Reviewer comments – Comments to responses made by the author team (third version)

Paper entitled: "Understanding Changes in Iceland's Streamflow Dynamics in Response to Climate Change"

Journal: HESS
Authors: Helgason et al.

First Review submitted: 3 March 2025
Second Review submitted 19 September 2025 and authors' response received 29 October.

Third Review (below) submitted December 2025.
Reviewer comments to the third version (dated 29 October) are provided below. As the response by the authors were only available as a pdf file, I have chosen to add my comments to my file dated 19 September. Below, comments to the first version are in italics, the authors' response in normal font and my original response to the these (second version) in blue. Note that the authors' response to this second review is missing (not copied from the pdf). However, my response to these is marked as "Author response: " along with my remarks (using a lighter blue that in the second review). Only comments that remained from the second round are kept.

Reviewer response to the general and specific comments (originally provided as a separate file) are given at the end of this document.

General comments – first round (italics)
including responses from authors (normal font) and reviewer's remark to these (blue).

The main aim is to "investigate the multi-annual variability in Iceland's climate and analyse changes in streamflow, aiming to link these changes to shifts in hydro-meteorological drivers and changes in glacier extent and mass". The latter part "changes in glacier extent and mass" is not explicitly analysed in the paper through own analyses, rather the results are discussed in light of previously reported changes in glacier extent and mass. This in contradiction to shifts in hydro-meteorological drivers, which are quantified as trends, although the link to changes in streamflow is discussed in qualitative terms. This distinction should be made clear to the reader from the start.

While our study does not directly analyze glacier mass balance, we do compare trends in annual and summer melt-season streamflow to changes in glacier extent (Figure 10). Beyond this, glacier extent changes and mass loss are currently discussed in the context of previously documented trends rather than through independent calculations. We have made this clear to the reader from the start by making the following changes to the text: "[…] investigate the multi-annual variability in Iceland's climate and analyze changes in streamflow, aiming to link these changes to shifts in hydro-meteorological drivers, catchment attributes, and changes in glacier extent."

Fine; however, it should be made clear in the text where the data on changes in glacier extent/area stem from, see Figure 9 (no reference included).

Author response: Fine

The study is thorough in that it investigates trends in streamflow and its meteorological drivers for different periods, temporal resolution and variables. The analysis is straightforward, using well-established methods. It is descriptive in its presentation, with an extensive result section, listing the results in a sequential way supported by a series of informative figures. As such, it resembles more a scientific report than a research paper. The results are discussed in a qualitative way, comparing the streamflow trends with trends in precipitation and temperature. The reasons for choices taken are not always well motivated; for example, what is the purpose/added value of looking at the daily trend in smoothed 21-day averages? And why is the spring freshet, rather than the timing of the peak flow (often used in the literature), chosen (or why not both)? Choosing indices commonly used in the literature, facilitates comparison with other studies. The paper would have benefitted from being tightened somewhat with a clear motivation for why - and how - the different analysis contributes to the aim and research question defined.

We acknowledge that the results section was extensive in the previous version of the manuscript. To enhance clarity and focus, we have streamlined this section by emphasizing the most critical findings that directly address our research questions. We have relocated detailed analyses (Figures 6 and 11 that show the sub-seasonal trends in meteorological forcings and streamflow 7 for all catchments) to the supplementary material. Since Figure 7 effectively summarizes these trends for meteorological forcings, we believe it is sufficient for the main text.

The paper has benefitted from moving material to the Supplement, although the presentation of the many trends is not always consistent when it comes to presenting non-significant trends vs significant trends. Sometimes a trend – encompassing only a few significant trends – are presented without a remark on significance. For example, related to Fig.5, it is written (author text in black):

- "The winter and spring seasons are characterized by modest positive or near zero trends". These are non-significant trends and one should further not distinguish between modest and near zero trends as long as these are insignificant (nor is there a definition of 'modest' versus near zero').
- "In the summer season, precipitation increased slightly in the northeast region and decreased in the southwest". No comment on significance here either, although in this case there are a few significant trends.
- "Analysis of temperature trends for period 2 (Figure 5c) shows less statistical significance in seasonal temperature trends compared to period 1, with winter and summer exhibiting the least pronounced (and mostly insignificant) trends". Summer does show a considerable number of significant trends in the southwest (although the number of significant vs non-significant trends is not provided.

