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Attribution of high resolution streamflow trends in Western Austria – an approach based on climate and discharge station data

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Point-by-point replies to the comments of the editor and the referees

Contents:

Response to editor, Prof. Peter Molnar 2

Response to referee #1, Prof. Juraj Parajka 8

Response to referee #2, Dr. Marius-Victor Birsan 12

Response to referee #3........ 21

Response to referee #4........ 29

Remarks: Referee comments are presented in blue, authors comments in black. Excerpts of other publications are quoted in quotation marks. Sometimes we refer to the “first paper”, which is our earlier publication on this subject (Kormann et al., 2014). To make the review easier, we added the corrections to each answer (yellow background). For larger revisions, we refer to the corresponding section that we changed.

Response to editor, Prof. Peter Molnar

Dear Prof. Molnar,

Thank you for the invitation to revise the manuscript according to the comments of the reviewers. We considered all of the referees suggestions and made the necessary changes in the manuscript. Please find below the list of specific responses to the individual points. The marked-up manuscript version is attached to this document. Finally we would like to express our gratitude to you and all referees for the time and efforts on this manuscript.

Sincerely yours,

C. Kormann, T. Francke, M. Renner, and A. Bronstert

1/ Ref 1 states that the presented trend assessment does not provide a confirmation of the causal physical processes. He correctly advises more careful interpretations in the paper to the meaning of the results. Ref 3 asks for more details on the most important methods applied. Overall practically all of the referees found parts of the work cumbersome to read and understand (I concur with them) and suggested many improvements to the readability of the paper in their detailed reviews. Please pay very close attention to these in your revision. Don't worry about keeping the manuscript short - focus on making it understandable.

Thank you for this comment. We have addressed all suggestions about a higher detailedness of the descriptions of the methods used. Next to this, we revised the sections where we discussed our results and provided more careful interpretations. We improved the readability of the manuscript, amongst other things with a restructuring of the methods section, an additional section on the structure of the overall analysis and a schematic illustration on the different methods used in the appendix.

2/ It is also a good recommendation to prepare a new Discussion section in your paper where you engage closer with the literature that is directly relevant to your results as well (Refs 1 and 2). Do not present the results from your earlier paper Korman et al. (2014) anymore, but be very careful and explicit in specifying what is the novelty in your HESS paper. I have read the preprints and your explanation of the content and aim of this work and conclude that overlap with the HESS paper is there, but is not excessive. However duplication of the presented results in both papers is unacceptable.

We added an extra discussion section where our results are interpreted in the context of other studies (section 5). We referred especially to papers, where trends were not only detected but also trend attribution was attempted. Next to this, we interpreted and attempted to attribute also the *not significant* changes, which had not been done in the first paper.

3/ Several of the referees raised the fact that there is other existing research you may have missed, and also correct your interpretations of past studies (especially Refs 2&4). Check that throughout the paper the interpretations you ascribe to past studies are accurate. This is very important.

Thank you for this point. We revised the according sections, corrected the interpretations and added additional references where appropriate, especially in the introduction and discussion sections.

4/ I would like you to explain more the issues of trends or shifts in your analysis, and how you separate them. This is more than a terminological issue (e.g. see Dery et al., 2009), a simple shift in time in a hydrograph will inadvertently lead to (possibly very strong) rising and dropping "trends" in time. These may have nothing to do with an overall increase or decrease in runoff, and can affect your interpretation and causality analysis. Can you please provide a better explanation of this fact in your revised text where appropriate?

We discussed this point in 5.2.1:

In summer, the snow reservoir has already emptied out in most of the watersheds. The negative Q trends during this time of year are possibly part of the effects of earlier snowmelt timing on streamflow. This shift causes first rising and directly afterwards dropping streamflow trends in spring and summer, which were similarly found for watersheds in western North America by other daily resolved trend analyses (Kim and Jain, 2010, Déry et al., 2009). However, to fully attribute summertime Q decreases, it would be necessary to separate the effects of shifts in snowmelt timing from the effects of lower snow accumulation (and with this, lower snowmelt volumes). This task had been adressed in Déry et al. (2009) by a simple model approach. However, a separation of these effects based on analyses of other observed variables is difficult, as negative Q trends in summer might also have other causes such as higher infiltration, rising evapotranspiration and changing storage conditions (Berghuijs et al., 2014).

And in 5.2.4:

Our seasonal analyses support the hypotheses that we proposed in the introduction section: The subseasonal structure of streamflow trends in higher-altitude, glaciated watersheds corresponds well with the one that might stem from glacier wastage. The overall annual 30DMA trend integral over time (and thus the annual trend) is positive, as additional water in spring enters the basin (Fig. 8 a). In lower-altitude watersheds, especially summertime decreases lead to an overall negative annual trend integral (Fig. 8 c). In case the annual 30DMA trend integral over time is close to zero, the trends are caused by shifts rather than by changes of the overall streamflow amount (Déry et al., 2009). This might be the case in mid-altitude, little glaciated watersheds, where only small changes affect the annual hydrograph (Fig. 8 b).

5/ You state on several occasions that yours is a high resolution study as opposed to annual, seasonal, monthly sums. In principle I agree, since you are directly using daily data. However the

30-day moving average operator in reality goes back to monthly sums (Ref 2). How does this averaging window impact the results? In your response you state that it reduces fluctuations, I suggest you engage more with the meaning of the averaging, especially in identifying dates of change and dQ/dt terms which are dependent on it, even if you have not explicitly tested other averaging windows.

Yes, we are in fact analysing a time window of one month. If we analyse daily trends, the high variability of the daily data will result in a low detectability, which is important when considering significance tests. With 30-day averages, there are more significant trends and trend testing does not depend so much on whether the single daily time series (e.g. for 1st Jan., 2nd Jan., etc.) has a high or a low variability.
When only considering trend magnitudes, the 30-day averaging will help interpreting the trends, as the changes found are less fluctuating. The influence of single events on a specific day of year, which might cause erroneous trends, is reduced as well.
The characteristic dates are calculated also as 30-day moving averages. This provides a more consistent estimate of the CDs and ensures comparability to the trends.

We added the following paragraph to the methods section (3.3.1):

The approach of trend detection via moving averages was similarly applied in Western US by Kim and Jain (2010) and Déry et al. (2009), however, they used only 3-day and 5-day moving averages and they only analysed trends in streamflow. Contrary to that, the 30-day moving average windows reduce daily fluctuations considerably. With this, the influence of single events on a specific day of year, which might cause erroneous trends, is reduced as well. The 30DMA trends thus yield more robust trends.

And

To calculate these CDs, all datasets were first smoothed by a 30-day moving average. Through this, comparability to the 30DMA trends is ensured and a more robust estimate of the CD is obtained because of reduced fluctuations.

6/ Please explain better the estimation of the minimum detectable trend (Ref 3).

In the revised manuscript, we further explained the estimation of the minimal detectable trend (section 3.1.2):

To calculate the Δ_{MD} of a given time series, we used the matrix that is represented in Fig. 6 of Morin, 2011. This is feasible, as the minimal detectable trend does not depend on the magnitude of the data. The plot displays the change of the probability of significant trend detection versus signal-to-noise ratio (S/N) and record length (R), averaged over all previously simulated trend values. For a given time series with a given record length it is then necessary to look up the S/N that fits the red contour in the figure, i.e., the S/N at which the probability computed reaches the 0.5 threshold. This S/N is then transferred into Δ_{MD} using the following equation …

7/ To the point of attributing melt to ice/snow and the point raised by Ref 4 that in spring glacier melt cannot dominate runoff, please engaged in the revised text more with this issue.

The strongest trends (that we attributed to icemelt) at e.g. Vernagt station (station ID no. 1, mean basin altitude: 3127 m) turn up around *end of May*. For the watershed of station ID no. 8, which has a lower average altitude (2590 m), strong streamflow trends start already half a month earlier, around *mid-May*.
This goes along with other studies: In Fig. 4 of Huss (2011), monthly components of glacier storage change are presented as mean over 1908–2008 for 50 glaciers of large-scale drainage basins in the European Alps: Icemelt starts in *May*, which is similarly found in Weber et al. (2010), Fig. 6, for the Upper Danube. Both plots are based on data in monthly resolution. For Hintereisferner (a glacier in the Ötztal Alps), *daily* mass balances show decreases of the net balance starting in early May for the exceptional year 2003: http://www.ptaagmb.com/the-glaciers/europe/austria/tirol/plusplus-vernagt-ferner-star.aspx, Fig. 8 (Daily Accumulation, Ablation and Net Balance).

We agree that the *main icemelt* is happening later in the year. However, the *strongest trends* turn up earlier (the *trends* in icemelt should not be confused with the actual amount of icemelt). These *trends* are highly connected to the temperature trends, which are as well strongest during this time of year. Later in the year, streamflow trends are probably caused as well by glacier melt, however, the strongest changes are observed between May and June.

Furthermore, as we also pointed out in the manuscript, it is probably impossible to explicitly separate snow and glacier melt. So the trends caused by earlier snow melt and less precipitation falling as snow are mixing later in the season with trends caused by glacier melt.

To further clarify this issue, we added the following sections to the manuscript (section 5.2.1):

At a first glance, glacier melt in May might appear as very early in the year when looking at seasonal streamflow composition. However, one has to note that the trends in glacier melt should not be confused with the actual amount of glacier melt: The main icemelt is happening later in the year, however, the strongest trends turn up earlier. These Q trends are highly connected to rising temperatures, which are as well strongest during this time of year. The results of modelling approaches (e.g. Alaoui et al., 2014) confirm our interpretations and suggest that glacier melt starts even earlier in the year.

8/ Several of the referees raised issues about regulation effects on the streamflow data (Refs 1&2). I understand of course that regulation cannot be completely discounted, as so many rivers are to some degree regulated, but please engage with this topic more in the paper. Also connected to station choice are nested basins which are not independent (Refs 2&4) and you have explained your choice in the response.

We added the sections below to the revised manuscript (section 2). Additionally, we added the information which basins are nested to Tab. 1.

All hydroclimatic datasets were checked by Austrian government officials via extensive examinations and plausibility checks. Additionally, we checked for inhomogeneities prior to the analysis. Any station that did not meet the requirements was removed. We excluded streamflow records of catchments influenced by major hydro-electric power production. Unfortunately, it was impossible to exclude all watersheds with influences from hydro power stations, as water resources in Western Austria are used extensively: Only in Tirol, there are approximately 950 small-scale hydro power plants of differing type with a capacity lower than 10 Megawatts[1]. However, by far most of the small hydro power plants in Austria are run-of-river power plants (A. Egger (Tyrolean spokesman of the association on small hydro power plants in Austria), personal communication, July 29, 2014). These power plants do not have any pondage and thus there is no delay of river runoff. The rest of the small hydro power plants are mostly equipped with 1-day water storage volumes, which means there is a maximum delay of an average daily discharge amount, so the impacts on the seasonal discharge behaviour are very limited.

And:

Eight of the 32 catchments analysed are nested. We used the approach that was applied as well in Birsan et al. (2005): To guarantee spatial independence of the station data, we checked for a considerable increase in watershed area among the corresponding gauges. Only the station pair Innergschlöß (39 sq km) and Tauernhaus (60 sq km) did not meet the requirements as defined in Birsan et al. (2005). However, as these basins were necessary to increase the number of catchments with glacial influence and the requirements of station independence were not violated too strongly, we left them in the dataset.

In the regression analysis you use climate, snow and streamflow gauging station data. It is not clear how the assignment of climate and snow stations to each streamflow gauging station was made.

We clarified this in the methods section (3.3.2):

Based on the previous results of this study, we gathered all possible variables which then served as predictor variables (independent variables): Next to catchment properties such as mean watershed altitude, glacier (forest etc.) percentage or decrease of glaciated area, we used linear regression to transfer long-term average temperatures to the mean watershed altitudes. This means, the assignment of the average temperatures was based on regionally derived temperature lapse rates.

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[1] http://www.kleinwasserkraft.at/en/hydropower-tyrol [July 2014]

We decided to not use snow data as the assignment of snow depth to certain altitudes is highly uncertain. The $\overline{\Delta T}$ time series were 30DMA temperature trends averaged over all available stations. This was feasible, as similar trends concerning timing and magnitude occur at all stations analysed. Similar to the earlier analyses, all the datasets of hydroclimatological variables were filtered on the basis of 30-day moving averages beforehand.

9/ The hourly analysis is not well connected with the rest of the paper (Ref 4). Consider how important this part is to the message of your paper.

We changed the structure of the methods/results/discussion section, so now the hourly analysis is more aligned with the other trend attribution approaches.

Furthermore, we added the following paragraph to the discussion (section 5):

The analysis of hourly streamflow trends supports the findings of the earlier analysis and shows, that hourly resolved trend analyses can provide additional information on the changes in alpine streamflow. Most of the gauging stations with hourly measurements have only been installed since the eighties, so there has been hardly any research on the subdaily changes of streamflow and there might be potential for further research.

Response to referee #1, Prof. Juraj Parajka

General comments:

1) One of the main messages of the paper is: "...it was confirmed that the main drivers of alpine streamflow changes are increased glacial melt and earlier snow melt". Is this statement really confirmed by presented results, particularly for earlier snow melt? I would say that the results (trend assessment and attribution) indicate this, but not confirm. Why are the significant changes observed only in a few catchments? Why in some very close basins do different trends (significant/not significant) occur? What is the role of other physiographic catchment (storage, vegetation, land use) properties? There are many unanswered questions and simple trend assessment does not allow to confirm causal physical processes, so more careful interpretations would be needed here. In addition, a definition of research hypotheses is based on only 9 (be precise in the statements) stations with statistically significant (and not consistent) runoff changes (out of 32 stations), which needs to be considered and reflected in statements based.

Thanks for this comment. We agree, we can definitely not provide a full confirmation of the causal physical processes but we found certain arguments that support our hypotheses. Not only the methodology but also the limited data availability restricts our conclusions and leaves many unanswered questions. Hence, we changed the wording all over the manuscript to more careful interpretations.
For example:
- we changed the word “confirm” with the word “support”, as “confirm” is probably too strong when talking about hypotheses;
- we changed the wording “we attributed” to “we attempted to attribute” and so on.

2) The statement (on p.6883) that there is not much literature on hydrological changes is not precise. There is (at least) a number of relevant studies focusing and summarizing trend assessment studies, seasonality analyses and climate change effect assessments published in recent years and covering the Alps or Austria. Below are some reference suggestions which might be considered and added to the story (Introduction and Discussion sections).

We thank you for this comment and the literature suggestions, which we considered in the revised manuscript. We agree, the sentence is not precise. We intended to point out that there are not many detailed regional trend assessments but mostly trend studies that cover whole countries/continents or the Greater Alpine Region as a whole. However, we reflected this statement and we removed it in the revised version of the manuscript, as it is probably impossible to verify.

3) Using terms "high-altitude" and "low altitude" stations is confusing as the low altitude basins have the mean elevation almost 1500 m a.s.l.. Such elevation would not be considered as low altitude basin in many regions of the world.

We agree, that this might be confusing. Maybe the term “lower-altitude” (“higher-altitude”) instead of “low-altitude” (“high-altitude”) stations would be more appropriate. We changed it accordingly all over the manuscript.

I would suggest to use some more clear stratification of the basins, i.e. according to glacier proportion, but generally refer to them as to alpine basins.

A clear stratification of the basins (if you mean this in the sense of a structuring of the order) is difficult, as we somehow had to sort or structure them. Glacier proportion is problematic as well: Maybe one catchment has a high glacier proportion, but a lower mean altitude (e.g. basin no. 17). Another catchment with a high glacier proportion has a higher mean altitude (e.g. basin no. 4), which generally means different soils, vegetation, hydrological properties etc. Furthermore the question arises, how to structure catchments with no glacier proportion.
We were looking as well for an appropriate structuring, but mean watershed altitude is in our opinion the easiest one to understand and indirectly includes most of the catchment attributes (e.g. with increasing mean basin altitude, forest proportion, vegetation cover, soil thickness etc. is generally decreasing whereas rock proportion, glacier proportion etc. is generally increasing).

4) Discussion of results is, in my opinion, an important part of the assessment, but is missing. Please add (i.e. revise the Summary) a separate Discussion section, which will discuss and relate the findings and implications found in this work with existing literature.

Thanks for the comment, we added a separate discussion section (p19 – p25).

5) It would be interesting to see a real discharge data and its changes (instead of or in addition to schematic representations in Figures 8 and 9). How are the significant runoff trends represented/translated in measured streamflow hydrographs?

Thanks for the suggestion, we plotted real hydrographs instead of the schematic illustration (Fig. 8).

Specific comments:

p.6886: " a relatively dry region in the rain shadow". Please consider to add a range of mean annual precipitation in the study region, otherwise it might be confusing.

Thanks for this suggestion, we considered it in the revised manuscript: "With 970 ± 290 mm average precipitation amount per year (based on station data), this is a relatively dry region in the Alps as it is situated in the rain shadow of the northern and southern Alpine border ranges".

p.6887: " so we assume that the impacts on the seasonal discharge behavior are very limited as well". What are the effects on daily and sub-daily discharge fluctuations? How are the ice effects on discharge measurements in winter accounted?

Reviewer Dr. Birsan, raised similar concerns. The streamflow datasets were carefully checked beforehand on whether there was any influence of hydropower on the discharge quantities. Additionally we checked for inhomogeneities in all hydro-climatic datasets. Any station that did not meet the requirements was removed.

However, minor influences cannot be excluded due to the sheer amount of small hydro power plants (e.g. ~950 only in Tyrol). According to DI Mag. Egger, who is Tyrolean spokesman of the association on small hydro power plants in Austria (www.kleinwasserkraft.at), by far most of the small hydro power plants in Austria are run-of-river power plants (Egger, personal communication). These power plants do not have any pondage and thus there is no delay of river runoff. The rest of the small hydro power plants are mostly equipped with 1-day water storage volumes, which means there is a maximum delay of an average daily discharge amount. The three gauges, where subdaily (hourly) trends were analysed, have no influence of these type of power plants (Egger, personal communication).

To double check, we analysed one station with influence of hydropower (Schalklbach, 982 m a.s.l.; lon.: 10 29 24; lat.: 46 56 17; basin size: 107 km²): The seasonal trends look completely different to the ones of near-natural catchments with no plausible explanation except anthropogenic influences.

So there might be small hydro power stations in the watersheds analysed, but their influence on absolute discharge quantities is negligible. We clarified this and rewrote the according section in the revised version of the manuscript (p7, 29 – p8, 11).

Concerning the ice effects on discharge measurements in winter, we cannot assure that these are completely negligible. In this case, we have to rely on the Austrian Hydrographic Service: According to them, extensive examinations and plausibility checks are performed before distributing the data (http://www.hydro.tuwien.ac.at/uploads/media/mueller_05.pdf, unfortunately only in german).

p.6894: " earlier snowmelt and less precipitation falling as snow. This in turn leads to multiple hydrological changes such as higher evapotranspiration, higher infiltration or changing storage characteristics ..." It is not clear (not visible from presented results) how is earlier snowmelt causing higher evapotranspiration or higher infiltration. Please consider to provide more details/reasoning for this hypothesis. Kormann et al. (2014) is not freely available. Difficult to justify the interpretations made (by referring to that paper) and ...

