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Hydrology and Earth System Sciences Discussions

Interactive comment on "Contrasting trends in hydrologic extremes for two sub-arctic catchments in northern Sweden – does glacier melt matter?" *by* H. E. Dahlke et al.

Anonymous Referee #2

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General assessment

This ms addresses questions related to the effects of climatic variability on peak flows in subarctic catchments located in the discontinuous permafrost zone. The authors use a comparative time series analysis approach, drawing upon discharge and mass balance time series that span almost a century. The questions addressed are important both scientifically and practically, and the data sets are unique in their length, particularly the glacier mass balance record. The analysis generates some novel insights that augment the existing literature on the topic. I recommend that the ms be accepted for publication following revision to address the specific comments provided below, which are intended to help the authors clarify the presentation and strengthen its contribution to the literature.

Specific comments

1. The title includes the term "hydrologic extremes," but the analysis focuses only on flood peaks, not low flows. I recommend that the title be modified to provide an accurate representation of the content.

2. The introduction should be revised to provide a more nuanced and complete summary of the literature and to provide a stronger bridge between the literature review and the stated objectives. The next two comments provide more specific directions.

3. In the introduction, the authors refer to findings of both increasing and decreasing streamflow trends in a rather broad-brush manner. It would be useful to clarify the specific metrics used in the different studies (e.g., monthly vs annual runoff) and to consider the seasonal signatures of streamflow trends associated with warming and glacier response. For example, Milner et al. (2009, Figure 2) showed a hypothetical sequence of streamflow response to glacier volume change. However, that schema did not illustrate changes to spring-season snowmelt associated with spring-time warming. Déry et al. (2009) illustrated empirically the variation in the seasonal pattern of warming-induced streamflow trends for a range of nival and glacier-fed catchments in western Canada. I also recommend that the authors refer to a classic chapter on floods in cold regions by Church (1980) to provide more context for the roles of different flood-generating mechanisms and how they might respond to climatic warming and glacier changes.

4. At the end of the introduction, the authors provide two sentences that indicate the types of analysis that were conducted. I recommend that the authors restate these as objectives, hypotheses or questions in a way that they clearly relate to gaps in our understanding and link back more strongly to the literature review; doing so would clarify the novel contributions made by this study. In particular, the reference to large-

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scale teleconnection patterns does not relate to any of the reviewed literature. The authors should consider adding a paragraph that reviews the hydrologic consequences of these climatic oscillations and why they might influence flood generation in the study catchments.

5. The authors should include information on changes in glacier area within Tarfalajokk catchment over the period of record.

6. The authors used a correlation test to support the validity of the Gumbel distribution for fitting flood frequency relations for the entire periods of streamflow record. However, they then showed that the assumption of stationarity is not valid, using the trend analysis on quantiles from 10-year moving windows. I am not an expert on frequency analysis, but was taught in my undergraduate hydrology courses that the classical approaches are based on an assumption of stationarity, which is clearly not valid in this case. Another potential issue is that the peak flow events were generated by at least two different processes: Table 6 reveals that some events were associated with high air temperatures, with the implication that they were dominated by meltwater, and others were associated with intense rainfall. Would a simple Gumbel distribution be valid for a mixed-population frequency analysis? Church (1980) and Waylen and Woo (1982) conducted frequency analyses that explicitly accounted for multiple flood generation mechanisms. A further concern is that, even if the assumptions underlying frequency analysis were valid, estimates of 100-year floods from 10-year samples would be associated with high uncertainty. The authors need to address these concerns when preparing the revised ms, possibly through additional analysis.

7. To analyse quantiles derived from the flood frequency analyses, the authors used generalized least squares (gls) regression so as to account for temporal autocorrelation in the residuals (given that flood quantiles from consecutive 10-year windows would be based on 9 years of common data, and thus should be strongly autocorrelated). Further detail on the gls regression approach would be appreciated. For example, what order of autocorrelation was included? It would be useful for less statistically minded

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Interactive Comment

readers of this article to have some clarification of and rationale for the methodological choices made in the analysis, especially for readers interested in applying these methods to other data sets.

8. In relation to the trends in the flood quantiles, I would find it interesting to know the extent to which these are reflecting changes in the mean versus the standard deviation.

9. In the caption for Figure 5, the last sentence indicates that the variability around the longer-term trends is a "response to decadal and interannual forcings." First, this sentence should be moved to the main body of the paper. Second, did the authors conduct a formal analysis upon which to base this comment, for example, by regressing deviations in quantiles from the longer term trends against indices such as NAO? I recommend that the authors conduct additional analyses to try to link the correlations illustrated in Table 7 with the temporal patterns in flood quantiles.

10. The process-based speculations for the cause of the declining flood magnitudes for Abiskojokk appear straightforward and are plausible in the context of the relevant literature. However, I am less convinced about the proposed explanations for the increasing trends for Tarfalajokk. The authors argue that the relative lack of response to rainfall events at Abiskojokk could be associated with permafrost thaw, which would reduce the responsiveness of the catchment to snowmelt and rainfall. Wouldn't this process also be active over the 70% of the Tarfalajokk catchment that is glacier-free? How about generation of rainfall-runoff from the 30% of the catchment that is glacierized? The authors speculate (p. 1061, line 23ff) that the decrease in glacier area has increased responsiveness of Tarfala catchment. How substantial was the change in glacier area? As an alternative hypothesis, perhaps decreased snow accumulation (in combination with thinning and decreasing area of firn cover) could be resulting in greater response of the glacier to rainfall events. I encourage the authors to consider a range of alternative hypotheses about the processes that may be responsible for the shifts in flood magnitudes, and to evaluate each as much as possible given the existing information base.

11. There a number of minor grammatical and typographical errors that should be corrected during revision.

References

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