These examples highlight the need for the authors to carefully go through the text and ensure that trends are presented in a consistent and scientific way. Although focus should be on whether a trend is significant or not, one may comment on regional consistency in trend direction even though these are not all significant, but this should then be made clear.

Further, any comment on magnitude on trends that are not significant should be made with care and preferable avoided, as these are just that – non-significant changes.

Author response: Fine

The combination of maps showing spatial trends in streamflow for different periods and seasons (shown as point values at the gauge, Figure 7) and heatmaps showing an overview of positive and negative trends for annual as well as seasonal trends in streamflow (Figure 8), is informative and a good starting point for discussion. Note, such heatmaps are not provided for meteorological forcings although stated in the replay above.

Author response: Fine (valuable addition).

We have added a paragraph to the methods chapter that explicitly articulates the motivation for including sub-seasonal analyses. "While annual and seasonal trend analyses are valuable for identifying long-term hydrological changes, they may overlook important shifts occurring at finer temporal scales. Sub-seasonal trends can provide valuable insights even when no significant annual or seasonal trends are detected. Moreover, when seasonal trends are present, this analysis helps pinpoint when changes occur in greater detail."

Fine, however, when commenting on September 20, the period this date represents given a 21-day moving average, i.e., 10-30 September, should be added to the text (e.g. in brackets).

Author response: Fine

We appreciate the reviewer's suggestion regarding the inclusion of additional catchment attributes in the analysis. We have included an analysis of correlations of trends with all catchment variables in the LamaH-Ice dataset.

Fine; however, these should be introduced under Data, i.e., the specific catchment attributes included (e.g., as a table), and can be grouped into types in the main text for space reasons.

Author response: Fine, the added text to the main manuscript is valuable; however, it is suggested that it is accompanied by a table (in the Appendix) that list all the attributes included in the analysis, their definition and reference if relevant.

We have expanded this analysis to other catchment attributes. We have thus removed figure S21 and added a discussion on significant correlations between timing metric trends and catchment attributes. Interestingly, while no correlation is found between the timing of onset of spring freshet and catchment mean elevation, the timing trends are correlated with catchment elevation standard deviation, indicating that flatter catchments experienced an acceleration in snowmelt onset.

Good to include all catchment attributes, and these are interesting results. However, hard to judge their relative importance without knowing their co-correlation.

Author response: Fine (valuable addition)

Blöschl et al. (2017) found that the timing of annual maximum floods in southwestern Iceland shifted later in the year during the period 1960–2010, while in northeastern Iceland, flood timing remained stable or occurred earlier, as noted in the introduction. Our results show that the timing of annual peak flows has not systematically shifted in the over the periods that we analyzed.

This reference is now included in the Introduction, but not referred to in the Discussion. The latter is recommended as the key point of mentioning related studies are to compare their findings with your study.

Author response: Fine

We have also expanded the discussion section to discuss findings in the analysis of trends in streamflow variability (coefficient of variation, flashiness index, baseflow index) and correlations between streamflow trends and catchment attributes.

Fine; suggest to link these analyses to the first part of the study, i.e., multiannual variability in hydrometeorological variables (rather than after the trend analysis). It can then be picked up upon when discussion the trends.

Author response: Fine, I see your point.

To maintain focus and conciseness, we will not expand the discussion to include comparisons with studies from other glaciated regions.

This does not appear as a valid argument, notable as it is argued that the study contributes to global knowledge on … (last sentence in abstract). Thus, it is recommended to link the results to similar studies in comparable environments. This does not have to be comprehensive (or a global review), but it is important to put the results in a wider context, particular as it provides mixed results as to trend magnitude and direction (variability in both space and time).