We thank you for this comment. On the page that you pointed out, we only defined the research hypotheses. In the analyses that follow, we tried to support our hypotheses. However, concerning the summertime streamflow decreases (which are effects of the processes you mentioned above), we were not able to support our interpretations with analyses of other variables.

Nevertheless, (as we also pointed out in the conclusions p27, 1 – 8) there is a shift of snowmelt to earlier DOYs and a higher rain/snow ratio. With these changes, the watershed potentially receives more precipitation in the form of rain which in turn leads to higher annual infiltration and interception rates (During spring snowmelt, the soil is generally saturated fast and is not able to hold the excess water in the watershed. With climate change, the season where water is bound to snow is shortened). This water is then additionally available for evapotranspiration and vegetation growth and thus will reduce seasonal – and with this annual – streamflow amounts. The study of Berghuijs et al. (2014) supports this assumption for the contiguous US: they found observational evidence, that a reduction in the percentage of snow in total precipitation goes along with decreases in average streamflow.

…also to recognize what are the differences between this study and the manuscript.

The other referees have pointed out this issue as well. We have answered to this in a separate comment.

References:

Berghuijs, W. R., R. A. Woods, and M. Hrachowitz (2014), A precipitation shift from snow towards rain leads to a decrease in streamflow, 775 Nat. Clim. Change, 4, 583–586, doi:10.1038/nclimate2246.

Kormann, C., Francke, T., and Bronstert, A.: Detection of regional climate change effects on alpine hydrology by daily resolution trend analysis in Tyrol, Austria, J. Water Clim. Change, in press, 2014.

Response to referee #2, Dr. Marius-Victor Birsan

General Comments:

Point 1

A major problem is that the manuscript is overlapping with another paper written by three of the authors: Kormann C, Francke T, Bronstert A (2014) Detection of regional climate change effects on alpine hydrology by daily resolution trend analysis in Tyrol, Austria, J Water Clim Change (in press). Some results are simply duplicated: that paper deals with the very same region, some methods are identical, e.g., Mann-Kendall test, Sen's slope, 30-day moving average (30DMA), and the data series are quite the same (except that, in that paper, longer intervals were also considered); the effect of altitude on trend timing and magnitude is also discussed; some figures are similar, too. This affects the originality of the present manuscript (even if the authors write that one manuscript is only limited to trend "interpretation", while this one deals with trend "attribution").

Due to the importance of this point, we addressed this issue already in a separate comment on HESSD: http://www.hydrol-earth-syst-sci-discuss.net/11/C2850/2014/hessd-11-C2850-2014- supplement.pdf

Point 2

The introduction lacks a proper literature review on streamflow trends in the region, and contains some statements that are misleading or false. I think this part has to be rewritten.

Thanks, we considered this point and rewrote the introduction section. Further information is found in the specific comments section below.

Point 3:

The streamflow data in particular have to be better described. Are the data series from independent basins? Is there any nested basin?

We added the following paragraph:

Eight of the 32 catchments analysed are nested. We used the approach that was applied as well in Birsan et al. (2005): To guarantee spatial independence of the station data, we checked for a considerable increase in watershed area among the corresponding gauges. Only the station pair Innergschlöß (39 sq km) and Tauernhaus (60 sq km) did not meet the requirements as defined in Birsan et al. (2005). However, as these basins were necessary to increase the number of catchments with glacial influence and the requirements of station independence were not violated too strongly, we left them in the dataset.

A detailed map containing the river network...

We improved Figure 1 as it was proposed by the referee and included it into the revised manuscript.

...and the dams and water withdrawals is necessary.

The discharge stations were carefully checked beforehand on whether there was any influence of hydro power on the discharge quantities (Each gauge, where discharge quantities are influenced by hydro power, is marked by Austrian government authorities. See http://ehyd.gv.at/). Additionally we checked for inhomogeneities in the datasets (see next point). Any station that did not meet these requirements was removed.
However, minor influences cannot be excluded due to the sheer amount of small hydro power plants (e.g. ~950 only in Tyrol; to compare: ~1000 in Switzerland). According to DI Mag. Egger, who is Tyrolean spokesman of the association on small hydro power plants in Austria (www.kleinwasserkraft.at), by far most of the small hydro power plants in Austria are run-of-river power plants. These power plants do not have any pondage and thus there is no delay of river runoff. This also reflects the position of Mag. Niedertscheider (Tyrolean Government, Department of Hydrography und Hydrology, personal communication).
The rest of the small hydro power plants are mostly equipped with 1-day water storage volumes, which means there is a maximum delay of an average daily discharge amount (the three gauges, where subdaily (hourly) trends were analysed, have no influence of these type of power plants (Egger, personal communication)).
To double check, we analysed one station *with* influence of hydro power (Schalklbach, 982 m a.s.l.; lon.: 10 29 24; lat.: 46 56 17; basin size: 107 km²): The seasonal trends look completely different to the ones of (near-)natural catchments with no plausible explanation except anthropogenic influences.
So there might be small hydro power stations in the watersheds analysed, but their influence on absolute discharge quantities is negligible. We clarified this in the according section (p7, 29 – p8, 10) in the revised version of the manuscript.

A homogeneity test is recommendable in order to check for eventual anthropogenic influence on such small basins.

We got the data from the Austrian Hydrographic Service, so the station data was already checked by Austrian government officials via extensive examinations and plausibility checks (http://www.hydro.tuwien.ac.at/uploads/media/mueller_05.pdf). We additionally checked for homogeneity of the stations beforehand via double sum analyses. In the case of inhomogeneities, the corresponding data was excluded. We added this information in the revised version of the manuscript (p7, 29 – p8, 1).

4) Finally, I think a paper dealing with trend attribution should have an in-depth, standalone Discussions section.

Thanks for the comment, we added a separate discussion section (p19 – p25).

Specific comments:

Slide 6883, lines 4-8: You write that temperature increase "is at least twice as strong in mountainous areas compared to the global average (Brunetti et al., 2009)". The statement in Brunetti et al. (2009) does not refer to the global average, but to the lower-elevated areas within the (same) HISTALP dataset. On line 8, I suggest to replace "." with ";"

We partly disagree as we understood this study different. In the following is a citation of Brunetti et al., 2009:
- *"The analyses highlighted an average GAR warming of about 1.3 K per century over the common period covered by all the variables (1886–2005). Such a warming turns out to be slightly stronger (1.4 K per century) over the 1906–2005 period (reference period of the IPCC AR4) and it results in about twice as large as the global trend referred to by IPCC (2007)."*
In our opinion, these statements refer to the Greater Alpine Region as such, compared to the global average. Please correct us if we understood this wrong.

We changed the punctuation mark as you proposed.

Slide 6883, lines 12-13: Your statement "Although the credibility of observations is far stronger than that of the model results, only a few studies analyse trends in historical data." is simply not true. There are plenty of studies on with streamflow trends. See for example Stahl et al. (2010) for a comprehensive review on streamflow trend studies in Europe until 2010. There are many others after 2010 as well. For a global view, see Dai et al. (2009). For other hypotheses on hydrologic responses to climate change, see Jones (2011).

We agree, this should have meant "..fewer studies (remark: compared to modelling studies) analyse trends in historical data". We intended to refer to detailed regional studies. Indeed, there are many studies that analyse trends in Europe and in the Greater Alpine Region, but fewer studies look detailed at regional trends. However, we reflected this statement and we removed it in the revised version of the manuscript, as it is probably impossible to verify. Additionally, we added the references mentioned (Jones (2011) in the conclusions).

Slide 6883, lines 17-18: You write: "A lot of trend studies in Central Europe did not find significant changes in the water cycle (cf. Pekarova et al., 2006), which has also been reported about trend studies in alpine regions (Viviroli et al., 2011)." The phrase is misleading. Neither Pekarova et al. (2006), nor Viviroli et al. (2011) reported that. The paper of Pekarova et al. (2006) refers to 18 large rivers (10'000 to 1'380'000 km2) in Europe, out of which 11 are in Central and Western Europe. The paper was published in 2006, before the vast majority of papers on streamflow trends in several European countries came out.

Thanks for this comment, we removed the reference to Pekarova et al. (2006). and corrected the one to Viviroli et al. (2011):
Viviroli et al., 2011 note in their review paper on climate change and mountain water ressources, that trend studies in alpine regions often report "*inconclusive or misleading findings*".

Slide 6883, lines 24-26: I think you are too harsh when claiming that studies based on indicators like centre of volume or annual peak flow day "should be revised".

We changed it accordingly in the manuscript: "The application of these measures is problematic"

Slide 6884, lines 25-27: You write "trends used for correlation analyses were mainly derived from annual or seasonal (3-monthly) totals (e.g. Birsan et al., 2005)". In Birsan et al. (2005), minimum, maximum and all deciles (i.e., 10th. 20th ... 90th percentiles) of the mean daily streamflow were involved in the correlation analysis, on a seasonal basis. Please rephrase (or remove the reference).

We thank the reviewer, this was corrected.

Slide 6885, lines 14-17: You write that the objectives of the study are: "(1) to explain the spatially incoherent streamflow trends in Alpine regions based on annual sums; (2) to find drivers of streamflow trends in these areas, and finally (3) to attribute the streamflow trends in the study region with a high level of credibility." Why do you think the streamflow trends in Alpine regions in general are incoherent? I suggest rewriting the objectives of the paper, highlighting the value of the study, and clearly pointing out the differences between this manuscript and Kormann et al., 2014 (in press). The order of the objectives seems a bit strange, too: the 1st and 2nd objectives refer to interpretation of streamflow trends in Alpine regions in general, while the 3rd refers to the study area in particular; the 2nd objective seems a generalization of the 3rd. To me, the main purpose of the paper is to explain (physically-wise), the streamflow changes in Western Austria.

As referee #4 had also concerns about the objectives of the study, we rewrote the according section: The present study combines the benefits of a temporally highly resolved trend analysis that is applicable to all different alpine runoff regimes with new approaches to physically-wise explain seasonal streamflow changes in Western Austria. We aim to extend the knowledge about regional trend causes, with the attempt to provide a holistic picture of the changes found under different alpine streamflow conditions.

Slide 6885, line 18: I think it is Kormann et al., 2014 instead of 2013.

Thanks, this was corrected.

Slide 6885, lines 24-26: You write that Kormann et al. stated that "the timing of daily trends (i.e. the day of year when a trend turns up) potentially is a more robust measure than trend magnitude". Measure of what? Do you mean it could be a better indicator of change? The expressions "stated" and "potentially is" do not fit well together. A statement refers to a clear and sure affirmation. Maybe you could change "stated" with "concluded" or some other verb.

We agree. This sentence was changed anyway, as another referee suggested this:
In addition, the timing of daily trends (i.e. the day of year when a trend turns up) reveals supplementary information on potential drivers of streamflow trends (Kormann et al., 2014).

Slide 6886, lines 26-27 "In the present study, we assume that precipitation has no trend." This is not really an assumption, since you already did a trend analysis of precipitation in Kormann et al (2014) and found no significant trends.

Thanks for this comment. We rewrote this paragraph for better understanding (p7, 11-16):

In Kormann et al. (2014), precipitation trends were studied as well. However, no clear and coherent significant change patterns could be identified in this study (similar to e.g. Pellicciotti et al. (2010) or Schimon et al. (2011)). Precipitation changes might exist, but cannot be detected which is due to methodological limitations stemming from a low signal-to-noise ratio.

Slide 6886, lines 5-6: You should provide a more detailed description of the region of study and its particularities, rather than referring to a paper from a low-level (closed-access) journal. Please indicate the exact elevation range.

Thanks for the suggestion. We added an extra section with further detailed information on the catchments used (p6, 2-13).

Slide 6886, line 9: Are there any nested basins?

We answered to this point already above (general comments, #3).

Slide 6886, lines 24-25: You write: "snow height changes have a much stronger effect on streamflow than those of snowfall". Please clarify. I guess you refer to the decreases in snow height in particular, as they translate into snowmelt.

Thanks for the comment, we removed the sentence.

Slide 6887, lines 14-15: You write that "the present analysis was carried out for the period 1980 to 2010". However, a 31-year period is close to the limits of acceptability for a streamflow trend analysis. Salas (1993) even recommends at least 40 years of data records. Longer intervals should also be considered – especially when concerned about streamflow attribution –, even if the number of gauging stations is small. As far as I noticed, there are at least 10 stations with records from 1950, according to Kormann et al. (2014). Also, runoff records might contain large scale periodic behaviour (e.g., Pekarova et al., 2003), and trend analyses should always be conducted on periods that span full cycles of this process if it exists.

We agree with the referee and it is true that we have longer (but a lot fewer) datasets to analyse. In the same section we provided reasons for this selection. However, we rewrote the argumentation for more conciseness:

We selected the period 1980-2010 for the data analysis. This ensured consistent data length for all hydro-climatic varaibles and best data availability. In this period, the Greater Alpine Region experienced a strong increase in air temperature by about 1.3 °C, compared to about 0.7 °C between 1900 and 1980 (Auer et al., 2007). Furthermore, the magnitudes of streamflow, temperature, snow depth and snowfall trends is strongest for this period within the study region (Kormann et al., 2014).

Finally, there are many publications that analyse trends of only 30 years or shorter.

The point that there could be a large scale periodic behaviour in streamflow data is definitely true and might be present in the trends derived. However, it is probable that these large-scale oscillations affect mostly large rivers such as the ones analysed in Pekarova et al. (2003) (Danube, Amazon, Mississippi etc.). In small rivers like the ones in our study region, these oscillations are usually masked by the effects of mostly small scale weather patterns (amongst other factors), as streamflow is not that strongly attenuated like in large river systems.

Slide 6887, lines 20-21: You should relate the storage capacity of smaller dams to the basin area. The fact that the storage volume of a small dam "is very limited compared to that of large dams" is quite obvious, but that does not necessarily imply "that the impacts on the seasonal discharge behaviour are very limited as well". There are indeed a lot of small hydro power plants in the region. I suggest (at least) adding a column Table 1 with the total storage volume of upstream dams. I think this is extremely important since 20 out of 32 basins have a drainage area between 9 and 100 km2.

Thanks for the comment. We have answered to this point already above (general comment #3).

Slide 6888, line 8; Slide 6889, line 6; Slide 6909, line 6: Helsel (not Hensel).

Thanks, we corrected that.

Slide 6890, Section 3.2.1: What is the rationale for choosing a 30-day interval as moving average? That way you are in fact analysing monthly values, centered on each day of the year, i.e., 365 times for each station. Please cite Kim and Jain (2010) who used a similar approach, but with a 3-day moving average.

Yes, we are in fact analysing a time window of one month. If we analyse daily trends, the high variability of the daily data will result in a low detectability, which is important when considering significance tests. With 30-day averages, there are more significant trends and trend testing does not depend so much on whether the single daily time series (e.g. for 1st Jan., 2nd Jan., etc.) has a high or a low variability.
When only considering trend magnitudes, the 30-day averaging will help interpreting the trends, as the changes found are less fluctuating. The influence of single events on a specific day of year,

which might cause erroneous trends, is reduced as well.
The characteristic dates are calculated also as 30-day moving averages. This provides a more consistent estimate of the CDs and ensures comparability to the trends.

We added the following paragraph to the methods section (3.3.1):

The approach of trend detection via moving averages was similarly applied in Western US by Kim and Jain (2010) and Déry et al. (2009), however, they used only 3-day and 5-day moving averages and they only analysed trends in streamflow. Contrary to that, the 30-day moving average windows reduce daily fluctuations considerably. With this, the influence of single events on a specific day of year, which might cause erroneous trends, is reduced as well. The 30DMA trends thus yield more robust trends.

And

To calculate these CDs, all datasets were first smoothed by a 30-day moving average. Through this, comparability to the 30DMA trends is ensured and a more robust estimate of the CD is obtained because of reduced fluctuations.

Tables and figures :

Table 1. In the caption, replace "watersheds" with "gauging stations".

Thanks, we changed the caption to “List of the gauging stations used in this study (sorted by mean watershed altitude) and their characteristics.”

Table 2. I suggest showing plots, rather than show correlation coefficients
– see Figure 2.1 from Helsel and Hirsch (1992), available at (page 18):
http://pubs.usgs.gov/twri/twri4a3/pdf/twri4a3-new.pdf.

Thanks for this comment. We thought about it but in our opinion it is not absolutely necessary and it would blow up the manuscript too much: Twelve more (sub-)plots are needed. Furthermore, the reader might already guess the corresponding plots from Fig. 2: Here, the trends were plotted against the *rank of station altitude*, and not *station altitude* as such.

Figure 1 should be redone. Please make a clear map with the river basins, the river network, and also including the main anthropogenic interventions (hydro power plants, water withdrawals, etc. There is no need for a km bar if Lat / Lon coordinates are present. Please make use of colors.

We added river basins and the river network, removed the km bar and used colours. In the general comments section, we have responded on the point of anthropogenic interventions.

Figure 2. Please clarify in the caption what "limits of minimal detectable trends" means.

Thanks for this comment, we included the symbol in the legend.

Figures 3, 5 and 7. The "z axis" mentioned in the figure legend does not exist (these are 2D pots). Please just refer to colour legend only.

Thanks, we changed it accordingly.

Figure 8. I suggest removing the word seasonal from the caption ("original seasonal hydrograph").

Thanks, we changed it to “Long-term annual streamflow cycle”.

Is the earlier snowmelt the only cause of streamflow increase in March to mid-April? Isn’t there also an increase in the rain/snow ratio? The figure seems to belong to a very small catchment, looking at the minimum and maximum streamflow. Also, the two volumes are not the same.
Figures 8 and 9 could be merged. It is not clear to me why you didn’t plot the REAL hydrographs – for a handful of basins, at different elevations or with different glacier coverage.

Thanks for the suggestion, we plotted real hydrographs instead of the schematic illustration (Fig. 8). Furthermore, we considered the increase in rain/snow ratio as well.

References:

Abermann, J., Lambrecht, A., Fischer, A., and Kuhn, M., 2009. Quantifying changes and trends in glacier area and volume in the Austrian Ötztal Alps (1969–1997–2006), The Cryosphere, 3, 205–215, doi:10.5194/tc-3-205-2009.

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Viviroli, D., Archer, D.R., Buytaert, W., Fowler, H.J., Greenwood, G.B., Hamlet, A.F., Huang, Y., Koboltschnig, G., Litaor, I., López-Moreno, J.I., Lorentz, S., Schädler, B., Schreier, H., Schwaiger, K., Vuille, M., Woods, R., 2011. Climate Change and Mountain Water Resources: Overview and Recommendations for Research, Management and Policy. Hydrol. Earth Syst. Sci. 15, 471–504. doi:10.5194/hess-15-471-2011.

Response to referee #3

General comments:

Besides a number of specific and technical comments, I have the following major critisisms. The presentation of the methods and result is rather "dense", and there is overlap with another paper (Kormann et al., in press):

(1) Explanations do not suffice to understand the methods and one could not redo this analysis without reading a number of other papers. I understand that the paper would get very long if all the methods would be given in detail, but I feel that more information on the methods should be given. I have made a few proposals where I feel that additional information would be very good.

Thanks for this comment. We considered each one of the referees proposals in the revised manuscript.

(2) To understand the results, the reader must pay close attention not to get lost. The paper is not an easy read. I wonder if the authors could facilitate reading this paper by adding more explanations and guiding the reader more smoothly through the material.

