Manuscript egusphere-2024-4186: Response to reviewer, February 1, 2026

Hörður B. Helgason, Andri Gunnarsson, Óli G. B. Sveinsson and Bart Nijssen, Understanding Changes in Iceland's Streamflow Dynamics in Response to Climate Change

We would like to sincerely thank the reviewer again for their exceptionally detailed and constructive feedback through three rounds of revisions, which has greatly improved the manuscript. We have addressed the comments in the latest review, dated Jan 1 2026.

In the following pages, the authors provide detailed, point-by-point responses to each comment.

First round of major revisions:

- Reviewer comments (*italics*)
- Author responses (normal font)

Second round of major revisions:

- Reviewer comments (blue)
- Author responses (red).

Third round, minor revisions:

- Reviewer comments (green)
- Author responses (**green, bold**)

Remaining comments (those resolved in the first and second round of revisions have been removed):

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.

We have added this text to Data:
"We incorporate static catchment attributes from the LamaH-Ice dataset, which provide information on topography, climate, and hydrology, as well as land cover, vegetation, soils, geology, and glaciation. In addition, we use time series that describe temporal changes in glacier-covered areas within the catchments. Further details are available in the LamaH-Ice publication (Helgason and Nijssen, 2024)."

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 thank the reviewer for this suggestion. All catchment attributes used in this study are taken directly from the LamaH-Ice dataset and are described in detail, including definitions, units, and data sources, in the associated data publication (Helgason and Nijssen, 2024). As this paper is already lengthy, we believe that reproducing a full table listing and defining all LamaH-Ice attributes would largely duplicate existing, peer-reviewed documentation without adding substantial value to the present analysis. To ensure clarity, we have explicitly stated in the Data section which categories of attributes are used and refer readers to the LamaH-Ice paper for full descriptions.

Issues that need clarification/discussion

- 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.

Thank you for pointing this out. We agree that the 2024 event should not appear as an isolated remark in the Discussion. In fact, to shorten the already lengthy paper, we have removed the discussion of the 2024 year. Since meteorological data for the 2024–2025 water year are now available, we have extended Figure 1 to include this period and updated both the Results and Discussion sections accordingly. To address your comment, we now state in the Results section:

"Although these two years are excluded from the trend analysis in later sections due to incomplete streamflow records, their contrasting hydroclimatic conditions provide a contemporary example of the variability discussed here."

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

We agree and have replaced 'contemporary example' with 'recent example'.

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).

In Sect. 3.2 we have added the following sentence: Note that meteorological forcings are calculated as catchment averages and may smooth over locally diverging trends within large catchments.

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.

We have revised Sect. 3.2 and Sect. 3.3 to explicitly incorporate catchment area when interpreting spatial variability in meteorological and streamflow trends. We now note that catchment areas span several orders of magnitude, implying that large basins integrate heterogeneous upstream signals, whereas smaller basins reflect more localized forcing.

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.

To address this comment, we have added the following sentence to Sect. 2.3: "As a result, trend estimates that rely heavily on winter flows or on early parts of the record (e.g. the 1970s and 1980s) should be interpreted with greater caution than those based on recent summer flows."

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).

Thank you for this clarification. We have revised the text to improve clarity.

- 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)?

We removed such years entirely from the time series rather than keeping them as missing values (e.g. NaNs or placeholders). Thus, the trend and corresponding p-value were computed only on the remaining valid years, without any gaps or empty timestamps influencing the calculation.

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.

We have revised Sect. 2.3.1 to explicitly state that years exceeding the missing-data threshold are removed entirely from the time series prior to trend estimation. We also now refer to Table S2, which reports the percentage of excluded years for each gauge and analysis period, allowing readers to assess the potential impact on trend estimates and statistical significance.

Reviewer comments – second round, submitted 19 September 2025

General comments

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.

We thank the reviewer for this observation. We agree that Section 3.1.1 contains visual long-term trendlines, which could be confused with the formal statistical trend analysis introduced in Section 3.2. To clarify the structure, we have changed the subsection title to:

"3.1.1 Multiannual variability and long-term tendencies in temperature and precipitation."

We also emphasize in the text that the trendlines in Figure 1 are included only as "a visual representation of overall long-term changes, while the trend analysis of streamflow and meteorological variables in sections 3.2 and 3.3 is based on different time periods."

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).

We have replaced 'tendencies' with 'changes' in the title for Section 3.1.1.

Specific comments

- 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.

We thank the reviewer for this observation regarding Section 2.2. In response, we have renamed Section 2.1 to 'The hydrology of Iceland', as it describes the hydrological regime of the country. Section 2.2 is now titled 'Climate variability and link to large-scale atmospheric patterns' to more accurately reflect its focus on climatic controls. We believe these revised subsection titles more clearly represent the content of each section.

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.

We have revised Sect. 2.1 to explicitly describe the hydrological regime of Icelandic rivers, including seasonal flow patterns and their spatial variability, to better support the interpretation of the results presented later in the manuscript.

- 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).

We have added this to Table S1.

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).