Author response: Fine

Issues that need clarification/discussion

1. **Presentation of the dataset**:
    - *It is stated that 38 stations (later reduced to 37 due to the homogeneity test) were used, however, in Section 3.2 one refers to the whole dataset of 107 catchments in the LamaH-Ice dataset, including human influenced catchments, and in the Data section reference is made to 79 stations (Section 2.3). This is confusing;*

      We analyze trends in meteorological forcings using data from all 107 catchments in the LamaH-Ice dataset. For streamflow trends, we use 25 gauges for period 1 and 37 gauges for period 2 (following the homogeneity test). We acknowledge that the current wording may be unclear, and we have revised Section 2.3 to explicitly state the number of stations used in each case to avoid confusion

      It is now clearer which number of stations that are used, but Section 2.3 still states "In this study, we use timeseries for total precipitation, snowfall, and temperature for all 107 catchments in LamaH-Ice." This still appears confusing, and if the case, it should be clearly stated for which analysis these 107 stations are used. Further, related to Figure S2, one refers to 111 catchments.

      Author response: Fine

- *Are any of the catchments analysed sub-catchments of larger catchments included?*

  A few of the catchments included in our analysis are sub-catchments of larger catchments that are also analyzed, though this is relatively uncommon. This can be observed in the figures displaying basin outlines along with gauge locations (e.g., Figures 3, 8, 12, and 13). To ensure clarity, we have explicitly stated this in the Data section, as follows: "Some gauges represent nested sub-catchments of larger river systems. Specifically, gauges 46 (Jökulsá á Fjöllum at Upptyppingar) and 59 (Kreppa) are sub-catchments of gauge 45 (Jökulsá á Fjöllum at Grímsstaðir); gauge 86 (Tungnaá at Maríufoss) is nested within gauge 102 (Þjórsá at Þjórsártún); and gauges 8 (Brúará at Dynjandi), 36 (Hvítá at Fremstaver), and 79 (Sog at Ásgarður) are sub-catchments of gauge 98 (Ölfusá at Selfoss). Rather than exclude gauges 45, 98, and 102, we retain them in the analysis because they integrate substantial additional drainage areas downstream of their respective sub-catchments."

  This is an important clarification, and needs to be reflected in the discussion, i.e., these three stations do not represent independent stations when it comes to number of stations with significant trends. On the other side, the trends of sub-catchments can provide important information about spatial variability in trends, e.g., whether they are consistent or diverse.

  Author response: Fine (important clarification)

- *The analysis is done for two different periods; 1973-2023 and 1993-2023, 31 and 51 years respectively). It would have been interesting to know the mean streamflow, precipitation and temperature for the two periods.*

  We have added a table in the Supplement (Table S2) that shows the mean streamflow, precipitation, and temperature for the two analyzed periods.

  Fine; however, this is not reflected in the discussion when summarised over the stations, i.e., is Period 1 overall wetter/drier and warmer/cooler than Period 2? Are there regional differences among stations?

  Author response: Fine

- *In Figure 1 the period 1951-2024 is used; why is this period chosen? Later in the text, the year 2024 is highlighted as a particular dry year and has been given a separate paragraph in the discussion. Why this focus on 2024 if not included in the trend analysis?*

  The period 1951–2024 was chosen primarily based on data availability from the ERA5- Land reanalysis. Initially, ERA5-Land was available from January 1, 1950, but it has since been extended back to 1940. In the updated manuscript we now use the period 1950-10-01 to 2024-09-30. The year 2024 is highlighted separately in the discussion due to its extreme conditions, which underscore the high variability in precipitation and temperature in Iceland. Although 2024 is not included in the trend analysis due to the lack of available streamflow data for that year, its recent occurrence makes it particularly relevant for water management and stakeholders in Iceland. We have clarified this distinction in the revised manuscript.