Thanks for the comment. We improved the manuscript as the referee recommended (e.g. with a schematic illustration on the different methods used (Appendix A.1) and further explanations of the overall structure (p8, 14 – 25)).

(3) There seems to be quite some overlap with another paper (Kormann et al., in press) from the first author, dealing more or less with the same data/region. In several instances the reader is referred to the other paper (which is not yet available), so understanding is sometimes difficult. Further, the question arises how novel the hessd paper is. I cannot answer this question since I do not know the other Kormann paper. The hessd paper should be written in a way that it is understandable on its own and that its contribution is very clear.

We answered to this point already in a separate comment (http://www.hydrol-earth-syst-sci-discuss .net/11/C2850/2014/hessd-11-C2850-2014- supplement.pdf).

Specific comments:

p6883-24: Are these metrics (centre of volume, day of occurrence of the annual peak flow) more sensitive than, for example, streamflow volume, quantiles etc.? If yes, please provide an explanation.

We clarified this: “… trends of indicators like ‘centre of volume’ or ‘day of occurrence of the annual peak flow', which serve as proxys to indicate consequences of global warming on alpine streamflow (e.g. earlier snowmelt)."

p6886-Data: The temperature and snow height stations used in the paper are never shown. I propose to add these stations to Fig. 1 or add another figure showing them.

Thanks, we included the T and SH stations in Fig. 1 in the revised manuscript.

p6886-17: "... The number of stations is a trade-off between a large number of stations that cannot be interpreted in a detailed way and an insufficient number of stations that cannot be rated as representative...". This sentence may be true, but what is the purpose of this statement? Does this mean that you have selected only a part of the available streamflow (temperature, snow height) stations? If yes, please give more information on which basis you have done the selection. How have you determined which sub-set of stations is representative?

Thanks for the comment, we removed the paragraph as it might be not necessary. We intended to perform a regional trend analysis, for this purpose we selected the stations. This is contrary to the majority of studies, which analyse trends for a larger area (e.g. nation-wide, european-wide, US-wide). But with more stations, it is more challenging to interpret the results thoroughly, as different hydroclimatological conditions could potentially mask and thus complicate finding clear and coherent trend patterns. Moreover, most trend studies only describe and interpret the spatial variability of the 3-monthly or annual trends, which is a too coarse solution in our opinion. We think, these approaches are responsible for the fact that many trend studies, even in mountain regions (where climate change signals should be stronger), often reveal "inconclusive or misleading findings" (Viviroli et al., 2011).
The aim of the overall project was to look at streamflow changes in North Tyrol, which primarily determined our selection. However, as we finally had to exclude many discharge gauges as they were influenced by hydro power, there were (in our opinion) not enough datasets to provide representative statements. For this reason, we included further gauges in the surrounding area within an additional range of approx. 40 km.

p6886-23: The decision not to study precipitation trends needs a clearer explanation. There seem to be 3 justifications: (1) "... precipitation did not reveal any clear trend patterns ...", (2) "... snow height changes have a much stronger effect on streamflow than those of snowfall ...", (3) "... we assume that precipitation has no trend. The validity of this assumption is supported by the fact that precipitation changes are most probably of a far smaller magnitude than changes caused by e.g. increased glacial melt ...". I find this difficult to understand. What exactly made you decide to refrain from analysing precip trends? Why do you assume that precip has no trend when precip did not reveal any clear trend patterns? Do you speak about regional precip trends / spatially coherent precip trends?

We analysed precipitation trends in the earlier paper (Kormann et al., 2014) and we could not find any significant trend patterns in precipitation, which was also reported in other studies. Some significant trends were found but these were spatially not coherent.
This means, spatially incoherent trends possibly might exist, but they cannot be detected due to a

low signal-to-noise ratio. Anyway, if these trends would exist, there would probably not be a clear signal in streamflow trends as there is no homogeneous signal in the precipitation trends. We added an explanation to clarify this issue (p7, 11ff).

The sentence "... precipitation changes are most probably of a far smaller magnitude than changes caused by e.g. increased glacial melt ..." is not clear. Do you mean 'changes in streamflow caused by increased glacial melt'?

Yes, we meant exactly this. however, we removed the sentence as it is probably impossible to verify.

p6888-14: Please give more explanations about the prewhitening methods you apply "... prewhitening methods described in Wang and Swail (2001) were applied ...". Did you apply several methods? Or just prewhitening for lag 1?

We added further explanations: "Lag-1 autocorrelation of the data is first calculated and then removed in the case that it is higher than a certain significance level (5 % in the present case)."

p6889-Equation 1: I do not understand equation 1 and feel that the explanation of MDT is not comprehensive enough. It would be good if one could understand MDT without going to Morin (2011). How generic is this equation? Does it apply to linear trends only? Has Morin (2011) used certain distributions in his Monte Carlo experiment and would this limit the application of MDT? Further, I am not sure what MDT adds to the work. From Fig. 2 I learn that trends are significant when they are outside the MDT band. If this is the case, then what additional information does MDT give?

The MDT points out the role of the signal-to-noise ratio when detecting trends. It makes visible, what otherwise might not have been obvious: That only at stations, where the detected trends are higher than a certain level (which is determined by the variability and the record length), the trends are significant. With this, we want to emphasize that trends may exist but do not get detected because of a low signal-to-noise ratio.
The MDT provides a potential explanation to support our 3rd hypothesis: Trends in mid-altitudes are not detected due to (1) the high variability in the data and (2) the low signal, which is caused by a compensating effect of increased glacial melt in higher altitudes and increasing ETP at lower altitudes.
In the revised manuscript, we further explained the MDT and additionally improved the comprehensibility of the equation (p10, 10ff).

p6889-section 3.1.3: Again, I think that more information about the method should be presented.

Thanks for the comment, we added further information:

"To analyse seasonal streamflow changes, we firstly applied indicators that are able to detect a change in the timing of the seasons. We used the approach of Renner and Bernhofer (2011), where a first order Fourier form model, is fitted to runoff data x with n observations per year (Stine et al

2009, Renner and Bernhofer 2011):

$$Y = \frac{2}{n} \sum_{j=1}^{j=n} e^{2i\pi(j-0.5)/n} \left(x_j - \bar{x}\right) \qquad (2)$$

From the complex valued Y, we estimate the phase $\phi_x = \tan^{-1}(\Re(Y)/\Im(Y))$ from the real and imaginary parts of Y. The annual phase of a variable describes the timing of its maximum within a given year. The amplitude $A_x = |Y|$ describes its range. By applying this harmonic filter to each year of data, we obtained a annual series of phase and amplitude which is further tested for trends. This approach was considered suitable for our purposes as well, as all of the annual hydrographs in our dataset follow a distinct seasonal cycle with strong streamflow maxima in summer and minima in winter. Fourier form models are a more robust measure than other commonly used indicators, like e.g. the centre of volume (Whitfield, 2013, Renner and Bernhofer, 2011). For further reading on this method, see Stine et al. (2009).”

p6892-9: Do you only average Tmin over all stations? If yes, does this mean that Tmin behaves similar across all stations but not Tmean and Tmax? What is the explanation for this result?

We needed some adaption of T trends to the mean watershed heights. As we found out that T trends in general (also Tmean and Tmax (!)) behave similarly across most of the stations analysed (Fig. 5 a)-c)), we averaged the daily trends over all stations analysed. However, Tmin proved to be most beneficial for the multiple regression model, so we only mentioned Tmin. We explained this in the methods section (p13, 19ff: The $\overline{\Delta T_{min}}$ time series are 30DMA trends averaged over all available stations. This was feasible, as similar trends concerning timing and magnitude occur at all stations analysed).

p6895-11: I do not understand the following sentences: "... The Mann–Kendall trend test has been criticised in some recent publications, particularly for the following issues: streamflow is usually not an independent and identically distributed variable, which is a precondition for using the MK test. Furthermore, a trend could be nonlinear or a part of a multispectral oscillation. Therefore, similar to Déry et al. (2009), the Sen’s Slope Estimators are presented as well without assigning trend significance. ..." The Mann-Kendal test estimates the significance of gradual trends and Sen’s slope estimates the magnitude/slope of a gradual trend. Hence, both methods give complementary information and are usually applied together. This is done also in this paper which is fine. However, the given justification is strange: (1) independence: this should have been considered via prewhitening, (2) nonlinear: the Mann-Kendall test does not require that the trend is linear, but it tests gradual change, (3) part of multispectral oscillation: I do not see that Sen’s slope deals in a better way with oscillations.

We actually wanted to question the use of the Mann-Kendall test (or significance tests) as such (which is also done in other trend studies). With this, we justify our decision of not using the

Mann-Kendall test in the further analyses of the paper. We did not aim to say that the Sen's Slope does the same thing or has better qualities. We shortened the paragraph to prevent confusion:

"The Mann-Kendall trend test and the Sen's Slope Estimator provide complementary information which we combined in illustrating the annual and seasonal trends. However, for reasons of graphical display and continuity we restrict further analyses of the seasonal changes to the Sen's slopes."

p6914 - Caption Fig. 1: I feel that this figure needs more explanation (in particular, since the other Kormann paper is in press only). Please give the significance level used. What exactly means 'trend in percent'? Even stations with 1% trend are significant - this is somewhat surprising. What is the time period studied?
Technical corrections: Several locations: The reference "Kormann et al., 2013" needs to be corrected to "Kormann et al., 2014".
Several locations: Trend magnitudes are given in %. How are they calculated? Change in magnitude during 1980-2010 divided by mean magnitude?

Thanks for the comment, we corrected and clarified the corresponding sections. Trends are given in per cent change per year, with a significance level of *alpha*=0.1 (We complemented: p14, 9; captions Fig. 1 and Fig. 3). The magnitudes in per cent are calculated from the *change per year* divided by *mean annual* streamflow. This is maybe why the magnitudes seem pretty small (but also trends of small magnitude may become significant when total variability is low). For knowing the change during the whole period, one has to multiply it with the number of years studied (31 years). We added "in percent change per year (period: 1980–2010; significance level: alpha=0.1" to caption of Fig. 1.

p6887-9: Does this sentence "... glacier mass balances have been completely negative only since the 1980s ..." refer to the Greater Alpine area?

Abermann et al. (2009) refer only to the Ötztal Alps. We first changed it for Abermann et al. (2011) who reported about mostly negative mass balances since 1980 for whole Austria. However, as proposed by Referee #4, we later discarded the sentence anyway.

p6889-5: Is Sen's slope really the "... mean of the slope between all possible pairs of data points ..."? I thought it was the median.

Thanks, we corrected that.

p6889-20: What do you mean with "... averaged observations ..."?

With "standard deviation of the series of averaged observations" we mean the standard deviation of a dataset, that already has been aggregated to a certain time resolution for analysing trends. We added an example (e.g. average annual streamflow) for better understanding.

p6890-11: The acronym 30DMA should not be used in the section title because it is introduced later.

Thanks, we corrected it: “30-day moving average trends and characteristic dates”

p6890-15: What do you mean by "... temporal relationship ..."? A relationship which changes in time?

Simply said, we meant that if something happens in one of the predictor variables on a certain day of year (e.g. T crosses the freezing point in spring; T trends turn up; snow height has reached its maximum in winter), and trends in streamflow turn up as well around this day of year, then this might indicate the causes for the streamflow trends. We clarified this with the following sentence: “If streamflow trends and the trends and CDs of temperature and snow depth occur at the same time, we suppose that this might be an indicator for one of the causes of the *Q* trends.”

p6892-3: These possible predictor variables are the indicators for temperature (mean, min, max) and snow height, right? In the current version, this sentence is somewhat cryptic.

We rewrote the sentence: “Based on the previous results of this study, we gathered all possible variables such as catchment properties, seasonal cycles of different variables, trends of other variables. These variables then served as predictor variables (independent variables) that could cause Q trends.“

p6896-8: I do not understand what you mean with ’Comparing single stations with each other’ in the sentence "... Comparing single stations with each other, it is shown that the fieldsignificant T trends appear in clusters that start and end during similar DOYs ..." Field significance looks at the complete collection of stations, it does not compare single stations.

Thanks for the comment. We changed it accordingly:
“Comparing single stations with each other, it is obvious that analogue T trends appear in clusters that start and end during similar DOYs”

p6896-23: Why should it be obvious? How do I know that snow height has a low signal-to-noise ratio?

We agree with the referee, therefore we removed the sentence.

p6901-6: Could you please extend the following sentences? I am not sure what is meant here: "... Our regression approach does not presume to capture the complete set of predictors, but is just meant as an heuristic approximation, as the Durbin–Watson statistic indeed indicates. Therefore, the coefficients should be taken with caution, since standard uncertainty measures cannot be derived in that case. ..."

We clarified this in the manuscript with the following section:
… we found significant autocorrelation in the residuals as the Durbin-Watson statistic indeed indicated. This is violating the assumptions of independence of linear regression, which often happens when fitting models to time series with a seasonal cycle. The autocorrelation in the residuals precludes statements on confidence bands and significance tests: The standard errors of the regression coefficients are potentially too small, which pretends higher model precision. However, our model stands as an approximation only. We are aware that the model is not perfect, as it is impossible to find all specific causes that explain the streamflow trends in our study region. The model is able to simulate streamflow trends sufficiently well, providing further hints on the causes of Q trends.

p6916-Fig3: Upper panel: I propose to change the color for ’not significant’ from dark blue to a color (e.g. white) which is not used for coding magnitude.

We agree with the referee and changed the coding accordingly.

p6917-Fig4: It seems that Figure 4 is not mentioned and discussed in the text.

On p15-26, we mentioned Fig. 4:
“The analysis on elevation dependence of the CDs of T and SD is presented in Fig. 4. The average DOYs of daily T_{mean}, T_{min} and T_{max} surpassing the freezing point ($\overline{DOY_{0^{\circ}Tmean/min/max}}$) all depend on station altitude, in spring as well as in autumn (Fig. 4a and b). The same applies for the average DOY of the annual snow depth maximum ($\overline{DOY_{SDmax}}$, Fig. 4c). Lines were fitted to represent these relationships. Nearly all the relationships analysed were found to be approximately linear.“

p6919-Fig6: Please include the line of perfect fit.

Thanks for the comment, we added a line of perfect fit.

References:

Abermann, J., Lambrecht, A., Fischer, A., and Kuhn, M.: Quantifying changes and trends in glacier area and volume in the Austrian Ötztal Alps (1969–1997–2006), The Cryosphere, 3, 205–215, doi:10.5194/tc-3-205-2009, 2009.

Abermann, J., M. Kuhn, and A. Fischer. A reconstruction of annual mass balances of Austria's glaciers from 1969 to 1998. Annals of Glaciology 52.59, 127-134, 2011.

Kormann, C., Francke, T., and Bronstert, A. (2014), Detection of regional climate change effects on alpine hydrology by daily resolution trend analysis in Tyrol, Austria, J. Water Clim. Change, in press, 2014.

Response to referee #4

Introduction

General comments:

1) The review of previous studies on trends in alpine rivers in the introduction is rather unsystematic, represents some of the references inaccurately, and omits relevant studies (a few examples below).

Thanks for the comment. We revised the introduction and address the issues in the specific comments section below.

Specific comments:

2) One way of improving the readability of the review may be to structure and order it by the known trends and hypotheses for attributed processes in order to work towards the attribution knowledge gaps later addressed; e.g. by drivers or by mean/seasonal/extremes change. Or alternatively from mountains globally to Alps to Austria.

We worked on more concisely following the structure you have proposed (“known trends”, “hypotheses for attributed processes”, “attribution knowledge gaps”) and improved the readability. Besides that, we added further literature to the review.
(→ Whole introduction section)

3) Published reports and grey literature make up a large part of the trend-research, which (as correctly noted) often doesn’t show very clear and exciting results and therefore often doesn’t make it into journal papers: An example of a very important study that looked at detailed trends in the entire Alps, and made an important step towards attribution by separation of regime and hence processes, was carried out in the AdaptAlp project. The technical report by Bard et al. 2011 is available on http://www.adaptalp.org. For Austria an ÖWAW paper looks at trends in high flows, low flows and their seasonality (Blöschl et al. 2011). Iris Stewart also published a nice paper in HP (Stewart, 2008) where she compares the snowpack change induced hydrological changes in many mountain regions, including the Alps. This may be more relevant to use here and in the discussion on attribution than her US papers.

Thanks for the reference suggestions, which we all have integrated into the review:

… Bard et al. (2011) made a relevant step forward by regime-specific trend analyses, as trend causing processes differ from one regime to another. … Stewart et al. (2008) reviewed snowmelt-induced streamflow changes in the literature and came to the conclusion, that especially at lower elevations, declining snowpacks and less snowfall affect streamflow quantities. … For further reading on low flow and flood regime changes, see e.g. … Blöschl et al. (2011) …

4) Some examples for unclear representation of the literature: The way reference is made to Déry et al., and Whitfield, is not very useful as one doesn't learn why and in which situation they criticize the COD. Déry et al. anyway elaborate mainly on the question of how a shift in time will be represented as a trend (an important aspect for attribution); Whitfield 2013's main concern is that the COV does not reflect the effect of temperature change (also a distinct aspect for attribution). A more balanced account on what these references contribute and why this is relevant to this study is required. The reference to Stahl and Moore 2006 is wrong: 25% glacier cover is not mentioned as a threshold and they did not draw attribution conclusions on runoff trends and glacier cover alone. What they did for attribution was to employ a formal statistical attribution analysis by fitting and analysing regression models for August (only!) streamflow and a subsequent analysis of the time trends in the residuals. Where these trends were negative, the reason for that were hence not the climate predictors, which are essentially filtered from the streamflow signal, but glacier retreat.

Specific comment on Whitfield (2013) and Déry et al. (2009):

We changed the reference to:

Whitfield (2013) claims that the 'centre of volume' is affected by other factors than temperature alone and has several shortcomings. Déry et al. (2009) found out that these metrics should be avoided, because they are sensitive to factors such as record length, streamflow seasonality and data variability.

Furthermore, we clarified the point of how a shift in time will be represented as a trend (See Editor comment #4).

Specific comment on Stahl and Moore (2006):

We changed the reference to:

Stahl and Moore (2006) fitted a regression model for August streamflow and then analysed trends in the residuals. The regression model accounted for the climate controls, so if the trends in the residuals were negative, they were attributed to increasing glacier melt. They found that most of the glacier fed streams are in the state of decreasing meltwater volumes.

5) 6885 line 5ff. This paragraph needs rephrasing to outline the way towards 'credibility in attribution' in a more scientific way. I re-read it several times, but without knowing the analyses/results from later, I doubt that anyone can understand its meaning.

Thanks for this comment. We have removed this section and added some sentences to the review section earlier (→ see point 6 below).

The three objectives are imprecise and contradictory. In 2) 'what areas?' – relation is unclear and if there are inconsistencies (in space?) then why by area anyway?. Anyway: what is the difference between 'explaining trends' (1); finding drivers (2); and attribution (3) – for me all is exactly the same, sorry.

We have changed the objectives to the following:

The present study combines the benefits of a temporally highly resolved trend analysis that is applicable to all different alpine runoff regimes with hydrological process understanding to explain seasonal streamflow changes in Western Austria. We aim to extend the knowledge about regional trend causes, with the attempt to provide a holistic picture of the changes found under different alpine streamflow conditions.

6) The final paragraph and reference to Kormann et. al. 2013 needs to be integrated with the rest of the review and/or used in the discussion section, but it cannot be used here or elsewhere. As pointed out by other referees, this paper needs to be understandable without knowing the other paper. This is not the case (see comments below) in several aspects.