We agree that it is important to clearly communicate the number of missing years within each analysis period. This information is already reported in Table S2 as the percentage of excluded years for period 1 (1973–2023) and period 2 (1993–2023). To improve clarity, we have revised the table header to explicitly state the definitions of period 1 and period 2 and to clarify that the reported values represent the fraction of years excluded from the trend analysis.

- 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.

We have now explicitly stated in the text which catchment attributes are reported in table S1. Further, we have added this text to address your comment: “We incorporate static catchment attributes from the LamaH-Ice dataset, which provide information on topography, climate, and hydrology, as well as land cover, vegetation, soils, geology, and glaciation. In addition, we use time series that describe temporal changes in glacier-covered areas within the catchments. Further details are available in the LamaH-Ice publication (Helgason and Nijssen, 2024).”

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.

We chose not to follow this suggestion, as the full overview and definitions of the catchment attributes are already provided in the LamaH-Ice data description paper (Helgason and Nijssen, 2024); see our response to the first comment (page 2).

- 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).

Presumably this refers to figure 2. We have fixed this in the text.

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~~".

We have fixed this in the text.

- Figure 1: Enhanced glacier melt around 2010 is commented on; how is this explaining the patterns seen.

We assume this comment refers to figure 2 as well. We have added the following text: "This is evident in the elevated flows in 2010–2011 in highly glaciated rivers such as Jökulsá á Dal, Jökulsá á Fjöllum, and Djúpá (noting that the 5-year rolling mean extends into subsequent years)."

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.

We thank the reviewer for this careful observation. We agree that Figure 2 does not show a clear, short-lived peak in annual streamflow around 2010–2011 across all highly glaciated catchments, and we have revised the text accordingly. We now emphasize that enhanced glacier melt during this period is expressed as sustained positive anomalies in some glaciated rivers rather than as a distinct spike confined to individual years, and that this signal is partly muted because the anomalies are computed relative to the 2000–2010 reference period, which already includes years of elevated melt.

In addition, Jökulsá á Dal (43) has been removed from the revised figure and discussion. This gauge was relocated in 2003 and again in 2007, after which the series is derived from calculations based on the Hálslón reservoir mass balance rather than direct streamflow measurements. These changes introduce discontinuities in the long-term mean that complicate the interpretation of anomaly-based plots. To avoid conflating hydrological signals with measurement and methodological changes, we therefore excluded this station from Figure 2 and focus the discussion on glaciated catchments with consistent observational records, such as Jökulsá á Fjöllum and Djúpá.

- 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)?

We have added the following paragraph into Sect. 4.2.3: "For glaciated rivers, our results showed positive long-term streamflow trends (1973–2023) but negative or insignificant recent (summer) trends (1993–2023). This contrast reflects a transition from a high-melt period in the early 2000s to reduced glacier runoff after 2010. As illustrated in Figure 2, exceptionally high flows around 2000 elevate the long-term trend, whereas the post-2010 decline dominates the shorter-period analysis."

We slightly revised the criteria for streamflow data availability used in Figures 2 and 3. In the previous manuscript version, monthly averages were calculated for months with at least 50% data availability, followed by computation of water-year averages. To ensure consistency with other parts of the manuscript, we now apply a uniform criterion: a given year is excluded if more than 10% of the data are missing. Additionally, in Figure 3 (AO and NAO correlations), we replaced the previous restriction to gauges starting before 1980 with a simpler requirement of at least 30 valid years of data. This adjustment allowed the inclusion of additional gauges in the correlation analysis.

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

We agree that the anomaly patterns in Figure 2 require careful interpretation given the choice of the 2000–2010 reference period, which coincides with anomalously high meltwater contributions in several glaciated catchments. We have therefore added explicit clarification in Sect. 3.1.2 noting that negative anomalies before and after this interval are common as a result of this reference choice.

In addition, in Sect. 4.2.2.1 we now explicitly state that the positive long-term trends detected for glaciated rivers over 1973–2023 are driven by the strong contrast between relatively low flows prior to 2000 and elevated flows during the early-2000s melt period, even though flows declined again after 2010 (Figure 2). Together, these additions clarify the apparent discrepancy between the anomaly patterns and the trend results.

- 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.

We agree that the analysis of AO/NAO influence on streamflow variability should be more clearly positioned as part of the study objectives. We have revised the final paragraph of the Introduction accordingly. Specifically, we now state that the study revisits previously reported correlations between Icelandic streamflow and large-scale atmospheric circulation (AO/NAO) using a broader and more recent observational dataset, and we explicitly include this as one of the three key research questions.

Further, we have moved the sentence "The AO and NAO indices were obtained from the NOAA Climate Prediction Center (NOAA CPC, 2024)." from Sect. 3.1.3 to Sect. 2.3 (Data).

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?

We have added a clarifying sentence noting that the AO and NAO influence cyclone trajectories and storm-track positioning over the North Atlantic. As stated in the manuscript, attribution of circulation changes is beyond the scope of this study; the discussion is therefore qualitative and based on the spatial coherence of the observed precipitation trends, which are known to be modulated by large-scale circulation patterns such as the AO and NAO.