Fine; However, it should be introduced up-front in the paper that the 2024 event will be discussed in particular. Now this appear a bit ad-hoc in the discussion.

Author response: Fine, although I wonder about the use of the term 'contemporary example'; could you not simply say 'recent example'?

- *Figure 1 further includes a rolling 5-year mean and trendlines; how do these trendlines relate to the trends later calculated?*

  The trendlines in Figure 1 provide a visual representation of overall long-term changes, while the formal trend analysis later in the manuscript is based on different time periods. We have explained this difference

  Fine; however, it is important to state whether the trendlines shown are derived based on annual values or the rolling 5-year mean. The latter is not recommended due to dependencies among the data points.

  Author response: Fine

- *The area of the catchments should be provided as it is important for evaluating the spatial variability in meteorological variables, notable in large catchments where a mixed trend pattern may impact the catchment average trend signal;*

  We have included catchment areas in Table S1 in the Supplement to help assess spatial variability in meteorological trends.

  Fine; however, I miss a comment on the size of the catchments when discussion trends, as a large catchment may experience diverging trends across the catchment area. The can be assessed by looking at trends in gridded ERA5-Land (see link below to the Climate Change Knowledge Portal).

  Author response: Fine, as a statement of facts, however, it is recommended to add the aspect of area into the discussion when commenting on spatial variability in the streamflow trends, i.e., some catchments representing significantly larger areas than others.

- *Uncertainty is not discussed, neither in terms of the observed data (streamflow) nor in the gridded ERA5-Land variables (temperature and precipitation), and how this may impact the results;*

  We recognize the need to discuss data uncertainties. We have added the following sections to the manuscript: "The uncertainty in the streamflow observations is discussed in detail in the LamaH-Ice data description paper (Helgason and Nijssen, 2024a). Streamflow measurements in Iceland are prone to interruptions (e.g., ice disturbances or instrument malfunctions), particularly during winter, which can reduce data availability and introduce additional uncertainty. Moreover, uncertainty in older streamflow periods is higher than in recent periods due to older instrumentation with greater uncertainty."

Fine; however, based on this assessment, would you consider some of the results more uncertain than others? Ref. winter streamflow measurements being more prone to interruptions than summer flow and the fact that the main low flow season is in winter.

Author response: Fine addition, however, should not the last part of the sentence read "than those based on summer flows and more recent years", being two separate aspects).

- *It is stated that precipitation in the ERA5-Land reanalysis is underestimated for Iceland (Helgason and Nijssen, 2024a); what about temperature? Cryosphere processes are very sensitive to air temperature, notable the zero-degree crossing.*

  To address this, we have assessed temperature differences between ERA5-Land and a another regional reanalysis (RAV-II) for the catchments in LamaH-Ice. See discussion in Sect. 2.3 in the main manuscript and figure S2 in the Supplement.

  This provides additional information, but the comparison of two reanalysis datasets does not necessarily contains information about the representativity of ERA5-Land versus observations. The revised Supplement compares observed temperature for a weather station (Reykjavik), with catchment average temperature derived from ERA5-Land. Why not compare the grid average of the grid cell where the weather station is located? This would provide a more direct comparison.

  Author response: I agree that comparing a grid cell average with the station observation, compares a point value with an areal average, still, the area of the catchment would in many cases be larger than the area of ERA5 Land grid cell. However, it is fine to skip this given that a note is made that one is comparing two reanalysis dataset and not observations as such.

  It is stated that ERA5-Land is slightly cooler that the observed series. Is a different around 2.5 °C seen as 'slight'?

  Author response: Fine

  Further, in the presentation of Figure S3, it is said that "both reanalyses indicate increasing precipitation trends in the east and northeast, transitioning to decreasing or near-zero trends toward the southwest. This spatial coherence strengthens confidence in the qualitative pattern of change." Looking at the maps, there are clear differences in the trends, also in the direction of trends (some of which are significant, in both increasing and decreasing direction). This should be corrected in the description and implications for the study discussed.