We have integrated the reference into the review section (p3,z12 ff).

Other studies analysed temporally highly-resolved trends (Kim and Jain, 2010, Déry et al., 2009, Kormann et al., 2014). These trends in daily resolution have the advantage, that not only a shift in snowmelt timing but also other increases or decreases of the streamflow volume are revealed (Déry et al., 2009). Furthermore, a more detailed picture of the changes can be obtained by daily trends than by seasonal or annual averages, where a lot of the information is lost by averaging data over a certain period of time. In addition, the timing of daily trends (i.e. the day of year when a trend turns up) reveals supplementary information on potential drivers of streamflow trends (Kormann et al., 2014).

It also needs to be clear that no duplicate publication of results is presented, which seems an unresolved issue.

Due to the importance of this point, we addressed this issue already in a separate comment on HESSD:
http://www.hydrol-earth-syst-sci-discuss.net/11/C2850/2014/hessd-11-C2850-2014- supplement.pdf

Data
General Comments:

7) I would like to be convinced better that the hydropower operations don't influence attribution efforts. Do the hydrographs really show no sign of redistribution of flow from

summer to winter and of residual flow management? I can hardly believe this.

We have clarified this issue in the according section in the revised version of the manuscript. (p7, 28 – p8, 9).

Prior to the analysis, we checked for inhomogeneities in all hydroclimatic datasets. Any station that did not meet the requirements was removed. Additionally, we excluded streamflow records of catchments influenced by major hydro-electric power production beforehand. Unfortunately, it was impossible to exclude all watersheds with influences from hydro power stations, as water resources in Western Austria are used extensively: Only in Tirol, there are approximately 950 small-scale hydro power plants of differing type with a capacity lower than 10 Megawatts[2]. However, by far most of the small hydro power plants in Austria are run-of-river power plants (A. Egger (Tyrolean spokesman of the association on small hydro power plants in Austria), personal communication, July 29, 2014). These power plants do not have any pondage and thus there is no delay of river runoff. The rest of the small hydro power plants are mostly equipped with 1-day water storage volumes, which means there is a maximum delay of an average daily discharge amount, so the impacts on the seasonal discharge behaviour are very limited.

8) Another aspect about the choice of data that I see a problem in is the extensive use of nested catchments.

We added the section below to the revised manuscript (p7, 1 – 7). Additionally, we added the information which basins are nested to Tab. 1.

Eight of the 32 catchments analysed are nested. We used the approach that was applied as well in Birsan et al. (2005): To guarantee spatial independence of the station data, we checked for a considerable increase in watershed area among the corresponding gauges. Only the station pair Innergschlöß (39 sq km) and Tauernhaus (60 sq km) did not meet the requirements as defined in Birsan et al. (2005). However, as these basins were necessary to increase the number of catchments with glacial influence and the requirements of station independence were not violated too strongly, we left them in the dataset.

With so many upstream-downtream pairs or triplets in the analysis, and hence clear physical reason for cross-correlation, an analysis of field significance doesn't make sense.

Field significance is actually analysed in order to consider this issue: Field significance *determines*

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[2] http://www.kleinwasserkraft.at/en/hydropower-tyrol [May 2014]

the influence of cross-correlation between stations and thus tests the collective significance of the trends in one region (Birsan et al., 2005, Livezey and Chen, 1983; Burn and Elnur, 2002).

Detailed comments:

9) What is "relatively dry"? Be precise.

Thanks for this suggestion, we have added additional information:

With 970 ± 290 mm average precipitation amount per year (based on station data), this is a relatively dry region in the Alps as it is situated in the rain shadow of the northern and southern Alpine border ranges.

10) Line 5 ff. Are more details necessary? If there is anything important from Kormann 2013 about the data that is needed here to understand this study, this needs to be shown.

We added the climate stations to the map in Fig. 1, furthermore we added further information on the study region and on the data analysed:

There is a temperate climate with distinct precipitation maxima in summer. The majority of the watersheds under study drain into the Inn, Drava and Lech rivers, all tributaries of the Danube. For the most part, grassland and coniferous forest dominate the landuse in the lower catchment areas, whereas the percentage of rocky areas with little or no vegetation increases with increasing watershed altitude. Due to the strong influence of glacier and snow melt, mostly glacial and nival discharge regimes prevail which means discharge quantities have a distinct seasonal cycle with maxima in spring or summer and low flows in winter.

11) Give a bit more info on what HOMSTART is (station data? Interpolation product? Resolution?) and explain the acronym.

Thanks for the comment, we have done so:

“homogenised station datasets, Nemec et al., 2012)”

12) 6886 line 19 “cannot be interpreted in a detailed way” – why not and what detail? Unclear.

The number of stations is a trade-off between a large number of stations that cannot be interpreted in a detailed way and an insufficient number of stations that cannot be rated as representative. This is contrary to many other studies, which analyse trends for a larger area (e.g. nation-wide or european-wide). But with more stations, it is more challenging to interpret the results thoroughly, as different hydroclimatological conditions could potentially mask and thus complicate finding clear and coherent trend patterns.

13) 6887 and before – again ref. to Kormann 2013 paper out of place. Only the results should matter: but what is "most probably" – is there a conclusion from that other paper or not. If not it should be taken here and at the end, but not in the data section.

We partly disagree with the referee: We somehow have to point out why we decided to not analyse precipitation. In our opinion, this belongs to the data section.
We further clarified the conclusions from the last paper concerning precipitation trends:

In Kormann et al. (2014), precipitation trends were studied as well. However, no clear and coherent change patterns could be identified in this study (similar to e.g. Pellicciotti et al. (2010) or Schimon et al. (2011)). Precipitation changes might exist, but cannot be detected which is due to methodological limitations stemming from a low signal-to-noise ratio.

14) 6887 line 3ff. This paragraph is out of place here and not convincing. Better be honest and 1) state data for what period is available and used and then 2) very briefly say where it ranges in the long-term change pattern.

We revised the paragraphs about the selection of the period for data analysis:

We selected the period 1980-2010 for the data analysis. This ensured consistent data length for all hydro-climatic varaibles and best data availability. In this period, the Greater Alpine Region experienced a strong increase in air temperature by about 1.3 °C, compared to about 0.7 °C between 1900 and 1980 (Auer et al., 2007). The increase in the trend magnitude is apparent for all hydro-climatic variables (streamflow, temperature, snow depth, snowfall) within the study region (Kormann et al., 2014).

15) Abermann et al refer only to the Ötztal Alps? What about other glacierized basins? I seem to remember that elsewhere in the Alps MB was positive until the mid-80ies.

Thanks, Abermann et al. (2009) refer only to the Ötztal Alps. We first changed it for Abermann et al. (2011) who reported about mostly negative mass balances since 1980 for whole Austria. However, as proposed by Referee #4, we discarded the sentence anyway.

Methods

General comment:

16) The structure of the methods section is confusing. Headings and subheadings are a mix of statistical method and variables. The reader doesn't get a clear picture of a) the statistical methods used b) which method is applied to which variable c) how the two (method and variable) together converge to an attribution approach. Some order that follows the logic of the study, but definitely a clear separation between techniques/statistics and approach/application is required to understand this.

Thanks for the comment. We improved the readability of the methods section, amongst other things with a restructuring of the whole section.
In the new version, the first part of the section treats only methods used in trend detection as such:

3.1 Trend detection and significance
3.1.1 The Mann–Kendall test and the Sen's Slope Estimator for trend detection
3.1.2 Minimum detectability

The second part of the methods section covers the methods used for the detection of annual streamflow trends and changes in streamflow timing:

3.2 Detection of annual trends and timing changes of streamflow
3.2.1 Trends of annual streamflow averages
3.2.2 Streamflow timing changes

The third part treats the trend attribution efforts based on subseasonal trends:

3.3 Trend attribution via subseasonal examinations of streamflow changes
3.3.1 Trends and characteristic dates
3.3.2 Linear model identification
3.3.3 Hourly trends

17) 3.2.1 is particularly difficult to understand and unnecessarily so. Why not say that streamflow is first smoothed by a 30-day MA, then daily regime's are calculated, . . . and define CD when it is explained and not already before.

We have clarified as you proposed:

The 30-day moving average (30DMA) trends of Q, Tmean, Tmin and Tmax and SD were partly calculated and partly taken from Kormann et al. (2014): To calculate these CDs, all datasets were first smoothed by a 30-day moving average. Through this, comparability to the 30DMA trends is ensured and a more confident estimate of the CD is obtained because of reduced fluctuations. Then we calculated the mean annual cycles for each variable and each station for the years 1980 to 2010, in a daily resolution. Afterwards we selected the characteristic dates: ...

Some specific comments:

18) The concept of field significance is fairly standard and the terminology should be used from the start and then the method chosen to calculate it stated. The paragraph describing it could thus be more concise.

Thanks, we have changed the paragraph accordingly:

To account for spatial correlation in the data, a resampling approach was applied (Livezey and Chen, 1983, Burn and Elnur, 2002): After randomly shuffling the original dataset 500 times, all the resampled datasets were tested on trends in the same way as the original one. The percentage of stations that tested significant with a local significance level α_{local} in the original and in each of the resampled datasets was determined. Based on the distribution of significant trends in the resampled datasets, the value was calculated, which was exceeded with an $\alpha_{\mathrm{field}} = 10\ \%$ probability. This value was then compared to the percentage of significant results calculated from the original data. In case it is higher in the original dataset, the patterns found are called “field significant”.

19) 3.1.2. Eq 1: HESS discourages the use of multi-letter symbols (see manuscript preparation for instructions on symbols etc.). Record length should get a symbol and all variables need to be explained in the text. These comments also apply to other parts. Level of mathematical description of methods should be harmonized throughout the manuscript and clunky variables names like these for the DOY. . .. . . should be changed to more readable symbols.

DOY is a standard abbreviation for the term “day of year”, so we left it as is. However, we improved most of the other symbols. To help the reader we added a list of abbreviations in the appendix (→ A.2 List of symbols and abbreviations). In any case, we will accept further concrete suggestions by the referee.

20) How is trend magnitude calculated? It is used, but nowhere is described whether the slope is calculated by lin. regression with time or as a Sen-slope or some other way. -ok later found in the results section. This needs to be clearly described in the methods section!

This is described in the methods section on p6888, heading “3.1.1 The Mann–Kendall test and the Sen’s Slope Estimator for trend detection”. There is a full section on which methods have been used for detection of trend significance and magnitude.

21) 6890 line 19/20 what is ‘high-resolution’ – be precise.

Thanks, we have changed it (→ trends in daily resolution).

22) 6890 line 22 where they taken from Kormann 2013? Or really calculated following

. . ..?

For the majority of the variables, only *significant* trends were calculated in the first paper. In the new manuscript, also *insignificant trends plus field significances* were calculated (with the same approach). For Tmax and Tmin, the 30-day moving average trends were calculated only in the new manuscript.
We have changed it to "partly calculated and partly taken from Kormann et al."

Earlier it says that there 'only 30day means were looked at. Very confusing and needs to be clarified.

Unfortunately, we cannot find to which section the referee is pointing us.

23) 6890 last sentence: I don't understand this sentence at all. What is it?

We have rephrased the sentence:

The final result was a 365-value dataset per station, which provides information on significance and magnitude of the 30DMA trend for every day of the year.

24) 6891 line 16ff. I don't follow why this needs to be done. Do you mean out of all stations a general elevation dependence of this is derived? But then catchment-specific CDs and hence catchment-specific attribution is not necessary anymore. But wasn't this the aim (end of intro)?

The CDs are the characteristic dates, such as e.g. the average day of year (DOY), when T crosses the freezing point in spring. They serve as indicator, e.g. for the average timing in the year when snowmelt is possible in a certain watershed.
These CDs had to be fitted to the mean catchment altitude. E.g., if the mean watershed altitude was on 3000 m, it does not make sense to use a CD which is derived for a station that is only at 1000 m elevation. So we derived the CDs for each station and depicted the DOYs of these against station altitude (Fig. 4). The relationships were all found to be approximately linear, so a regression model was feasible to use. With this, we could transfer the DOYs of the CDs to the average watershed altitude and compare e.g. them with the streamflow trends we found.

In the methods section, we further clarified:

The CDs of T_{mean}, T_{min} and T_{max} and SD had to be fitted to the average altitudes of the watersheds. For this purpose, the average CD of each station was depicted as a function of station altitude. As all the CDs analysed had an approximate linear relationship with altitude, the DOYs of the trends and thresholds were transferred to the mean altitudes of the watersheds on the basis of a linear regression model.

In the results section, we further clarified:

The analysis on elevation dependence of the CDs of T and SD derived from climate stations is presented in Fig. 4. The average DOYs of daily T_{mean}, T_{min} and T_{max} surpassing the freezing point all depend on altitude, in spring as well as in autumn (Fig. 4a and b). The same applies for the average DOY of the annual snow depth maximum ($\overline{DOY_{SDmax}}$, Fig. 4c). Almost all the characteristic dates show a linear relationship with station altitude. Thus this linear relation is being used to establish a representative, long-term CD for each watershed using the mean catchment altitude.

Results

25) The Results and Discussion section is a mix of methods, results and discussion and very difficult to read and extract the essentials, also due to inadequate terminology, use of headings that are variables. I ran out of time reading all details and am afraid that impact will suffer if this section is not improved considerably by a clear separation of methods, results and discussion and more conciseness througout.

We added a separate discussion section. We improved readability with an additional section at the beginning of the results section (see following paragraph) and a schematic illustration on the different methods used in the appendix.

The results and discussion sections are structured according to the analyses that were conducted (for a schematic illustration, see appendix A.1). In the first part, we analysed trends of *annually averaged streamflow and trends of the results of the Fourier form models*. For this purpose, three different approaches were used: (1) mapping of annual trends in the study area, (2) analyses of a potential altitude dependency of the annual trends and (3) analyses of trends of the phase and the amplitude of the annual streamflow cycle. Based on the outcomes of this analyses, we defined research hypotheses (see introduction section).
To support these hypotheses, we derived *trends of seasonally averaged streamflow* in the second part, of not only streamflow but also (mean, maximum and minimum) temperature and snow depth. These seasonal trends were then further applied in the attribution approaches: (1) a combination of characteristic dates and trends, (2) a multiple regression model for streamflow trends and (3) hourly trends.

26) The statement of the three hypotheses in the first subsection of the results is out of place in a results section. It would make way more sense to state these in the intro or method – based on literature - and use them the to justify the design of the overall approach.

Thanks for the comment, we considered this point and moved the statement of the three hypotheses to the introduction section (p5, 14 – 24).

Hypotheses are falsified, not verified.

We agree with the reviewer that hypotheses never really can be verified, so we will change the wording from *verify theories* to *support theories*.

27) Fig 8: Glacier (ice) melt in April (and May) is virtually impossible and entirely unrealistic. First the snow on the glacier needs to melt, before ice can melt. What do glacier MB studies in the area say?

The strongest trends (that we attributed to icemelt) at e.g. Vernagt station (station ID no. 1, mean basin altitude: 3127 m) turn up around *end of May*. For the watershed of station ID no. 8, which has a lower average altitude (2590 m), strong streamflow trends start already half a month earlier, around *mid-May*.
This goes along with other studies: In Fig. 4 of Huss (2011), monthly components of glacier storage change are presented as mean over 1908–2008 for 50 glaciers of large-scale drainage basins in the European Alps: Icemelt starts in *May*, which is similarly found in Weber et al. (2010), Fig. 6, for the Upper Danube. Both plots are based on data in monthly resolution. For Hintereisferner (a glacier in the Ötztal Alps), *daily* mass balances show decreases of the net balance starting in early May for the exceptional year 2003: http://www.ptaagmb.com/the-glaciers/europe/austria/tirol/plusplus-vernagt-ferner-star.aspx, Fig. 8 (Daily Accumulation, Ablation and Net Balance).

We agree that the *main icemelt* is happening later in the year. However, the *strongest trends* turn up earlier (the *trends* in icemelt should not be confused with the actual amount of icemelt). These *trends* are highly connected to the temperature trends, which are as well strongest during this time of year. Later in the year, streamflow trends are probably caused as well by glacier melt, however, the strongest changes are observed between May and June.

Furthermore, as we also pointed out in the manuscript, it is probably impossible to explicitly separate snow and glacier melt. So the trends caused by earlier snow melt and less precipitation falling as snow are mixing later in the season with trends caused by glacier melt.

To further clarify this issue, we added the following sections to the manuscript:

At a first glance, glacier melt in May might appear as very early in the year when looking at seasonal streamflow composition. However, one has to note that the *trends* in glacier melt should not be confused with the *actual amount* of glacier melt: The main icemelt is happening later in the year, however, the strongest trends turn up earlier. These Q trends are highly connected to rising temperatures, which are as well strongest during this time of year. The results of modelling approaches (e.g. Alaoui et al., 2014) confirm our interpretations and suggest that glacier melt starts even earlier in the year.

Lastly, we agree that our schematic illustration of trend drivers was not precise enough and we exchanged it for real hydrographs.

28) I only learned from the results section that analyses were carried out for "sub-daily" "diurnal" data, but it is unclear how. Was hourly streamflow data used? Very unclear.

In the Methods section, there is a section on this analysis (p6892 “3.2.3 Diurnal streamflow trends”): “we analysed hourly streamflow and temperature data.”. Anyway, we added “the hourly *T* and *Q* trend analysis ” to the corresponding results section to clarify this issue.

Figures

29) The different color scales for the different trends are confusing (sometimes green is positive, sometimes negative). At least the colors for positive and negative trend signs should be the same always to allow comparison.

Thanks, we changed Fig. 5 and Fig. 7 accordingly.

The labels are not well readable, possibly an issue of resolution. Caption text needs to state the content and not describe the axes.

We provided figures in vector-format, so this issue should be solved by *Copernicus Publications*. Anyway, we further increased the size of the letters. Captions were improved as proposed.

30) Fig.3 dark blue is in the legend. It cannot be used to indicate no significance then. Suggest do Use grey or something. It would also be better to use a legend for the black and white instead of complicated caption description.

We changed the colours accordingly. However, we prefer to not use a legend for the field significance, as we don't really know where to accommodate the legend in Fig. 3.

31) Figure 6: why are there so many observed trends with zero? Constant flow at a particular time of the year? This would mean human regulation or gap filling? Needs to be explained.

Thanks, we added the following sentences:

“… All of these values were found at the gauge with the highest percentage of glaciated area in the watershed (ID 1, Vernagt). Also at this gauge, there are several occasions when observed trends are zero although the model predicts that there is a trend. This happens during earlier DOYs, when there is no discharge as all water in the basin is still frozen.“

Some examples of imprecise wording and inaccurate terminology. Language improvement and preciseness is essential in the revision.

We had the manuscript professionally double-checked by a native speaker. However, we tried to improve wording and terminology as proposed.

32) p.6884 line 28 “Totals of what?” be precise

We changed it to “streamflow averages”.

33) p.6884 line 29. What is a “single trend?” imprecise

We clarified: “Hence the isolation of trends, that are caused by one single source, is often not possible …“

34) p. 6885 line 5 “Contrary to that” (what anyway? Relation unclear)

We removed the paragraph as this was proposed earlier.

35) 6893 line 2: why suddenly ‘ water yield’ – previously you used ‘annual sums’. Better would be to have a variable named.

