Dear Reviewer,

We thank the reviewer for providing critical and constructive comments. This reply is not a paper revision. Here, we would like to describe how we intend to address your primary suggestions and to answer the most critical questions. Please see our responses to your points below.

Reviewer's comments	Our Responses
The paper outlines a method for estimating long term changes in the return frequency of extreme drought and cold events, which are associated with major livestock mortality events in Mongolia. The author's extend empirical climate data for the region prior to the instrumental record using tree ring data, an indicator of summer precipitation, which is inferred as Palmer Drought Severity Index. They infer pre-instrumental winter minimum temperatures based on weather stations from Irkutsk in Siberia and neighbouring stations for periods when data is missing. They apply extreme value analysis to estimate changes in the frequency of extreme summer droughts and winter minimum temperatures over the reconstructed records. They find that the frequency of extreme droughts has been increasing over the period of analysis but that the frequency of extreme cold winter events shows no clear trend. The analysis outcomes are potentially useful for understanding changes in extreme climate events which can have important implications for environment and society in Mongolia. However, I would like to see a better embedding of the approach within existing literature, better motivation and explanation of methodological choices and improved formatting and presentation of the paper.	Thank you very much for your positive comments.
1. The authors put forward a method for estimating changes in the frequency of extreme climate events, however there is little reference to previous approaches to estimating changes in extreme climate event frequency. There should be a deeper exploration of previous approaches to estimating changes in extreme climate events, their strengths and weaknesses and the contexts in which they were applied. This allows the reader to better assess the novelty and scientific contribution of the approach outlined here. You don't motivate you choice of extreme value analysis in comparison to other approaches.	Thank you very much for your comments. We have discussed long-term climate variability from the previous literature specifically associated with dzuds, such as D'arrigo et al. (2001), Davi et al. (2015), and Davi et al. (2021), Davi et al. (2010) in Section 1.1. In the revised version, we will provide a brief overview/classification of the extensive literature that covers alternate methods for modeling the changing frequency of extreme climate events.

2. The authors extend instrumental records with additional proxy data or data from locations remote of the study area. Again, there should be more reference to previous approaches to doing both of these things. There should also be stronger motivation to the approach chosen here and the variables chosen to include in your analysis. In table 4. For instance, why is the Arctic Oscillation index of relevance and what relation would you expect between that and the PDSI or winter temperature records?	We appreciate your comments. We hypothesize that the Arctic Oscillation (AO) index may be associated with summer drought and winter temperature, given Mongolia's climate characteristics, following Cohen et al. (2010), Iijima and Hori (2018), Munkhjargal et al. (2020), and He et al. (2017), among others. A brief discussion of the dynamics of the summer atmospheric moisture transport and the winter jetstream as related to the precipitation and temperature and to the AO will be added in the revised version of the paper.
3. You present results such a BIC scores but it would be helpful if you indicate to the author what these scores represent and what is an acceptable on unacceptable score and why that might be the case.	The BIC and AIC are standard information-theoretic criteria whose relative magnitudes allow one to choose one model over another. We will add a note as to their interpretation in the revised version.
4. Figure 2: You present a smoothing of the PDSI. It is not clear if you are using this smoothed value in your analysis or the original. If the former, please explain why and how you came to that smoothing window. Qualitatively, it doesn't seem to capture the variation in the data which looks to be better represented by a higher frequency variability. If you do use this moving average then you should rigorously demonstrate that the moving average window chosen captures the variance in the data. For example, by applying Fourier analysis to look at what timescales demonstrate the highest power in the dataset. If the latter, then do not show this smoothing as it is simply confusing.	We are using original values, not smoothed values. We showed the lowess smooth lines because we wanted to show potential PDSI data trends to explore hidden trends without assuming any statistical traits.
5. Your analysis deals with drought and cold winters separately, whilst you outline that it is the co-occurrence of these events which contribute mostly to livestock die off. Therefore, I would like to see, if possible, an estimation of the frequency at which these events co-occur and if that has changed over time. Also, some discussion on the mechanistic relation between summer drought and winter minima.	Thank you very much for commenting on the critical point. We agree with you that the estimation of the frequency at which two events co-occur and if it has changed over time are of interest to the community. Begzsuren et al. (2004) examined the co-occurrence of these extreme events by using 51 years of observational data. They identify that mortality rates are highest in *combined* drought and dzuds years than those with dzuds or drought *alone*. We will add this and/or related analysis in the revised version.
6. The use of the Siberian dataset needs stronger justification I feel to convincingly demonstrate that it captures variability in extreme winter temperatures for the period where we have instrumental observations. Also, the use of neighbouring stations to fill in missing data seems very suspect given that you already demonstrate that climate variations at these stations demonstrate no	We have used the Siberian (Irkutsk) dataset (1820-2016) because it covers the periods when there exist no high-resolution gridded climate datasets. Existing studies, such as Munkhjargal et al. (2020), Iijima et al. (2018), and He (2017), suggest the winter temperatures between Mongolia and Siberia are correlated spatially.

significant statistical relation with climate in Mongolia. Please justify this better.	We did not use Irkutsk data to fill in the missing data of Mongolia. We have included it as one variable in the statistical analysis of GEV and GPD. What we imputed/filled in missing values are for the Irkutsk data itself. We did so by using pattern matching methods, by Gibbs sampling using predictive mean matching method (Van Buuren and Groothuis-Oudshoorn, 2011).
7. I would play down discussion of risk analysis and insurance in your paper. You don't really address those questions so don't oversell. Also, it I don't see how this method provides an early warning signal. Maybe if you address the point above it can.	We will revise the paper to connect to prior related work on dzud insurance that would be informed by our analysis, and also elaborate on how the information on the summer drought could act as an early warning signal using our model. Thanks for raising this issue
8. Some figure axes miss units	Thank you very much for pointing out these. We will adequately address this when we revise the manuscript.
9. General formatting, presentation, figure and table design, section numbering should be improved.	We will adequately address this when we revise the manuscript.

Reference

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