Author response: Fine, good to learn that the erroneous data were identified and the updated dataset provided more consistent results.

- *How does ERA5-Land perform for other glaciated areas of the world?*

  While ERA5-Land's global performance is an interesting question, our study is focused specifically on Icelandic conditions.

  The comment related in particular to glaciated areas and whether there were any studies evaluating the performance for these areas. Recommended to check.

  Author response: Fine

- *Has the ERA5-Land dataset been evaluated for its ability to reproduce trends in observations for Iceland? If not, it is recommended to include an at-site comparison where observed (station) precipitation (P) and temperature (T) time series are compared with the ERA5-Land grid cell representing the location of the stations;*

  ERA5-Land has not been explicitly evaluated for trend reproduction in Iceland. This has been done for temperature in other areas, e.g. Turkey (Yilmaz, 2023) and China (Zhao & He, 2022). We have included a comparison of observed station-based temperature time series with ERA5-Land data (See discussion in Sect. 2.3 in the main manuscript and figure S1 in the Supplement). Precipitation observations are difficult to use for this kind of comparison due to snow and wind in winters. However, we have compared trends in precipitation between ERA5-Land and another reanalysis dataset (CARRA: See discussion in Sect. 2.3 in the main manuscript and figure S3 in the Supplement).

  See comments above; the obvious would be to compare trends based on station data with trends in the corresponding grid cells in ERA5-Land.

  Author response: Fine; very valuable addition.

- *It is suggested to present spatial trend patterns in the meteorological variables for the whole of Iceland. By mapping the trends for each grid cell (ERA5-Land dataset) over Iceland, one can detect potentially regional diverging trend pattern that may help understand the spatial aggregated trends in streamflow for large catchments (as they may cover an area with mixed trend patterns).*

  We analyze trends for all 107 catchments in LamaH-Ice, providing high spatial coverage across Iceland. Given the consistency of trends observed across regions (see Figures 4 and 5), a gridded analysis would not add substantial new insights. Our approach ensures that catchment-scale trends, which are most relevant for streamflow analysis, are well represented.

  The Climate Change Knowledge Portal explores trends in temperature and precipitation for different periods and seasons on a gridded as well as catchment scale for countries around the world, including Iceland. Thus, these data exist, allowing to detect regional as well as within catchment spatial variability in trends. It can be highlighted that the current paper adds to existing knowledge by exploring catchment average trends and the link to streamflow trends.

Iceland - Trends & Variability (ERA5) | Climate Change Knowledge Portal

Author response: Missing response, but OK (author's choice).

- *Further, if more than 10% are missing, that year is excluded from the trend analysis. How is a missing year dealt with in the time series; just skipped or indicated as missing? Is the assessment of significance adjusted accordingly?*

  When a given year (or season) has more than 10% missing daily data, we exclude that year from the analysis. This ensures that only years with sufficiently complete records are used to represent the typical flow conditions. We omit series with more than 20% of annual/seasonal values missing. A missing year is dropped from the series before calculating trend and significance. We acknowledge that omitting these years reduces the total number of observations and may affect the trend estimates and the significance test's power. This is discussed in Sect. 2.3 (Data)

  For clarification: did you mark the year as missing in the time series prior to the trend analysis or simply remove it (resulting in a shorter time series)?

  Author response: Fine (years are removed); however, it is important to know the number of years removed (ref. comment earlier on stating the number of missing years in period 1 and 2 specifically) to judge the impact. It should also be made clear in the text that this is how missing years are dealt with. If the number is low (and it is not an abnormal year), it will have minor impacts on the trend over the whole period.

- *The trend direction is shown as blue and red. Is there a range in z-values for when a trend is defined as zero?*

  We do not use a range in z-values for zero trends.

  Zero trend must still be indicated through the accuracy in the z-value. The question further relates to how to report positive and negative trends, and restrain for commenting on weak and non-significant trends.