We removed this sentence.

36) 6893 line 9: trend in ‘annual totals’ – yet another term and not possible a trend needs to have a change unit per time unit.

Thanks, we changed it to “significant Q trends in annual averages”.

37) Commonly used is “snow depth” (not: snow height)

Thanks, we changed it throughout the manuscript.

38) It should be “basin/station elevation” (or “altitude”, but definitely not ‘height’) – inconsistent use throughout

Thanks, we changed also this throughout the manuscript.

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Attribution of high resolution streamflow trends in Western Austria – an approach based on climate and discharge station data

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Abstract

The results of streamflow trend studies are often characterised by mostly insignificant trends and inexplicable spatial patterns. In our study region, Western Austria, this applies especially for trends of annually averaged runoff. However, analysing the altitudinal aspect, we found that there is a trend gradient from higher-altitude to lower-altitude stations, i.e. a pattern of mostly positive annual trends at higher stations and negative ones at lower stations. At mid-altitudes, the trends are mostly insignificant. Here we hypothesize that the streamflow trends are caused by the following two main processes: On the one hand, melting glaciers produce excess runoff at higher-altitude watersheds. On the other hand, rising temperatures potentially alter hydrological conditions in terms of less snowfall, higher infiltration, enhanced evapotranspiration etc., which in turn results in decreasing streamflow trends at lower-altitude watersheds. However, these patterns are masked at mid-altitudes because the resulting positive and negative trends balance each other. To support these hypotheses, we attempted to attribute the detected trends to specific causes. For this purpose, we analysed the trends on a daily basis, as the causes for these changes might be restricted to a smaller temporal scale than the annual one. This allowed for the explicit determination of the exact days of year (DOY) when certain streamflow trends emerge, which were then linked with the corresponding DOYs of the trends and characteristic dates of other observed variables, e.g. the average DOY when temperature crosses the freezing point in spring. Based on these analyses, an empirical statistical model was derived that was able to simulate daily streamflow trends sufficiently well. The identified explanatory variables were the minimum temperature, the first derivative of the mean annual hydrograph indicating rising or falling conditions and the glacier percentage in the watershed. Analyses of subdaily streamflow changes provided additional insights. Finally, the present study supports many modelling approaches in the literature who found out that the main drivers of alpine streamflow changes are increased glacial melt, earlier snow melt and lower snow accumulation in wintertime. However, further research is needed to explicitly determine which processes related to positive temperature trends lead to the summertime streamflow decreases.

Keywords: Trend attribution; Trend detection; Mountain hydrology; Streamflow; Climate Change

1. Introduction

Climate change alters the hydrological conditions in many regions (Parry et al., 2007). Especially watersheds in mountain regions are more sensitive compared to those in lowlands (Barnett et al., 2005, Viviroli et al., 2011). This is mostly due to the strong connection between mountain hydroclimatology and temperature increase, which is at least twice as strong in mountainous areas compared to the global average (Brunetti et al., 2009): On the one hand, increasing temperatures result in diminishing glaciers, earlier snowmelt and less precipitation falling in the form of snow; on the other hand, the local climate is changed by interdependencies like e.g. the snow-albedo feedback (Hall et al., 2008).

A multitude of studies have tried to assess the detailed impacts of these changes through modeling approaches, especially for future scenarios (e.g. Magnusson et al., 2010, Tecklenburg et al., 2012, Vormoor et al., 2014). Another way of understanding climate change impacts on local hydrology is to analyse trends in observed streamflow data (e.g. Stahl et al., 2010, Dai et al., 2009). However, the aim of finding clear changing patterns is often hindered by strong noise in the data, as well as the fact that signals are usually small. Viviroli et al., 2011 note in their review paper on climate change and mountain water ressources, that trend studies in alpine regions often report "*inconclusive or misleading findings*".

However, other studies with different statistical approaches to analyse streamflow changes in alpine regions were published: In the mountainous areas of western North America, many studies agree that snowmelt and thus spring freshet is appearing earlier in the year (e.g. Stewart et al., 2005, Mote et al., 2005; Knowles et al., 2006). However, most of these studies are based on trends of indicators like 'centre of volume' or 'day of occurrence of the annual peak flow', which serve as proxys to indicate consequences of global warming on alpine streamflow (i.e. earlier snowmelt). The application of these measures is problematic: Whitfield (2013) claims that the 'centre of volume' is affected by other factors than temperature alone and has several shortcomings. Déry et al. (2009) found out that these metrics should be avoided, because they are sensitive to factors such as record length, streamflow seasonality and data variability. Contrary to these indicators, a measure that is based on a harmonic filter (Renner and Bernhofer, 2011) provides more robust estimates of the timing of the hydrological cycle. Other studies analysed temporally highly-resolved trends (Kim and Jain, 2010, Déry et al., 2009, Kormann et al., 2014). These trends in daily resolution have the advantage, that not only a shift in snowmelt timing but also other increases or decreases of the streamflow volume are revealed (Déry et al., 2009). Furthermore, a more detailed picture of the changes can be obtained by daily trends than by seasonal or annual averages, where a lot of the information is lost by averaging data over a certain period of time. In addition, the timing of daily trends (i.e. the day of year when a trend turns up) reveals supplementary information on potential drivers of streamflow trends (Kormann et al., 2014).

In hydroclimatology, the proof that observed changes are significantly different from variations that could be explained by natural variability is referred to as *trend detection*, whereas *trend attribution* describes the assignment of these changes to specific causes. Kundzewicz (2004) underlines the importance of not only trend detection but also trend attribution to understand the reasons for these changes. In this context, it is common practice to set up comparisons or correlations between the variable under consideration and the features of the system in which it is embedded (Merz et al., 2012a). However, previous analyses usually often considered trend magnitudes as the main subject of investigation, e.g. the correlation of observed streamflow trend magnitudes with certain catchment characteristics (e.g. glacier coverage). In addition, trends used for correlation analyses were mainly derived from annual or seasonal (3-monthly) streamflow averages. Both of these approaches are only partially capable of attributing trends, as streamflow integrates multiple processes across the watershed and different time scales. Hence the isolation of trends, that are caused by one single source, is often not possible, resulting in ambiguous outcomes (Merz et al., 2012a). Additionally, correlation can only give hints and does not imply causation. This is especially true in our case, as many of the watershed attributes are themselves correlated with each other (the higher a watershed, the more glaciated and the less vegetated it usually is).

In recent years, there has been some progress towards the attribution of streamflow trends via other approaches: Bard et al. (2011) made a relevant step forward by regime-specific trend analyses, as trend causing processes differ from one regime to another. Déry et al. (2009) used a simple model to simulate the cause-and-effect relations between the volume/timing of snowmelt and streamflow. Stewart et al. (2008) reviewed snowmelt-induced streamflow changes in the literature and came to the conclusion, that especially at lower elevations, declining snowpacks and less snowfall affect streamflow quantities.

Apart from the hydrological changes caused by earlier spring snowmelt, it is often difficult to find robust links between trend causes and their effects in observational data. Few studies have analysed the long-term effects of glacier mass loss on streamflow. Glaciers may have already reached the turning point when glacier mass has decreased to such a degree that meltwater volumes are reduced as well (Braun et al., 2000). Stahl and Moore (2006) fitted a regression model for August streamflow and then analysed trends in the residuals. The regression model accounted for the climate controls, so if the trends in the residuals were negative, they were attributed to increasing glacier melt. They found that most of the glacier fed streams are in the state of decreasing meltwater volumes. In Europe, however, Pellicciotti et al. (2010) related ice volume changes with streamflow trends and showed that streamflow is still increasing in four Swiss watersheds with high glacier coverage, and decreasing in one watershed with low coverage.

Next to changes through earlier snowmelt and increased glacial melt, climate change also influences streamflow through e.g. increasing evapotranspiration (ET) (Walter et al., 2004) or an increase of the timber line (Walther, 2003). However, robust links between detected trends and their causes are missing.

Summing up, there are several studies that elaborate on certain aspects of trend causes in alpine catchments. Hence, an integrated attempt would be desirable. For this purpose, the present study combines the benefits of a temporally highly resolved trend analysis that is applicable to all different alpine runoff regimes with hydrological process understanding to explain seasonal streamflow changes in Western Austria. We aim to extend the knowledge about regional trend causes, with the attempt to provide a holistic picture of the changes found under different alpine streamflow conditions. We limit our study to changes in mean values, and exclude analyses of extreme values since these changes might be caused by different processes. For further reading on low flow and flood regime changes, see e.g. Birsan et al. (2005), Parajka et al. (2009), Parajka et al. (2010), Blöschl et al. (2011), Hall et al. (2014).

Our study is divided in two parts, (1) an analysis of annually averaged trends/indicators and (2) an analysis of seasonally highly resolved trends. On the basis of the findings in the first part, we derived the following hypotheses:

- In higher-altitude, glaciated watersheds in the study region, rising temperatures result in increased glacial melt, which in turn cause positive annual streamflow trends. Most of the larger glaciers still have not reached the point where annual streamflow decreases because of decreasing glacier area.
- In lower-altitude, unglaciated watersheds, increasing temperatures result in earlier snowmelt and less precipitation falling as snow. This in turn leads to multiple hydrological changes such as higher evapotranspiration, higher infiltration or changing storage characteristics, to name a few. The negative streamflow trends in the study region are a result of these changes.
- In watersheds located at middle altitudes and covered by a smaller glacier percentage, both processes are prevalent to a lesser degree and compensate for each other.

To support these theories, it is necessary to attribute the streamflow trends. This is done in the second part of the present study: It is realised via a seasonal examination of the changes, as the driving processes for these changes might be limited to a smaller scale than the annual one.

2. Data

The study area is situated in Western Austria, mainly in North Tirol. With 970 ± 290 mm average precipitation amount per year (based on station data, 1980–2010), this is a relatively dry region in the Alps as it is situated in the rain shadow of the northern and southern Alpine border ranges. The study region includes altitudes from 673 m up to 3768 m a.s.l., with an extent of roughly 200 km in the East-West direction and 60 km in the North-South direction. There is a temperate climate with distinct precipitation maxima in summer. The majority of the watersheds under study drain into the Inn, Drava and Lech rivers, all tributaries of the Danube. For the most part, grassland and coniferous forest dominate the landuse in the lower catchment areas, whereas the percentage of rocky areas with little or no vegetation increases with increasing watershed altitude. Due to the strong influence of glacier and snow melt, mostly glacial and nival discharge regimes prevail which means discharge quantities have a distinct seasonal cycle with maxima in spring or summer and low flows in winter.

In the present analysis, we studied daily observations of mean, minimum and maximum temperatures (T_{avg}: 29, T_{min}: 12 and T_{max}: 10 stations), snow depth (SD*:* 43 stations) and streamflow (Q*:* 32 gauges), which were provided by *Hydrographischer Dienst Tirol (Innsbruck), AlpS GmbH (Innsbruck), Zentralanstalt für Meteorologie und Geodynamik (Vienna)* and *Tiroler Wasserkraft AG (Innsbruck).* T_{min} and T_{max} data was taken from the *HOMSTART* dataset (homogenised station datasets, Nemec et al., 2012). Hourly temperature data was only available for the *Vernagt* station, which was provided by the *Kommission für Glaziologie* (*Munich*, Escher-Vetter et al., 2014). The IDs of the T and SD stations were generated from the rank of station altitude, Q station IDs from the rank of mean watershed altitude, i.e., the higher the adjacent watershed, the lower the ID. Prior to the analysis, streamflow records were normalised by catchment area (flow rate per unit area).

Eight of the 32 catchments analysed are nested. We used the approach that was applied as well in Birsan et al. (2005): To guarantee spatial independence of the station data, we checked for a considerable increase in watershed area among the corresponding gauges. Only the station pair Innergschlöß (39 sq km) and Tauernhaus (60 sq km) did not meet the requirements as defined in Birsan et al. (2005). However, as these basins were necessary to increase the number of catchments with glacial influence and the requirements of station independence were not violated too strongly, we left them in the dataset.

The characteristics of the watersheds and their IDs are summarized in Table 1. A map of the study area together with the meteorological stations used in this study and annual streamflow trends is provided in the results section (Fig. 1).

In Kormann et al. (2014), precipitation trends were studied as well. However, no clear and coherent significant change patterns could be identified in this study (similar to e.g. Pellicciotti et al. (2010) or Schimon et al. (2011)). Precipitation changes might exist, but cannot be detected which is due to methodological limitations stemming from a low signal-to-noise ratio.

We selected the period 1980-2010 for the data analysis. This ensured consistent data length for all hydro-climatic varaibles and best data availability. In this period, the Greater Alpine Region experienced a strong increase in air temperature by about 1.3 °C, compared to about 0.7 °C between 1900 and 1980 (Auer et al., 2007). Furthermore, the magnitudes of streamflow, temperature, snow depth and snowfall trends is strongest for this period within the study region (Kormann et al., 2014).

All hydroclimatic datasets were checked by Austrian government officials via extensive examinations and plausibility checks. We additionally ensured that no data inhomogeneities remained. We further excluded streamflow records of catchments influenced by major hydro-electric power production. Unfortunately, it was impossible to exclude all watersheds with influences from hydro power stations, as water resources in Western Austria are used extensively: Only in Tirol, there are approximately 950 small-scale hydro power plants of differing type with a capacity lower than 10 Megawatts[1]. However, by far most of the small hydro power plants in Austria are run-of-river power plants (A. Egger (Tyrolean spokesman of the association on small hydro power plants in Austria), personal communication, July 29, 2014). These power plants do not have any pondage and thus there is no delay of river runoff. The rest of the small hydro power plants are mostly equipped with 1-day water storage volumes, which means there is a maximum delay of an average daily discharge amount, so the impacts on the seasonal discharge behaviour are very limited.

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[1] http://www.kleinwasserkraft.at/en/hydropower-tyrol [July 2014]

3. Methods

3.1 Trend detection and significance

3.1.1 The Mann-Kendall test and the Sen's Slope Estimator for trend detection

The rank-based Mann-Kendall (MK) test was used to calculate the trend significance. The MK test has been widely used in hydrological and climatological analyses (e.g. Gagnon and Gough, 2002, Birsan et al., 2005). Its advantages are the robustness concerning outliers, its high statistical power and the fact that it does not require a certain distribution of the data. A further description of the test is found in Helsel and Hirsch (1992).

The MK test in its original version has two main drawbacks: It accounts neither for autocorrelation in one station dataset, nor for cross-correlation between datasets of different stations. Both of them could result in the overestimation of an existent trend. Different methods of taking this into account have been published in recent years: Concerning serial correlation, the prewhitening method after Wang and Swail (2001) was applied: Lag-1 autocorrelation of the data is first calculated and then removed in the case that it is higher than a certain significance level (5 % in the present case). To account for spatial correlation in the data, a resampling approach was applied (Livezey and Chen, 1983, Burn and Elnur, 2002): After randomly shuffling the original dataset 500 times, all the resampled datasets were tested on trends in the same way as the original one. The percentage of stations that tested significant with a local significance level α_{local} in the original and in each of the resampled datasets was determined. Based on the distribution of significant trends in the resampled datasets, the value was calculated, which was exceeded with an α_{field} = 10 % probability. This value was then compared to the percentage of significant results calculated from the original data. In case it is higher in the original dataset, the patterns found are called "field significant".

After calculating the significance of a trend, it is necessary to estimate its magnitude, i.e. the slope of the trend. This was done by the robust linear Sen's Slope Estimator, which is computed from the median of the slope between all possible pairs of data points (Helsel and Hirsch, 1992). The Mann-Kendall trend test and the Sen's Slope Estimator provide complementary information which we combined in illustrating the annual and seasonal trends. However, for reasons of graphical display and continuity we restrict further analyses of the seasonal changes to the Sen's slopes.

3.1.2 Minimum detectability

To cope with the problem that trends may exist but do not get detected because of a low signal-to-noise ratio, we calculated minimal detectable trends (Δ_{MD}) as proposed by Morin (2011). In this study, annual mean values and coefficients of variance were computed for global precipitation data. Monte-Carlo simulations were carried out to generate trended data with similar statistical features as the original one but with varying trends. By testing the trend significance with the Mann-Kendall test, it was possible to estimate the minimal trend that was detected as significant in 50 % of the cases. This absolute trend was named the minimal detectable trend for a given station at a predefined α-level.

To calculate the Δ_{MD} of a given time series, we used the relationship that is represented in Fig. 6 of Morin, 2011. This is justified, as the minimal detectable trend does not depend on the magnitude of the data. The plot displays the change of the probability of significant trend detection versus signal-to-noise ratio (S/N) and record length (*R*), averaged over all previously simulated trend values. For a given time series with a given record length it is then necessary to look up the S/N that fits the red contour in the figure, i.e., the S/N at which the probability computed reaches the 0.5 threshold. This S/N is then transferred into Δ_{MD} using the following equation:

$$\Delta_{MD} = \frac{S/N * \sigma(X)}{R} \qquad (1)$$

where σ(X) is the standard deviation of the series of averaged observations (e.g. average annual streamflow).

3.2 Detection of annual streamflow trends and timing changes

3.2.1 Trends of annual streamflow averages

First, we derived trends of annual streamflow to understand, whether the overall yearly water availability changes while there is no information about seasonal changes. For this purpose, annual averages of streamflow were calculated and later tested on trend significance and magnitude. Next to this, minimal detectable trends of the annually averaged streamflow datasets were calculated to find out, whether trends might not get detected due to a high signal-to-noise ratio. Both significant and insignificant annual trends were then plotted on a map of the study area and against the mean watershed altitude. Lastly, general change patterns were identified.

3.2.2 Streamflow timing changes

To detect changes of the timing of seasonal streamflow, we used the approach of Renner and Bernhofer (2011). Here, a first order Fourier form model is fitted to runoff data x with n observations per year (Stine et al 2009, Renner and Bernhofer 2011):

$$Y=\frac{2}{n}\sum_{j=1}^{j=n} e^{2i\pi(j-0.5)/n}\left(x_j-\bar{x}\right) \qquad (2)$$

From the complex valued Y, we estimate the phase $\phi_x=\tan^{-1}\left(\Re(Y)/\Im(Y)\right)$ from the real and imaginary parts of Y. The annual phase of a variable describes the timing of its maximum within a given year. The amplitude $A_x=|Y|$ describes its range. By applying this harmonic filter to each year of data, we obtained a annual series of phase and amplitude which is further tested for trends.

The approach was considered suitable for our purposes as well, as all of the annual hydrographs in our dataset follow a distinct seasonal cycle with strong streamflow maxima in summer and minima in winter. Fourier form models are a more robust measure than other commonly used indicators, like e.g. the centre of volume (Whitfield, 2013, Renner and Bernhofer, 2011). For further reading on this method, see Stine et al. (2009).

3.3 Trend attribution via subseasonal examinations of streamflow changes

3.3.1 Trends and characteristic dates

To understand the relationship between streamflow trends and the variables that cause these trends, we derived high temporal resolution trends of streamflow on the one hand as the target variable and both (1) the trends and (2) characteristic dates (CDs) of explanatory variables on the other hand. We assume that it is possible to represent certain processes via these trends and the CDs. If streamflow trends and the trends and CDs of temperature and snow depth occur at the same time, we suppose that this might be an indicator for one of the causes of the Q trends.