- Higher temperature may both lead to an increase (snow/glacier melt) and a decrease in streamflow (higher ET). The discussion should better balance this.

We agree, see a new paragraph at the end of Sect. 4.2.2.1

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.

We have changed this in the text.

Additional remarks

- The Conclusion should reflect on the aims identified.

We have revised the Conclusions section to explicitly reflect the study aims identified in the Introduction by clearly linking our main findings to each of the three research questions.

- Would it not be better to use 'changes' rather than 'tendencies'?

We deliberately use the term "directional tendency" to distinguish coherent regional patterns in trend direction from statistically significant changes at individual sites. This terminology is defined in the Methods section and used consistently throughout the manuscript to avoid implying statistical significance where it is not present. We therefore prefer "tendency" over "change" in these cases, as the latter may be interpreted as implying a detected change. We have clarified this definition better in the methods section.

- Make sure to introduce period 1 and 2.

We have revised the Methods section to explicitly introduce and label the two analysis periods as period 1 (1973–2023) and period 2 (1993–2023) at first mention.

- 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).

We agree and have replaced the term ‘strengthening’ with more precise language in Sect. 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.

We have revised the manuscript to avoid the use of ‘great’ and related forms when referring to physical quantities, replacing them with more neutral terminology.

- 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.

We agree and have restructured the Methods section by introducing a separate section describing the hydrological indices used in the analysis. This improves clarity by separating index definitions from the trend detection methodology.

- Looking careful at Fig. 1 it appears that the shift in rainfall versus snow happens earlier than 2000, rather it peaks shortly after 2000.

Thank you for this observation. We have revised the text to clarify that the transition toward rainfall dominance emerges in the late 1990s and becomes persistent after 2000, consistent with the behavior shown in Figure 1.

- Figure 1: add marks so one can identify the years on the X-axis.

We have added yearly marks on the X-axis in Figure 1.

- Should be Figures S14-S18, not S13-17 on p.9

We have fixed this.

- Make sure to reflect on cross-correlations between attributes when discussion the results.

We have addressed this by explicitly noting in Sect. 2.6 that many catchment attributes co-vary and that bivariate correlations reflect shared physiographic gradients rather than independent effects. In addition, interpretations of soil–streamflow relationships (sect. 3.3.1) were revised to reflect coherent soil texture and storage gradients rather than individual attributes

- 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.

We have clarified in Sect. 3.1.2 that streamflow anomalies are referenced to the 2000–2010 mean and explained that this period was chosen because complete data are available for all gauges, ensuring consistency across catchments

- Suggest adding information about how ET is derived, which is a difficult variable to estimate and thus important information when considering uncertainty.

Thank you for this suggestion. We agree that evapotranspiration is a challenging variable to estimate and that its uncertainty should be acknowledged. We have therefore added a brief clarification in Sect. 2.3 noting that evapotranspiration in ERA5-Land is a model-derived diagnostic variable and generally more uncertain than the meteorological forcing variables, and that its trends should be interpreted with caution. We consider this level of detail sufficient for the scope of the present study, while avoiding an extensive discussion of land-surface model parameterizations.

- When commenting on the shorter period (1993-2023), it is valuable to add 'more recent' not only shorter period.

Thank you for the suggestion. We now consistently refer to the 1993–2023 period as the shorter, more recent period throughout the manuscript.

- Section 4.2.3 and 4.2.4: Some unclear formulations, including:
o The statement 'directional increases' (Section 4.2.3) is hard to grasp

Thank you for pointing this out. We have revised the wording in Sect. 4.2.3 to clarify that "directional" changes refer to predominantly positive but non-significant trends, consistent with the definition provided in Sect. 2.6. The revised text now explicitly states this to improve clarity for the reader.

o What is meant with 'weaker statistical support'?
We have revised the wording to explicitly state that trends in the more recent period are predominantly positive but mostly non-significant, thereby clarifying what was meant by weaker statistical support.

o 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.
We indeed refer to correlations between trends in the baseflow index and soil attributes, not between the baseflow index itself and soil properties. We have revised the wording to make this explicit and avoid ambiguity.

o What is meant by 'baseflow contribution streamflow trends'? (note spelling error)

By "baseflow contribution on streamflow trends" we refer to the relationship between catchment baseflow index and the magnitude and direction of observed streamflow trends. We have revised the wording to make this relationship explicit.

o What is meant by muted changes?

By "muted changes" we refer to smaller trend magnitudes relative to surface-fed rivers. We have revised the wording to make this explicit.

o What is meant by downturn in summer flows?

By "downturn in summer flows" we refer to the emergence of negative or near-zero summer streamflow trends during the more recent period. We have revised the wording to make this explicit.

- When referring to Blöschl et al., make sure to compare your results, i.e., do your results support or disagree with their findings?

We now explicitly compare our results with those of Blöschl et al. (2017), noting that while they reported later flood timing in parts of Iceland, we do not observe a clear trend. We also clarify that differences in study period and gauge selection likely contribute to the differing results.