresholds and include it as suggestion for future research.

All changes related to this point including the uncertainty related to the thresholds and the inclusion of the evaluation of the sensitivity of different thresholds for classifying drought events have now been added to the revised manuscript and are described in the response to the reviewers (R1–C6, R2–C4).

E–C5: Parametric & non-parametric drought indices (reviewer 3): thanks for doing the initial analysis. I understand that doing a full analysis of the effect of different parametric and non-parametric analysis is beyond the scope of this study, but I do agree with the reviewer that it potentially increases uncertainty if you calculate correlations between indices that are calculated with different methods. I was for example triggered by your comments that not all indices have values lower than -2 (p.11 l.255) and "that the SWEI and ESSMI have fewer events below the -1.5 threshold" (p.16 l.335). This shows that the distribution of the values of the indices is not the same between the indicators and this affects your conclusions of the coincidence of drought events. You should discuss what the effects of this methodological choice on the overall results are.

All changes related to this point that are described in the response to the reviewers (R3–C3) have now been added to the revised manuscript. Additionally, to clearly mention the impacts of this choice we have added in L589–594 the following statement:

The combination of parametric and non-parametric approaches to classify drought events can potentially increase the uncertainty in the evaluation, as the distribution of standardised values may differ among indicators. The selection of parametric or non-parametric approaches could impact the occurrence of extreme events and, consequently, the correlation values and event coincidence rates. For instance, in the case of the non-parametric ESSMI and SWEI, less extreme values were observed compared to the SPI and SPEI parametric approaches.

E–C6:Furthermore, I agree with you that the Results and Discussion do not need to be separated (reviewer 2), but I do suggest to add more general discussion about what insights this research brings on a more general level (including references), either in the Results or in the Conclusion section. I also would suggest to shorten the title even further. And finally I don't see the need to mention agricultural and socioeconomic droughts (reviewer 2).

A more general discussion about what insights this research brings on a more general level has been now included in the Conclusion Section between lines L552–566:

Mountainous catchments play a pivotal role in supplying freshwater resources to both society and ecosystems. They contribute substantial runoff, redistribute winter P to spring and summer, and mitigate flow variability in lowlands (Viviroli and Weingartner, 2004; Viviroli et al., 2011). Numerous studies have demonstrated the impact of temperature increases and changes in snowfall fraction on droughts in mountainous areas (Marke et al., 2018; Blahušiaková et al., 2020), potentially affecting both the total volume of streamflow and its seasonality. Given that mountainous catchments generally remain ungauged, it is crucial to identify which drought indicators and timescales of monitored variables can be used to assess streamflow drought conditions. The approach demonstrated in this paper highlights the effectiveness of using individual drought indices to characterise streamflow drought, while also highlighting the varying behaviour of these indices across catchments with diverse hydrological regimes. This suggests that combining different drought indices based on catchment-specific responses (e.g., catchment memory Alvarez-Garreton et al., 2021) could provide further avenues for strengthening the relationship between meteorological, soil moisture, and snow drought indices as proxies of streamflow drought. Although catchment characteristics are important in understanding the relationship between meteorological, soil moisture, snow drought and streamflow droughts, this article provides a complementary perspective on how this relationship can also be related to the hydrological regime of the catchments. Our findings indicate that hydrological regimes offer valuable insights into the time lag at which these indices could be used to assess streamflow drought.

Also, the title has been shorten and now reads: *On the timescale of drought indices for monitoring streamflow drought considering catchment hydrological regimes*. Finally, we have removed the references to both agricultural and socioeconomic droughts :)

References

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