(1) Initially, trends in daily resolution were derived. This approach enables the detection of finer temporal changes compared to the conventional annual or seasonal Mann-Kendall trend test. The 30-day moving average (30DMA) trends of Q, T_{mean}, T_{min} and T_{max} and SD were partly calculated and partly taken from Kormann et al. (2014): At first, the station dataset under consideration was filtered using a 30-day moving average. Then a time series of each DOY for the years 1980–2010 is derived which we then tested for trends. This procedure yields a 365-value dataset per station, which provides information on significance and magnitude of the 30DMA trend for every day of the year. These series allowed us to pinpoint the emergence, direction and magnitude of trends within the course of the year. In addition, daily field significances inform during which DOYs the trend patterns found were overall significant. The approach of trend detection via moving averages was similarly applied in Western US by Kim and Jain (2010) and Déry et al. (2009), however, they used only 3-day and 5-day moving averages and they only analysed trends in streamflow. Contrary to that, the 30-day moving average windows reduce daily fluctuations considerably. With this, the influence of single events on a specific day of year, which might cause erroneous trends, is reduced as well. The 30DMA trends thus yield more robust trends.

(2) Next to the trends, characteristic dates of the annual cycle of Q, T_{mean}, T_{min} and T_{max} and SD were derived. To calculate these CDs, all datasets were first smoothed by a 30-day moving average. Through this, comparability to the 30DMA trends is ensured and a more robust estimate of the CD is obtained because of reduced fluctuations. Then we calculated the mean annual cycles for each variable and each station for the years 1980 to 2010, in a daily resolution.

Afterwards we selected the characteristic dates: For streamflow, the DOY of the overall annual maximum streamflow ($\overline{DOY_{Qmax}}$) was chosen. With regard to the CDs of T_{mean}, T_{min} and T_{max}, we selected the average DOY when temperature passes the freezing point in spring and autumn (T = 0 °C (mean DOY when $T >$ -0.2 and $T <$ +0.2 °C)), as this point is crucial for multiple hydroclimatological processes in the watershed ($\overline{DOY_{0°Tmean/min/max}}$). Concerning snow depth, the average DOY of the annual maximum snow depth was chosen to indicate the date of the average start of the snowmelt in the watersheds ($\overline{DOY_{SDmax}}$).

The CDs of T_{mean}, T_{min} and T_{max} and SD had to be fitted to the average altitudes of the watersheds. For this purpose, the average CD of each station was depicted as a function of station altitude. As all the CDs analysed had an approximate linear relationship with altitude, the DOYs of the trends and thresholds were transferred to the mean altitudes of the watersheds on the basis of a linear regression model.

3.3.2 Linear model identification

An empirical statistical model is another tool for analysing which processes cause streamflow trends. Hence, a multiple linear model was fitted to the 30DMA streamflow trends found in the study region. This was restricted to the period between the beginning of March and mid-September (DOY 60 to DOY 250), where 85 % of the total annual streamflow and 84 % of the seasonal streamflow trends (based on absolute trend magnitudes) occur. It is approximately the time between the average annual snow depth maximum (top-of-winter) in spring, before snow and glacier melt starts, and the average start of snow depth increases in autumn.

Based on the previous results of this study, we gathered all possible variables which then served as predictor variables (independent variables): Next to catchment properties such as mean watershed altitude, glacier (forest etc.) percentage or decrease of glaciated area, we used linear regression to transfer long-term average temperatures to the mean watershed altitudes. This means, the assignment of the average temperatures was based on regionally derived temperature lapse rates. We decided to not use snow data as the assignment of snow depth to certain altitudes is highly uncertain. The $\overline{\Delta T}$ time series were 30DMA temperature trends averaged over all available stations. This was feasible, as similar trends concerning timing and magnitude occur at all stations analysed. Similar to the earlier analyses, all the datasets of hydroclimatological variables were filtered on the basis of 30-day moving averages beforehand.

Different combinations were first tested via a heuristic search based on the *R*-package *glmulti* (version: 1.0.7, Calcagno and de Mazancourt, 2010). Later, the model with the best performance in terms of an information criterion was chosen.

3.3.3 Hourly trends

To get an impression of the changes on a subdaily scale and support the previous statements based on seasonal trends, we analysed hourly streamflow and temperature data. As there were only a limited number of stations available, we selected several gauges that were representative for the area (*Gepatschalm*, *Obergurgl*, *Tumpen;* ID no. 3, 4 and 9; Table 1) with differing glacier percentages (39.3 %, 28.2 % and 11.8 %). Obergurgl and Tumpen are both located in the Ötztal valley, Gepatschalm is located in an adjacent valley. The data was available only in the period 1985 to 2010 (compared to 1980 to 2010 for the earlier analyses). The applied methods are analogous to the previous analyses: For each station, DOY and hour, 30DMA trends were calculated and depicted in a similar way to the seasonal 30DMA trends. However, compared to the earlier plots, the ordinate is now changed from rank of station altitude to hour of day. Accordingly, the averages of one day's trend magnitudes (the entire y-axis) are the same values as the trend magnitudes of one station in the earlier plot.

4. Results

The results and discussion sections are structured according to the analyses that were conducted (for a schematic illustration, see appendix A.1). In the first part, we analysed trends of *annually averaged streamflow and trends of the results of the Fourier form models*. For this purpose, three different approaches were used: (1) mapping of annual trends in the study area, (2) analyses of a potential altitude dependency of the annual trends and (3) analyses of trends of the phase and the amplitude of the annual streamflow cycle. Based on the outcomes of this analyses, we defined research hypotheses (see introduction section).

To support these hypotheses, we derived *trends of seasonally averaged streamflow* in the second part, of not only streamflow but also (mean, maximum and minimum) temperature and snow depth. These seasonal trends were then further applied in the attribution approaches: (1) a combination of characteristic dates and trends, (2) a multiple regression model for streamflow trends and (3) hourly trends.

4.1 Detection of trends based on annual averages, phases and amplitudes

Fig. 1 displays the annual streamflow trends ($\Delta Q_{\overline{year}}$), which were calculated from the change per year divided by mean annual streamflow, on a map of the study area. Roughly two-thirds of $\Delta Q_{\overline{year}}$ in the study region are not significant at a significance level of alpha=0.1, and no field significance was detected. The mapped trends neither depict any clear spatial trend pattern, nor show strong overall changes in Alpine hydrology. However, when presenting all annual streamflow trends, significant and insignificant, versus station ID as a rank of mean watershed altitude, another impression stands out (Fig. 2): It seems that higher-altitude watersheds depict mostly positive trends, whereas lower-altitude watersheds show negative trends. The watersheds at mid-altitudes show both positive and negative trends. Only nine out of 32 trends, where the change signal is high enough compared to the noise, are significant. The other ones are below the corresponding Δ_{MD}s. This applies both for trends calculated from the change per year divided by mean annual streamflow (Fig. 2 a) as well as for trends derived from absolute values (Fig. 2 b). Concerning the phase of streamflow, there is a clear signal of decreasing trends at higher stations (Fig 2 c), representing an earlier onset of spring freshet. At lower stations, phase trends are insignificant, mostly due to higher signal-to-noise ratios, which increase the minimal detectable trend (dashed lines). The trends of the streamflow amplitudes show a similar behaviour to the trends of annual Q averages, but shifted to mostly negative trends (Fig 2 d): In general, amplitudes are decreasing, but less so at higher stations and more so at lower stations.

All the trends mentioned above show an explicit correlation with the mean watershed altitude, which does not depend on trend significance (Table 2). Note that the Pearson's correlation coefficients of significant trends are based on fewer values, so in this case higher correlation coefficients are easier to obtain. All of the correlations tested significant at the $\alpha = 0.1$ level.

4.2 Trend attribution via subseasonal trends

4.2.1 Trends and characteristic dates of streamflow

As already found in Kormann et al. (2014), coherent 30DMA streamflow trend patterns appear when plotted against the time of year and altitude (Fig. 3a). We refer to the groups discernible in these plots as "trend patterns". Streamflow clearly rises in spring, followed by decreases in summer; both trend patterns depend on watershed altitude. Another obvious pattern is the positive one in autumn, roughly from October to December; this one was not found to be altitude-dependent. Over most of the time, the 30DMA trends are field-significant (Fig. 3a), *bar above diagram*), meaning the trend patterns as a whole are statistically more frequent than expected by random chance.

At higher-altitude basins, significant Q trends in annual averages ($\Delta Q_{\overline{year}}$) were found especially where ΔQ_{30DMA} in spring have high values (Fig. 3a), *bar on the right*). At lower stations, only two significant $\Delta Q_{\overline{year}}$ were detected, both at watersheds where hardly any positive ΔQ_{30DMA} were detected.

When analysing all 30DMA streamflow trends (Fig. 3b), not only the significant ones, the designated trend patterns are even more obvious. An additional positive trend pattern occurs in mid-August at higher stations, though this one is less evident than the others.

The CD, that indicates the DOY when the long-term annual streamflow peak occurs ($\overline{DOY}_{Qmax}$), is often found after the increasing trends in spring and before the decreasing trends in summer (Fig. 3b), which is especially true for lower stations. This means that increasing Q trends mostly occur during the rising limb, and decreasing ones during the falling limb of the seasonal hydrograph. These patterns correspond to a shift in the hydrograph and thus a decreasing trend in the phase of streamflow timing.

4.2.2 Trends and characteristic dates of temperature and snow depth

The analysis on elevation dependence of the CDs of T and SD derived from climate stations is presented in Fig. 4. The average DOYs of daily T_{mean}, T_{min} and T_{max} surpassing the freezing point ($\overline{DOY_{0°Tmean/min/max}}$) all depend on altitude, in spring as well as in autumn (Fig. 4a and b). The same applies for the average DOY of the annual snow depth maximum ($\overline{DOY_{SDmax}}$, Fig. 4c). Almost all the characteristic dates show a linear relationship with station altitude. Thus this linear relation is being used to establish a representative, long-term CD for each watershed using the mean catchment altitude.

Regarding trends, there are differences between the T_{min}, T_{max} and T_{mean} trends, but these differences mostly concern the trend magnitude, not its direction or timing (Fig. 5 a, b and c). Comparing single stations with each other, it is obvious that the T trends appear in temporal clusters that start and end during similar DOYs. Four main patterns of field-significant positive T trends are evident: 1) mid-March until the beginning of May, 2) mid-May until the end of June, 3) the beginning of July until mid-August, and 4) the beginning of October until mid-November. The T_{max} trends are roughly twice as intense as the ones for T_{min} and T_{mean}, but field significance was detected only in two of the four highlighted segments (upper bar in Fig 5). For most of the stations, the magnitude and days of occurrence are similar, meaning there is no altitude dependence of the T trend signal.

Fig. 5d shows the analogous trend results for the explanatory variable snow depth (SD). Strong negative SD trends dominate the results; however, some positive trends occur at two upper stations and around November at many of the stations. One main cluster of field-significant trends in spring can be distinguished, which also indicates that local significant trends were found only in spring.

4.2.3 Comparison of the timing of trends and characteristic dates of streamflow with those of temperature and snow depth

Spring ($\overline{DOY_{0°TmaxSpring}}$ *to* $\overline{DOY_{0°TminSpring}}$ *):* $\overline{DOY_{0°TmaxSpring}}$ and $\overline{DOY_{SDmax}}$ appear during similar days as the first Q trends (Fig. 5e). Between $\overline{DOY_{0°TmaxSpring}}$ and $\overline{DOY_{0°TmeanSpring}}$, the Q trend magnitudes further increase, most of them in shifts, i.e. first the lower basins around early March and the later ones in April. In April, there is a general major peak in the observed streamflow trends at basically all of the watersheds. This is also the time when field-significant SD trends turn up at the majority of stations (Fig. 5d). During this period, it seems that there is an elevation-dependent trend pattern between $\overline{DOY_{0^{\circ}TmaxSpring}}$ to $\overline{DOY_{0^{\circ}TminSpring}}$ superposed by an elevation-independent one.

The overall strongest Q trends occur at high-lying watersheds after the average daily T_{mean} is positive and when T_{min} is still negative. T trends are also at their highest levels during this time of year, and the dynamics of the T trends resemble the ones in the Q trends with overall maxima between end of May and beginning of June. Pearson's r between all single streamflow trends from $\overline{DOY_{0^{\circ}TmeanSpring}}$ to $\overline{DOY_{0^{\circ}TminSpring}}$ and the corresponding glacier percentage in the watershed was calculated at 0.74, which means the strongest Q trends turn up mostly at watersheds that are highly glaciated.

Some trends at mid-altitude watersheds stand out with high magnitudes and long persistence (at gauges No. 8, 12, 17). All these rivers are fed by glaciers that originate from the *Hohe Tauern* region (eastern side of the study region, cf. Fig. 1).

Summer ($\overline{DOY_{0^{\circ}TminSpring}}$ *to* $\overline{DOY_{0^{\circ}TminAutumn}}$ *):* During summer, many of the Q trends observed are negative, with the strongest ones at lower basins after T_{min} has crossed the freezing point in spring. At higher, glaciated watersheds, negative Q trends occur only after positive Q trends have diminished. Field significant T trends go along with these Q trends; both of them are especially strong from mid-May until mid-June.

Autumn ($\overline{DOY_{0^{\circ}TminAutumn}}$ *to* $\overline{DOY_{0^{\circ}TmaxAutumn}}$ *):* In autumn there are two main patterns with opposing signs: Negative Q trends at higher-altitude watersheds in September and slightly positive Q trends at all watersheds around October. In September, the negative Q trends coincide with negative T trends. In October, positive field-significant trends in T_{mean} and T_{min} were detected. $\mathrm{DOY}_{\mathrm{0^{\circ}Tmax\_Autumn}}$ and $\mathrm{DOY}_{\mathrm{0^{\circ}Tmin\_Autumn}}$ do not border the Q trends as clearly as in spring.

Winter ($\overline{DOY_{0°TmaxAutumn}}$ *to* $\overline{DOY_{0°TmaxSpring}}$)*:* All throughout winter, there is hardly any streamflow persisting in the highest watersheds. This is also reflected in the fact that there are only few trends at the upper 20 watersheds. Contrary to that, minor streamflow trends exist at lower watersheds; however, there is no clear positive or negative pattern and trend magnitudes are small.

4.2.4 Empirical statistical model for streamflow trends

The heuristic model selection based on the information criteria identified the most relevant explanatory variables. The best performance (the adjusted R^2 was calculated as 0.70) was achieved with the model in Eq. 3. Note that we normalized the trend of streamflow at a specific DOY (ΔQ_{30DMA}), as well as the first derivative of the seasonal 30DMA Q average ($\dot{\overline{Q_{30DMA}}}$) by the long-term average streamflow at a specific DOY ($\overline{Q_{30DMA}}$).

$$\frac{\Delta Q_{30DMA}}{\overline{Q_{30DMA}}} = 0.0017 - 0.096\,\overline{\Delta T_{min}} + 0.0036\,\frac{\dot{\overline{Q_{30DMA}}}}{\overline{Q_{30DMA}}} + 0.59\,\frac{A_{ice}}{A_{tot}}\,\overline{\Delta T_{min}} \qquad (3)$$

From the a-priori selected explanatory variables, we found that only 3 variables are required to predict the streamflow trend at a specific day of the year: minimum temperature, the first derivative of streamflow indicating rising or falling streamflow conditions as well as the percentage of glaciated area in a watershed (A_{ice}/A_{tot}) multiplied by the 30DMA T_{min} trend in °C per year for the corresponding DOY, averaged over all available stations.

The prerequisites of a linear model (homoscedascity, normally distributed residuals) were checked via standard diagnostic plots. The large majority of the predicted trend values were in accordance with the observed ones (Fig. 6); only several very high values (> 4 %) could not be simulated well. All of these values were found at the gauge with the highest percentage of glaciated area in the watershed (ID 1, Vernagt). Also at this gauge, there are several occasions when observed trends are zero although the model predicts that there is a trend. This happens during earlier DOYs, when there is no discharge as all water in the basin is still frozen.

4.2.5 Analysis of hourly streamflow trends

The overall results of the hourly *T* and *Q* trend analysis show similar structures to the seasonal one (Fig. 7). Concerning *Q*, there are certain periods when subdaily dynamics in *Q* trends are obvious, like the period from mid-May until mid-June. During other periods, there is hardly any difference between the trends at different times of day.

More specifically, from mid-March to early May, there is merely a diurnal dynamic in the *Q* trends. Positive *T* trends without any explicit diurnal dynamic occur at the same time. Contrasting with this, from mid-May until mid-June there is a clear dependency between the positive trends in the afternoon, the time of day and the watershed analysed: The lower the watershed and the smaller the glacier percentage, the later the *Q* trends occur and the lower are their magnitudes.

5. Discussion

5.1 Detection of trends based on annual averages, phases and amplitudes

The positive (and often significant) annual streamflow trends at higher-altitude, glaciated watersheds might be a sign that glaciers in Western Austria are still in the phase, where overall streamflow still rises due to increasing glacial melt. This corresponds well with other studies in the European Alps (Pellicciotti et al., 2010, Bard et al., 2011, Braun and Escher-Vetter, 1996).

Contrary to that, the annual *Q* trends at lower-altitude basins are mostly insignificant, but negative. Rising temperatures change hydroclimatic conditions in the basins, resulting in e.g. shorter winters, higher evapotranspiration, higher infiltration and alternating storage capacities (Berghuijs et al., 2014). Hence, less water contributes directly to runoff, which might be a potential cause for the negative annual trends observed in lower-altitude basins.

The ambigous change signals of annual *Q* trends at mid-altitude watersheds with little or no glacier cover might be a result of a balancing effect of increased glacial melt and rising evapotranspiration. Hence, trends are mostly lower than the corresponding minimal detectable trends, so in many cases, no significance is detected. This goes along with Birsan et al. (2005), who found decreasing trends in basins with a glacier cover of less than 10 %.

The present analysis of annual streamflow trends shows once more that it is important to also include insignificant trends in the interpretation of the results. It might not have been possible to find the overall altitude-dependent patterns when only looking at significant results. However, it is crucial to interpret the insignificant trend results more carefully.

The analyses of Q phase and Q amplitude highlight the different behaviour of higher- and lower-altitude watersheds under climate change. We observe a significant shifts towards earlier streamflow timing in the upper catchments, whereas the amplitudes decrease in the lower catchments. However, the Fourier form models are increasingly uncertain in lower catchments where the annual hydrograph deviates from a harmonic fucntion. Therefore, a seasonal trend analysis is required to detect potential regime changes.

5.2 Trend attribution via subseasonal trends

5.2.1 Comparison of the timing of trends and characteristic dates of streamflow with those of temperature and snow depth

Spring: The ambiguous structure of the mid-January to April streamflow increases (altitude dependent vs. altitude independent trends) is possibly caused by the following two mechanisms: On the one hand, temperatures need to rise above the freezing level to allow for snowmelt initiation. This DOY depends on the altitude of the snowpack (e.g. Reece and Aguado (1992) found an altitudinal melt onset gradient of 4 days per 100 m in the Sierra Nevada). With T trends occurring during the whole spring, snowmelt initiation shifted to earlier DOYs, which probably caused the elevation-dependent trend pattern.

On the other hand, the average spring rise of streamflow occurs at most of the watersheds in the study region during similar days of the year (see Kormann et al., 2014), which implies that snowmelt starts simultaneously at different altitudes. Hence, it seems that snowmelt in our study region is highly driven via weather patterns and their hydrological effects such as rain-on-snow events that influence e.g. whole valleys and not just single altitude bands. Garvelmann et al.