  Author response: Fine

- *Suggest to focus on discussing significant trends and only comment on non-significant trends if there are a clear regional or temporal pattern, which then contains information beyond the individual station.*

  Our manuscript already follows this approach for meteorological trends, where we primarily discuss significant trends and comment on non-significant trends only when they exhibit a clear regional or temporal pattern. To ensure consistency across all variables, we have refined the results and discussion sections to apply this approach to streamflow trends as well.

Fine; however, there are still inconsistencies as to how non-significant trends are presented, ref. earlier comments.

Author response: Fine

- *A miss a discussion on the cause of the sharp, and consistent, transition between positive and negative trends seen in Fig. 6.*

Miss an answer here.

Author response: Fine

Consistency in terminology:

- *Both watershed (US) and catchment (UK) are used (choose one)*
- *Both fall (US) and autumn (UK) are used (choose one)*

Both terms are understood and accepted in both countries and widely used in English around the world. The interchangeable use of these terms does not lead to confusion and we therefore see no need to change this.

I disagree and suggest to be consistent. It was further noticed that also the term 'basin' is used; i.e., three terms are used for the same term (number of times given in bracket): basin (8), watershed (7) and catchment (49). What to gain by using different terms, is not clear.

Author response: Fine

Reviewer comments – reviewer's response to the third version (December 2025)

Paper entitled: "Understanding Changes in Iceland's Streamflow Dynamics in Response to Climate Change"

Journal: HESS
Authors: Helgason et al.

Second review submitted 19 September 2025, and authors' response received 29 October, then as a pdf. My comments to the response made (not included here) are provided below (in blue). Some of which are marked as **highly recommended revisions**.

General comments

Overall, the paper has significantly improved, addressing in a thorough way most of the remarks and concerns raised.

The comments below adds to the response given to the first and second round of review (see above).

1. Overall, choices made are not always well argued, it is often stated this is what we do, not why.
This is a general remark that has not been specifically responded to in the author response, which is fine at this stage in the review process as it is in the authors' interest to ensure the study is well motivated.

2. Carefully check Figure legends as they:
   - do not always refer to the subfigures (e.g. a), b) and c)).
   - do not need to repeat methodology in the legend (ref. Figure 1. – how trends are calculated and Fig 3 – how correlations are calculated).
   - sometimes repeats heading of subfigures and use different fonts, e.g., Figure 4 and Figure S9 (periods are given at three places).

   This more specific remark has not been responded to explicitly, but partly addressed other places, and I leave it with the authors to carefully check this in the final version of the manuscript.

3. The result section separates between multiannual variability (Section 3.1) and trend analysis (Section 3.2); however, trends are also presented in 3.1.1. Thus, the result section should be better structured.

   Author response: Fine, you may consider using 'changes' rather than 'tendencies' in the suggest title for Section 3.1.1. (as also used in the revised text).

Specific comments

- Abstract: sub-seasonal trends are not mentioned here.
  Author response: Fine

- Section 2.2: the heading is misleading as it does not describe the hydrological regime of Iceland (which should be introduced); rather it focuses on the climate variability and its link to large scale atmospheric patterns.
  Author response: The new Section 2.1 entitled "The hydrology of Iceland, does not describe the hydrology of Iceland (as stated in the response), rather its geology focusing on how the landscape was shaped and related hydrogeology characteristics in terms of baseflow contribution. I miss a section introducing the hydrology of Iceland, i.e., the hydrological regime of its rivers (i.e., the seasonal variation in mean monthly flow over the year) and its variability across Iceland. The hydrological regime is controlled by the interaction between the climate and catchment characteristics, and provides vital information about streamflow seasonality, including periods of high and low flow. This basic information is fundamental to the paper in the interpretation of the changes found as well as to the section that follows (Section 2.2).
  Highly recommended revision.