(2014) showed that snowmelt is strongly driven via rain-on-snow events and highly depends on the previous moisture of the snow pack. Lundquist et al. (2004) observed altitude-independent snow melt in single years. With increasing *T,* rain-on-snow events might have turned up earlier in the season, thus causing the elevation-independent trend pattern during spring.

It is possible, that in some years, the first mechanism is stronger, and in other years the second one, with both of them moving to earlier DOYs.

The May to June streamflow increases at upper watersheds are by far the strongest Q trends that were found. The similar dynamics of T and positive Q trends during this period suggest a strongly temperature-driven trend cause. Furthermore, not only the high correlation of the Q trend magnitude with watershed glacier percentage but also the fact, that many trends in glaciated basins still persist when average T_{min} has already been above 0° C for many days (see next section), indicate that these pattern might be caused by increasing glacial melt. The strong Q trends of watersheds in the Hohe Tauern region suggest a particularly high glacial meltdown in this area.

All these evidences suggest that the first spring trend pattern is caused by both earlier snowmelt and less snowfall (Kormann et al., 2014) and the second one is a result of shrinking glaciers due to rising temperatures. Anyway, one has to keep in mind that it is practically impossible to explicitly separate trends caused by snow melt and the ones caused by glacier melt, as melt at lower glacier parts already starts while the upper parts are still covered with snow.

At a first glance, glacier melt in May might appear as very early in the year when looking at seasonal streamflow composition. However, one has to note that the *trends* in glacier melt should not be confused with the *actual amount* of glacier melt: The main icemelt is happening later in the year, however, the strongest trends turn up earlier. These *Q* trends are highly connected to temperature trends, which are as well strongest during this time of year (cf. Fig. 5). The results of modelling approaches (e.g. Alaoui et al., 2014) confirm our interpretations and suggest that glacier melt starts even earlier in the year.

Summer: In summer, the snow reservoir has already emptied out in most of the watersheds. The negative *Q* trends during this time of year are possibly part of the effects of earlier snowmelt timing on streamflow. This shift causes first rising and directly afterwards dropping streamflow trends in spring and summer, which were similarly found for watersheds in western North America by other daily resolved trend analyses (Kim and Jain, 2010, Déry et al., 2009). However, to fully attribute summertime Q decreases, it would be necessary to separate the effects of shifts in snowmelt timing from the effects of lower snow accumulation (and with this, lower snowmelt volumes). This task had been adressed in Déry et al. (2009) by a simple model approach. However, a separation of these effects based on analyses of other observed variables is difficult, as negative Q trends in summer might also have other causes such as higher infiltration, rising evapotranspiration and changing storage conditions (Berghuijs et al., 2014).

At higher-altitude basins, the negative summertime Q trends are balanced to a certain degree by positive trends due to excess water from glacial melt, which is evident via trends that persist far longer than the $\overline{DOY}_{0°TminSpring}$. This superimposition might also cause positive Q trends in mid-August at upper stations, maybe because the negative summertime trends have already weakened then. According to Stahl and Moore (2006), the biggest difference in streamflow trends of glaciated and unglaciated basins is found during the month of August. However, contrasting to their study, we found mainly increasing August Q trends at glaciated watersheds and slightly decreasing ones at unglaciated watersheds.

The altitude dependency of the timing of $\overline{DOY}_{Qmax}$ highlights the need for highly resolved, subseasonal trend analyses: As upward trends generally occur before and downward trends occur after $\overline{DOY}_{Qmax}$, a separation of trend statistics in periods of 3-month (spring, summer, autumn, winter), as it is usually done in trend studies, might produce ambiguous trend results especially in summertime.

Autumn: Cahynová and Huth (2009) showed that significant increases in cyclonic circulation types are the major cause for autumn temperature decreases. These negative T trends in turn might have caused the Q decreases at higher-altitude basins in September, as during this time of year, the glacier is exceptionally not melting but accumulating. These effects are possibly increased by the negative summertime Q trends due to snow decreases in the previous winter and earlier melt. Contrary to that, during October, rising T_{mean} and T_{min} might cause less snowfall and less snow to be accumulated and hence generate more rainfall-driven runoff during this time of year. This generally goes along with the interpretations in earlier literature (e.g. Déry et al., 2005).

Winter: During winter, $T_{\max}$ is far below zero, so on average no melt processes are possible. However, temperatures might reach above zero in the lower catchment areas of certain watersheds, so positive Q trends could be caused through lower snow accumulation in these watersheds. The negative trends in absolute snow depth might have been caused at the beginning of the winter, so it is plausible that these have no effect on streamflow during mid-winter. These interpretations generally go along with e.g. Scherrer et al. (2004), who attributed SD decreases at lower-altitude stations to T increases rather than changes in precipitation patterns.

5.2.2 Empirical statistical model for the identification of streamflow trends

The multiple linear model is able to simulate daily streamflow trends sufficiently well. The predictor $\overline{\dot{Q}_{30\mathrm{DMA}}}$ accounts for both positive Q trends in the rising limb of the annual Q cycle (before the annual maximum) and for negative trends that turn up in the falling limb (cf. Fig. 3). Reinterpreted as a trend, the term $\overline{\dot{Q}_{30\mathrm{DMA}}}$ corresponds to a shift in earlier streamflow timing of one day per year. The coefficient (0.36) in our model adjusts this term to the shift found in our data. For the 30-year study period, this counts up to a shift of 10.8 days of earlier streamflow timing, which is similar to shifts reported in the literature. For example, Renner and Bernhofer (2011) report an shift of 10 to 22 days earlier timing (comparing 1950–1988, and 1989–2009) in the runoff ratio for catchments in the low mountain ranges of Saxony, Germany. Déry et al. (2005) found that annual peak snowmelt discharge appears roughly 8 days earlier (study period 1964–2000), Stewart et al. (2005) detected a shift of 6–19 days (1948–2003), both in North America and based on timing measures such as 'centre of volume'. However, depending on factors like the study period, region and the methods used, results in previous literature differ strongly.

The predictor 'A_{ice}/A_{tot}' considers the increased excess water from glacial melt in the model. The selection of this term and not that of e.g. 'decrease of glaciated area' (which has been tested as well) supports the findings of Weber et al. (2009): As glacial melt mostly occurs at the surface, the quantity of melt water generally behaves proportionately to the extent of glaciated area in the watershed, independent of the underlying glacier thickness.

The glacial melt is driven via the temperature increases, hence the glacier term includes the 30DMA temperature trends. As the '$A_{ice}/A_{tot}\ \overline{\Delta T}_{min}$' term enters the model with a positive coefficient, one can assume that the majority of the glaciers have not yet reached the point when overall streamflow decreases due to diminishing glacier mass.

The additional single term '$\overline{\Delta T}_{min}$' has a negative coefficient, and hence might account for the negative trends in summertime caused by increased ET, higher infiltration and decreased snow cover accumulation. The selection of $\overline{\Delta T}_{min}$ instead of $\overline{\Delta T}_{max}$ is somehow surprising, as one might expect many of the streamflow trends to be strongest during daytime, when temperatures are at their highest. Indeed, the selection makes sense: The ground is potentially frozen once T_{min} falls below zero. If this is the case, additional energy is necessary for melting during daytime. With a rise in T_{min}, energy that is not needed any more for melting is now available for atmospheric warming in addition to $\overline{\Delta T}_{min}$ alone.

The advantage that only little input data is necessary has also some drawbacks: As the model is very slim, it only captures the main factors that could cause streamflow trends in highly alpine catchments. Contributors such as changes in groundwater or precipitation are not accounted for explicitly, only via their response to the other predictors. In autumn, the model is not able to simulate the actual trends adequately either. However, these trends are small in magnitude and do not influence the overall statements too much.

Furthermore, we found significant autocorrelation in the residuals, as the Durbin-Watson statistic indeed indicated. This is violating the assumptions of independence of linear regression, which often happens when fitting models to time series with a seasonal cycle. The autocorrelation in the residuals precludes statements on confidence bands and significance tests: The standard errors of the regression coefficients are potentially too small, which pretends higher model precision. However, our model stands as an approximation only. We are aware that the model is not perfect, as it is impossible to find all specific causes that explain the streamflow trends in our study region. The model is able to simulate streamflow trends sufficiently well, providing further hints on the causes of Q trends.

5.2.3 Analysis of subdaily streamflow trends

The hourly *Q* trend analysis supports the findings of the earlier analyses. Going into detail, the patterns found might occur for the following reasons: Due to the relatively low albedo of glacial ice (~0.3 to 0.5) compared to snow (~0.7 to 0.9, Paterson, 1994), glacial melt depends stronger on incoming radiation than snowmelt. Climate change results in earlier snow-free conditions on glaciers, which in turn cause earlier glacial melt during noontime. The resulting *Q* trends are temporally delayed with increasing distance from the glacier and their magnitudes decrease with decreasing watershed altitude. This might be due to a generally lower percentage of glaciated area in the lower-altitude basins and a balancing effect of the negative *Q* trends which is caused by earlier snowmelt, lower snow accumulation and rising ET.

In this context, it is noteworthy that there is no clear subdaily dynamic in the negative trends during DOYs with *T* increases: With rising ET, one would expect stronger negative *Q* reductions at noon due to the maximum necessary radiation input. This is either balanced via glacial melt or the magnitude of the changes is too small compared to the reductions due to the shift of snowmelt to earlier DOYs.

5.2.4 Synthesis of the streamflow trend attribution approach

In the following we synthesize the streamflow trends and potential causes. The overall findings are illustrated with three representative catchments. Fig. 8(a) represents a typical higher-altitude watershed (Gepatschalm, 2880 m, 39.3 % glaciated), (b) a mid-altitude, little glaciated watershed (See i. P., 2303 m, 1.6 % glaciated) and (c) a lower-altitude, unglaciated watershed (Ehrwald, 1467 m), which are depicted along with the detected trends and their probable main drivers. Our seasonal analyses support the hypotheses that we proposed in the introduction section: The subseasonal structure of streamflow trends in higher-altitude, glaciated watersheds corresponds well with the one that might stem from glacier wastage. The overall annual 30DMA trend integral over time (and thus the annual trend) is positive, as additional water in spring enters the basin (Fig. 8 a). In lower-altitude watersheds, especially summertime decreases lead to an overall negative annual trend integral (Fig. 8 c). In case the annual 30DMA trend integral over time is close to zero, the trends are caused by shifts rather than by changes of the overall streamflow amount (Déry et al., 2009). This might be the case in mid-altitude, little glaciated watersheds, where only small changes affect the annual hydrograph (Fig. 8 b).

In summary, the two main influences on alpine streamflow are the increased glacial melt and the shift to earlier snowmelt, both driven via temperature increases. This is supported by many studies in alpine regions, where drivers of streamflow changes were identified via modelling approaches (e.g. Braun et al., 2010). Anyway, we want to emphasise that our analysis is based on observed station data only. For this reason, we consider our statements concerning both the detection and the attribution of the changes to be more robust than results obtained by stand-alone model approaches. However, a few patterns still exist, where streamflow trend attribution via temperature, glacier and snow depth changes is not sufficient and thus the need for further research remains: For example, we could not explicitly identify the drivers of summer streamflow decreases, especially with regard to ET increases. Also a model approach was not successful to identify the role of rising ET on alpine hydrology for the past: Al Alaoui et al. (2014) could not establish a link between changing hydrology and increased ET in the Swiss Alps for the years 1983–2005. This was mainly due to the fact, that hydrological changes due to rising ET were masked by changes caused by snow and glacier melt, which is similar in our results.

Nevertheless, the shift of snowmelt to earlier DOYs and a higher rain/snow ratio has been detected, also by other studies. With this, the watershed potentially receives more precipitation in the form of rain which in turn possibly leads to higher annual infiltration and interception rates. This water might be additionally available for evapotranspiration and vegetation growth and thus will reduce seasonal - and with this annual - streamflow amounts. The study of Berghuijs et al. (2014) supports this assumption for the contiguous US: They found observational evidence, that a reduction in the percentage of snow in total precipitation goes along with decreases in average streamflow.

Also higher transpiration rates through vegetation changes might be (additional) drivers of the summertime streamflow decreases (Jones, 2011): In the study area, alpine livestock farming is the main type of cultivation. The decline of this type of farming during the 1960s and 1970s

(Neudorfer et al., 2012) resulted in a still ongoing overgrowth of former grasslands, enhanced by climate-change related land-use changes like increases of the timber line (Walther, 2003).

The empirical-statistical model established in the present study was proven to simulate streamflow trends sufficiently well. Not only could it serve as a tool to gain deeper insight into the processes that cause streamflow trends, but it could also be used to derive streamflow trends in such alpine catchments, where only recently a gauge has been installed. T trends were found to be quite uniform over the entire study region, so a climate station that is very close to the watershed is not absolutely mandatory. The percentage of glaciated areas in the watershed can be derived via glacier cadastres or satellite imagery.

The analysis of hourly streamflow trends supports the findings of the earlier analysis and shows, that hourly resolved trend analyses can provide additional information on the changes in alpine streamflow. Most of the gauging stations with hourly measurements have only been installed since the eighties, so there has been hardly any research on the subdaily changes of streamflow and there might be potential for further research.

6. Summary and Conclusion

The present study analyses trends and its drivers of observed streamflow time series in alpine catchments, taking data from Western Austria as example. At first, trends of annual averages were analysed: It was found that streamflow at higher-altitude watersheds is generally increasing, while it is decreasing overall in lower-altitude watersheds. The following hypotheses are proposed: (1) positive trends at higher, glaciated watersheds are caused by increased glacial melt, (2) negative trends at lower, non-glaciated watersheds are caused by the hydrological effects of rising temperatures such as less snowfall causing higher infiltration and in particular increasing ET, and (3) many of the trends at watersheds in mid-altitudes are not identified, because positive and negative trends cancel each other out and the final annual trend is too small to be detected. To support these hypotheses, we attempted to attribute the trends, i.e. we tried to identify the processes that cause the trends.

The biggest challenge in streamflow trend attribution is that streamflow measured at one gauge integrates multiple processes all over the catchment area. This makes the identification of individual drivers difficult as the final streamflow signal is a result of multiple processes where upward and downward trends could balance each other out. The problem applies for many trend analyses in the literature, where trends are calculated from averages over a certain period of time. Therefore, daily resolution streamflow trends are derived, as they allow for a more precise temporal localisation of the trends. The DOYs of these trends are then compared to average DOYs of other hydroclimatological characteristics, such as the temperature surpassing the average freezing point in spring, or e.g. DOYs of trends in snow depth. The DOYs of these long-term characteristics fit well with the ones of the trends found in streamflow time series and thus can be related to them. Additionally, an empirical statistical model and analyses of the subdaily changes gave further hints for the causes of the streamflow changes in the study region.

With the present study, we have shown that the hydrological dynamics in alpine areas are changing significantly. Still, looking at the yearly averages of streamflow data, the ongoing change is masked by the fact that additional runoff caused by enhanced glacier melt and possibly increased precipitation is counter-balanced by modifications of the water cycle such as higher ET, less snowfall and rising infiltration in the vegetation season. These opposing forces may balance out within catchments comprising higher and lower altitudes, because the increased streamflow mainly prevails in higher areas while decreasing streamflow is mostly found in lower areas. We are confident that we have identified a rather robust trend of hydrological change in specific hydro-climatological regions, e.g. alpine catchments. Even though the changes are only partially identifiable when analysing yearly averages, they can clearly be seen when studying smaller time increments. This detailed analysis of high-resolution hydrological time series follows Merz et al. (2012b), who called for a more rigorous data analysis in order to analyse possible hydrological changes. The identified altered hydrological dynamics in the case of the alpine catchments is driven mostly by temperature increases. This supports Bronstert et al., 2007, who concluded that temperature increases, rather than precipitation changes, cause hydrological changes which may be quite robustly detectable. A trend attribution of this kind is an important step towards a scientifically sound assessment of climate change impacts on hydrology. A proceeding step should be the process-based modeling of such hydrological systems (Bronstert et al., 2009), which – in case the detected trends can be replicated by the model results – can further sustain the findings concerning climate effects on alpine hydrological systems.

Our attribution approaches could possibly be applied to regions other than mountainous areas. However, one must be aware that results might be rather different and/or less well identifiable if changes are not as strongly temperature-driven as those in mountain regions. However, as stated above, hydrological trend studies should attempt to not only detect but also attribute the trends. For this reason, it is worth looking for attribution methods adapted to the particular local condition. In any case, daily resolved trends are helpful to detect and attribute hydrological regime changes in alpine catchments, which could be overseen by annual or trimonthly trend assessment.

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Appendix

A.1 Schematic illustration on the structure of the analyses

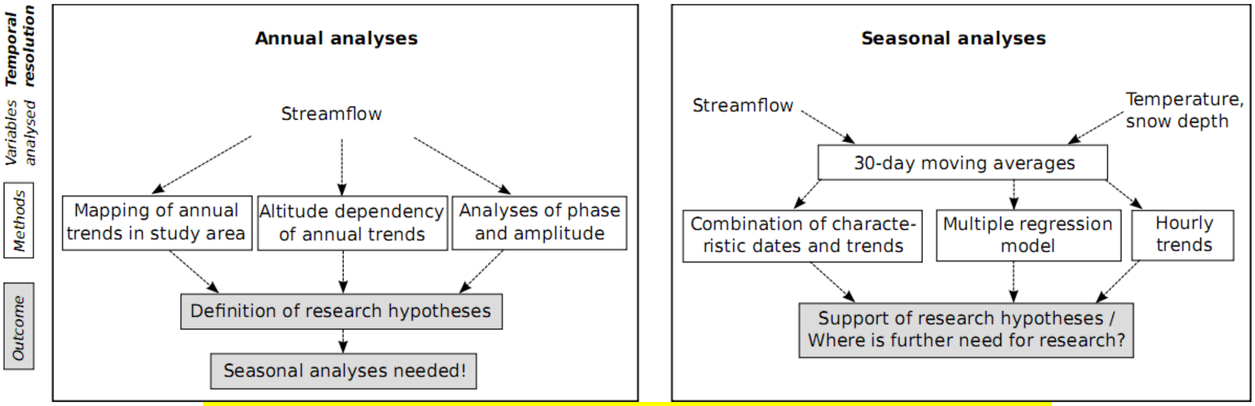


Figure A.1: Schematic illustration on the structure of the analyses.