- Data: Should add how many stations contains > 10% missing values. This can be added to the Table S1 (important as it says something about the uncertainty in the trend analysis for specific stations).
  Author response: Fine but would be more useful to state the number of years missing in the two periods used to derive the trends as this is the same periods for all catchments. Further suggest repeating what period 1 and 2 represent in the heading of the table (can be given in bracket).

- Data: It is not sufficient to state a 'selection of catchment attributes' without stating what these are (if one does not want to list all, one can group these, e.g. soil parameters) and referring to where these are described in details.
  Author response: Fine but there is a need for an overview of the attributes included and their definitions (not sufficient to refer to another source). This could be given as a table in the Appendix (refer remark given earlier).
  Highly recommended revision.

- It is vital to know how the BFI was calculated. Given that it is a hydrograph separation method, it will be affected among other by the climate, e.g. catchment in wet climates (or during a wet period) will generally have a higher BFI than a similar catchment in a dry climate (or during a dry period), simply because of higher flows and the lack of longer dry periods.
  Author response: Fine

- Section 2.6: the trend method is not insensitive to outliers, rather it is less sensitive than other methods (e.g. linear regression).
  Author response: Fine

- Section 3.1.1 under Results. The first two sentences report from literature and should thus go to the Introduction.
  Author response: Fine

- Section 3.1.1 Changes in CV, flashiness and BFI are recommended to move to this heading (multi-annual variability).
  Author response: Fine

- Figure 1: Snowfall anomalies are shown; is the change in the contribution of snowfall vs rainfall quantified? Ref. statement line 259-261.
- Author response: Fine (important information now added)

- Figure 1: The text refers to low flow periods in the annual plots; this should rather be lower than normal annual flows (low flow periods mean something else, (refer hydrological regime).
  Author response: Fine. A minor revision is suggested:
  "After 2011, flows in glaciated rivers have been predominantly below the 2000-2010 mean, whereas most non-glaciated rivers have remained above ~~average~~".

- Figure 1: Enhanced glacier melt around 2010 is commented on; how is this explaining the patterns seen.
  Author response: I agree that this relates to Fig.2 as noted by the authors. From the plot in Fig.2 it is hard to identify a notable increase in annual streamflow in 2010-11 for highly glaciated catchments. For instance, Jökulsá á Dal (43) shows a light red colour these years (mentioned in the reply, but not in the revised paper), whereas the two other catchments referred to show a light blue colour, expanding over several years (> five). As Jökulsá á Dal (43) has the highest percentage of glacier, there is not a consistent change toward an increase in streamflow these years. The text should be modified to reflect this, which partly can be explained by the fact that the anomalies are derived based on the 2000-2010 mean.
  Highly recommended revision.

- Figure 2: Valuable that the authors have included the percentage of glacier in the anomaly plot. The figure shows that glaciated catchments show a consistent reduction in annual streamflow (glacier % >10), with higher anomalies for the highest glaciated catchments. How does this agree with the trend results seen in S5 (albeit different periods)?
  Author response: Looking at Fig.2 it is not obvious that the long-term trend is positive for glaciated catchments (focusing on those with > 10%). Anomalies are for these catchments largely negative (red) except for a period around 2000-2010. The trend found can be explained by streamflow being lower than the 2000-2010 mean in 1973 (at start of the period), than at the end (2023). Choosing the 2000-2010 period as the mean, being a period with rather high flow, influences the pattern seen. It is recommended to better explain this to the reader.
  Highly recommended revision.

- Looking at causal factors (link large scale atmospheric patterns) for the annual variability is not introduced as an aim for the study, and part of the text in Section 3.1.3 relates to Data and Method and should be moved accordingly.

Author response: Fine; however, in the Discussion (Section 4.2.1) a potentially shift in cyclone trajectories is discussed. How does this agree with NAO/AO patterns?

- Section 3.1.3: What is the common factor for catchments showing high correlations?
  Author response: Fine

- Section 3.2: here it is referred to 107 catchments, but these are not included in the results (see earlier comment). Further, information about the data used are repeated.
  Author response: Fine

- Figure 6: Are these trends calculated based on the grid averages for the whole of Iceland?
  Author response: Fine

- Figure 6: Make sure to focus the presentation/discussion on significant trends, emphasising that the warming signal is more pronounced in the longer period.
  Author response: Fine

- Figure 8: this figure nicely summarises the trends in streamflow; it would be valuable to see a similar figure for temperature and precipitation as it is often stated that there is a higher number of positive/negative trends without quantifying this.
  Author response: Fine

- Line 389; why would positive trends in spring streamflow (i.e., increasing flow) correlate negatively with temperature and total ET, implying that higher Q is linked to lower temperature and lower ET? It is suggested by the authors that this is linked to higher evaporative demand causing reduced flow, however Q is increasing in this case. Please clarify.
  Author response: Fine

- Higher temperature may both lead to an increase (snow/glacier melt) and a decrease in streamflow (higher ET). The discussion should better balance this.
  Author response: Fine. A minor revision in Section 4.2.2.1 is suggested:
  "Winter flows also strengthened at several sites" suggest changing 'strengthened' to 'increased' if this is what is meant.

- Figure 9: it is not commented on which of these trends are significant.
  Author response: Fine

- Changes in some flow indices show a positive relation to BFI (e.g. summer streamflow), some a negative (e.g. spring streamflow); is this well explained?
- Author response: Fine (valuable addition).

- Figure 10: nice to include also the trend in peak streamflow, albeit the results are very diverse across regions.
  Missing response

- You may consider reducing the use of ‘in this study’ in the text.
  Author response: Fine

- The correlation heatmaps in the Supplement lack reference to significance.
  Author response: Fine

- The correlation between the variables should preferable be shown in a matrix, and perhaps the many heatmaps could be summarised in a correlation matrix (or two) as well.
  Author response: Fine

Additional remarks

- The Conclusion should reflect on the aims identified.
- Would it not be better to use ‘changes’ rather than ‘tendencies’?
- Make sure to introduce period 1 and 2.
- The use of the term ‘strengthening’ reads odd at places, e.g. “Spring and autumn emerged as the most consistently strengthening seasons” (Section 4.2.2.2).
- Great versus large; it is recommended to use ‘large’ when referring to a physical quantity (increasing or decreasing). Note in the abstract ‘greatest’ is spelled ‘greates’; using greatest (rather than largest) reads as something positive.
- Section 2.6 Calculation of trends: you may consider adding a separate section introducing the indices included in the analysis (e.g., flashiness, BFI), rather than placing it under the current heading.
- Looking careful at Fig. 1 it appears that the shift in rainfall versus snow happens earlier than 2000, rather it peaks shortly after 2000.
- Figure 1: add marks so one can identify the years on the X-axis.
- Should be Figures S14-S18, not S13-17 on p.9
- Make sure to reflect on cross-correlations between attributes when discussion the results.
- Section 3.1.2: Make sure to introduce that the anomalies are derived based on the 2000-2010 mean, and why this period was chosen.
- Suggest adding information about how ET is derived, which is a difficult variable to estimate and thus important information when considering uncertainty.
- When commenting on the shorter period (1993-2023), it is valuable to add ‘more recent’ not only shorter period.
- Section 4.2.3 and 4.2.4: Some unclear formulations, including:
  - The statement ‘directional increases’ (Section 4.2.3) is hard to grasp
  - What is meant with ‘weaker statistical support’?
  - The sentence that follows state the ‘Correlations between baseflow index trend and soil attributes’. I guess you mean ‘Correlations between baseflow index and soil attributes’, not the trend.
  - What is meant by ‘baseflow contribution streamflow trends’? (note spelling error)
  - What is meant by muted changes?

  - What is meant by downturn in summer flows?
- When referring to Blöschl et al., make sure to compare your results, i.e., do your results support or disagree with their findings?