A.2 List of symbols and abbreviations

Symbol	Unit	Property
α	-	significance level
α_{local}	-	local significance level
α_{field}	-	field significance level
Δ	var. units/year	trend
$\Delta Q_{\overline{year}}$	mm/year	trend of annual Q means
ΔQ_{30DMA}	mm/year	trend of *30DMA* Q means, for certain *DOY* at certain station
$\overline{\Delta T_{min}}$	°C per year	mean trend in T_{min}, averaged over all stations, for certain DOY
Δ_{MD}	var. units/year	minimal detectable trend
σ_X	variable units	standard deviation
30DMA	variable units	30-day moving averages
A_{ice}/A_{tot}	%	Percentage of glaciated area in the watershed
DOY	-	day of year

$\overline{DOY}$	-	characteristic date (average *DOY* of a certain event)
$\overline{DOY_{0°TmeanSpring}}$	-	average *DOY*, when T_{mean} crosses 0 °C in spring (1980-2010)
$\overline{DOY_{Qmax}}$	-	average *DOY*, when annual *Q* maximum occurs (1980-2010)
$\overline{DOY_{SDmax}}$	-	average *DOY*, when annual *SD* maximum occurs (1980-2010)
ET	mm	evapotranspiration
Q	mm	specific runoff
$Q_{\overline{year}}$	mm	annual Q mean
$Q_{30\mathrm{DMA}}$	mm	*30DMA Q* for certain *DOY*
$\overline{Q_{30\mathrm{DMA}}}$	mm	*30DMA Q*, averaged for 1980-2010, for certain *DOY*
$\overline{\dot{Q}_{30\mathrm{DMA}}}$	mm	first derivative of $\overline{Q_{30\mathrm{DMA}}}$
SD	cm	snow depths
S/N	-	signal-to-noise ratio
T_{max}	°C	daily maximum temperature
T_{mean}	°C	daily mean temperature
T_{min}	°C	daily minimum temperature
R	-	record length

Table 1: List of the gauging stations used in this study (sorted by mean altitude) and their characteristics.

Station ID	Station name (and ID of nested basin)	Altitude (m)	Latitude	Longitude	Gauged Area (km²)	Mean basin alt. (m)	Glacier coverage (%)
1	*Vernagt*	2640	46.8678	10.8007	11	3127	71.9
2	*Vent* (1)	1891	46.8665	10.8895	90	2934	33.0
3	*Gepatschalm*	1895	46.9112	10.7142	55	2880	39.3
4	*Obergurgl*	1883	46.8717	10.9998	73	2849	28.2
5	*Huben* (1, 2, 4)	1186	47.0508	10.9598	517	2700	15.7
6	*St. Leonhard*	1337	47.0796	10.8312	167	2613	15.5
7	*Hinterbichl*	1321	47.0026	12.3380	107	2600	14.3
8	*Innergschlöß*	1687	47.1099	12.4551	39	2590	29.4
9	*Tumpen* (1, 2, 4, 5)	924	47.1707	10.9031	786	2579	11.8
10	*Ritzenried* (6)	1095	47.1329	10.7711	220	2544	13.2
11	*Neukaser*	1824	47.0225	11.6877	24	2499	9.6
12	*Tauernhaus* (8)	1504	47.1037	12.4990	60	2474	19.4
13	*Spöttling*	1486	47.0106	12.6358	47	2473	10.6
14	*Kühtai*	1902	47.2124	10.9994	9	2448	0.0
15	*Galtür-Au*	1544	46.9988	10.1747	98	2411	5.7
16	*Waier* (7)	931	46.9798	12.5290	285	2376	8.4
17	*Sulzau*	882	47.2185	12.2508	81	2354	17.2
18	*Fundusalm*	1600	47.1492	10.8909	13	2336	0.0
19	*See i. P.*	1019	47.1051	10.4541	385	2303	1.6
20	*Habach*	880	47.2322	12.3276	45	2117	6.9
21	*Mallnitz*	1174	46.9661	13.1835	85	2081	0.6
22	*Steeg*	1113	47.2643	10.2867	248	1951	0.0
23	*Bad Hofgastein*	837	47.1456	13.1184	221	1937	1.3
24	*Haidbach*	888	47.2377	12.4921	75	1915	0.0
25	*Rauris*	917	47.2233	12.9999	242	1841	1.6
26	*Vorderhornbach* (22)	958	47.3842	10.5389	64	1726	0.0
27	*Hopfreben*	943	47.3144	10.0416	42	1701	0.0
28	*Wagrain*	849	47.3102	13.3112	91	1594	0.0
29	*Viehhofen*	861	47.3487	12.7448	151	1550	0.0
30	*Mellau* (27)	673	47.3881	9.8790	229	1494	0.0
31	*Laterns*	830	47.2956	9.7195	33	1475	0.0
32	*Ehrwald*	958	47.4150	10.9159	88	1467	0.0

Table 2: Pearson's *r* between annual streamflow trends and mean watershed altitude.

	Significant trends only	Insignificant trends only	Both
$\Delta Q_{\overline{year}}$, percent	0.84	0.54	0.68
$\Delta Q_{\overline{year}}$, absolute	0.81	0.65	0.62
ΔQ_{phase}	0.86	0.68	0.83
$\Delta Q_{amplitude}$	0.87	0.74	0.76

Fig. 1: Study area with meteorological stations, watershed boundaries, glaciers and trends of mean annual streamflow in percent change per year (period: 1980–2010; significance level: alpha=0.1). Station ID next to the triangles.

Fig. 2: Trend magnitude (percent and absolute values, resp.) versus station ID (sorted by rank of mean watershed altitude (1 = highest)).

Fig. 3: Seasonal distribution of daily streamflow trends (period: 1980–2010; significance level: alpha=0.1); **a)** 30DMA trend magnitude, only where significant trends are detected (dark blue if not significant); **b)** 30DMA trend magnitude, without assigning significance; white squares: average annual Q maxima; bar above upper diagram: pink-coloured if the 30-DMA trends are field-significant; bar on the right of upper diagram: pink-coloured if the *annual* streamflow trend of the corresponding station is significant.

Fig. 4: **a**) Station altitude vs. $\overline{DOY}$ of daily T_{mean} passing the freezing point in spring; **b**) same as **a**), but for autumn; **c**) station altitude vs. $\overline{DOY}$ of annual SD maximum; all graphs with the line of best fit and corresponding equation. DOYs are calculated as averages of the period 1980–2010.

Fig. 5: **a**) - **d**) Seasonal distribution of daily mean (a), minimum (b) and maximum (c) temperature, (d) snow depth trend magnitudes and e) streamflow trends (with characteristic dates) (1980–2010); bar above diagram: black-coloured if field significant.

Fig. 6: Scatterplot of predicted vs. observed streamflow trends in percent per year on the day considered.

Fig. 7: Seasonal distribution of hourly trend magnitudes (1985–2010); a) T at Vernagt; b) Q at Gepatschalm; c) Q at Obergurgl; d) Q at Tumpen.

Fig. 8: Long-term annual streamflow cycle of a) a higher-altitude watershed (Gepatschalm, 2880 m, 39.3 % glaciated), b) a mid-altitude, little glaciated watershed (See i. P., 2303 m, 1.6 % glaciated) and c) a lower-altitude, unglaciated watershed (Ehrwald, 1467 m), trends generated from the end point of the Sen's Slope Estimator (dashed line, similar to Déry et al., 2009) and potential causes. Long arrows correspond to strong drivers, short arrows to smaller ones.

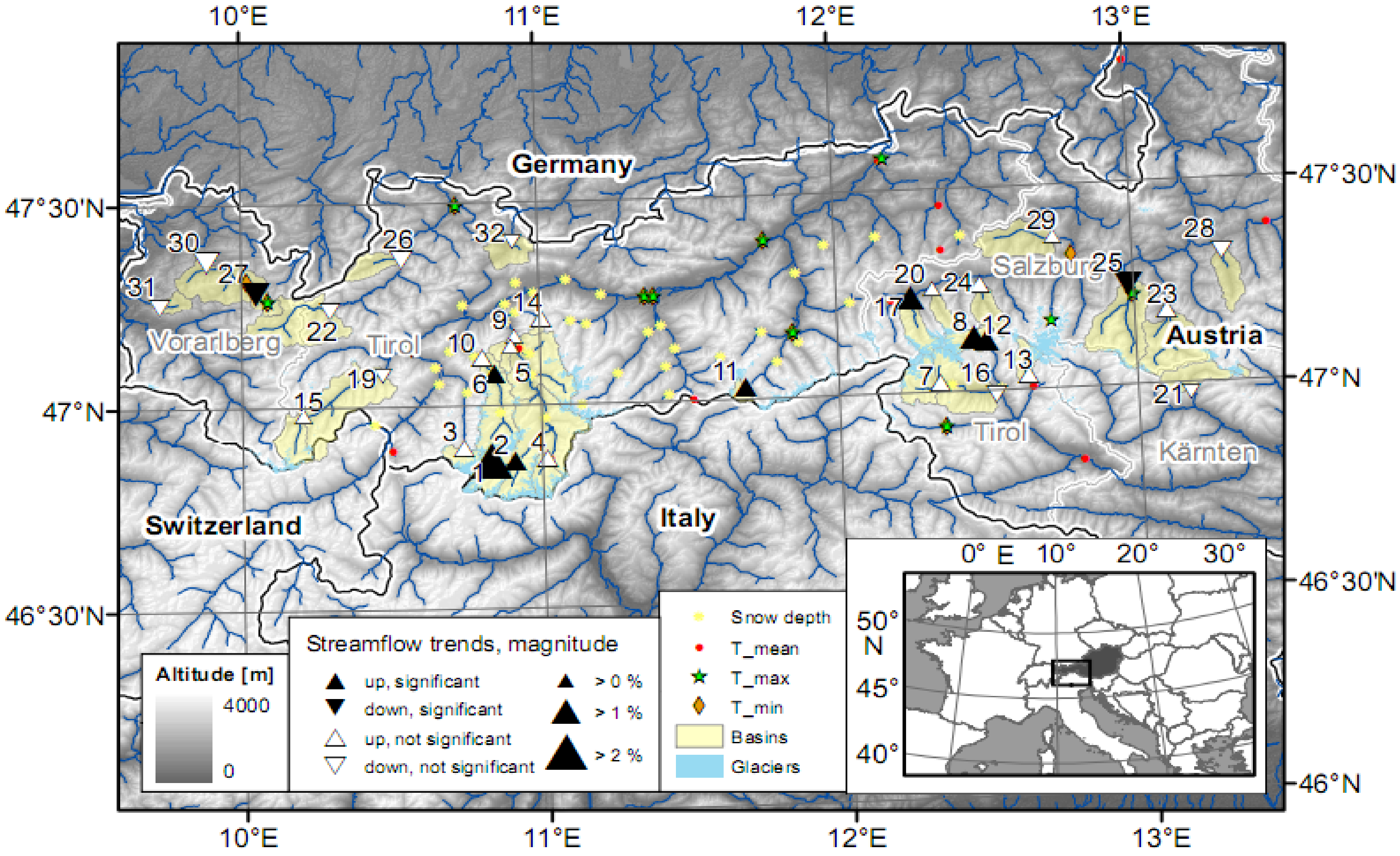

10°E
11°E
12°E
13°E
47°30'N
47°N
46°30'N
46°N
Germany
Austria
Switzerland
Italy
Vorarlberg
Tirol
Salzburg
Kärnten
1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
19
20
21
22
23
24
25
26
27
28
29
30
31
32
Altitude [m]
4000
0
Streamflow trends, magnitude
▲ up, significant
▼ down, significant
△ up, not significant
▽ down, not significant
> 0 %
> 1 %
> 2 %
Snow depth
T_mean
T_max
T_min
Basins
Glaciers
0° E
10°
20°
30°
50° N
45°
40°

Fig. 1

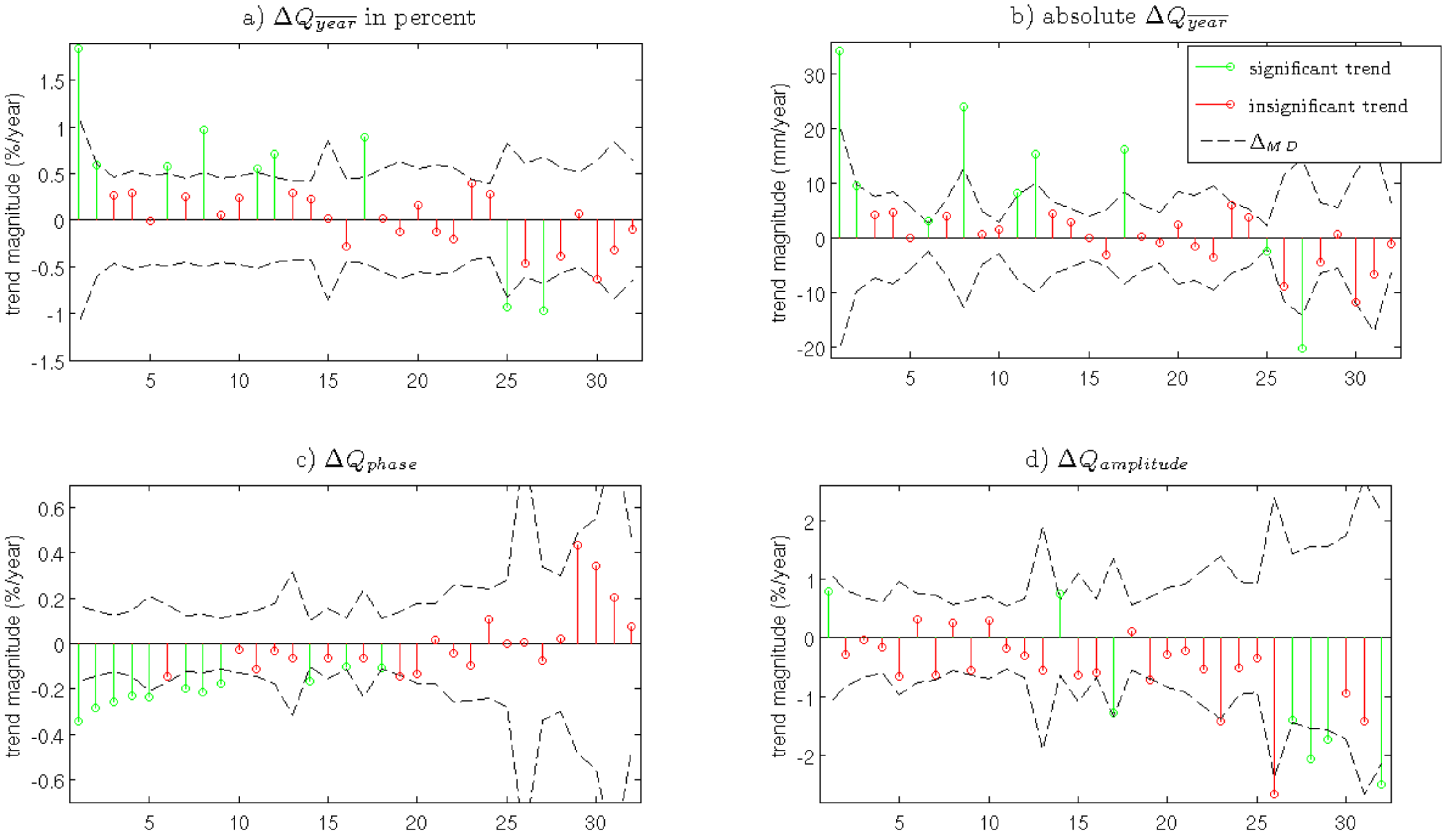

a) $\Delta Q_{\overline{year}}$ in percent
trend magnitude (%/year)
1.5
1
0.5
0
-0.5
-1
-1.5
5
10
15
20
25
30
b) absolute $\Delta Q_{\overline{year}}$
trend magnitude (mm/year)
30
20
10
0
-10
-20
significant trend
insignificant trend
Δ_{MD}
c) ΔQ_{phase}
trend magnitude (%/year)
0.6
0.4
0.2
0
-0.2
-0.4
-0.6
d) $\Delta Q_{amplitude}$
trend magnitude (%/year)
2
1
0
-1
-2

Fig. 2

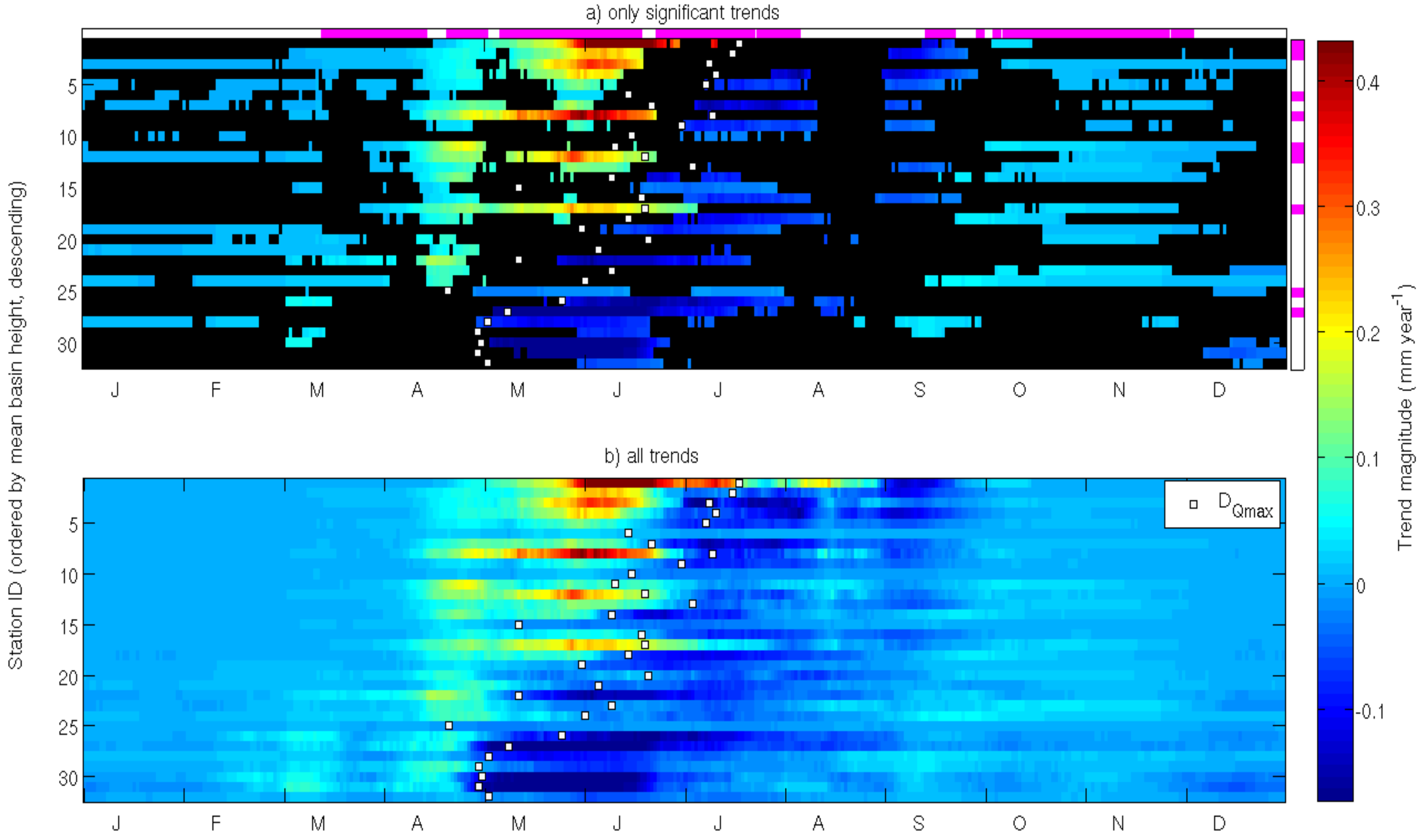

a) only significant trends
b) all trends
Station ID (ordered by mean basin height, descending)
5
10
15
20
25
30
J
F
M
A
M
J
J
A
S
O
N
D
D_{Qmax}
Trend magnitude (mm year^{-1})
0.4
0.3
0.2
0.1
0
-0.1

Fig. 3

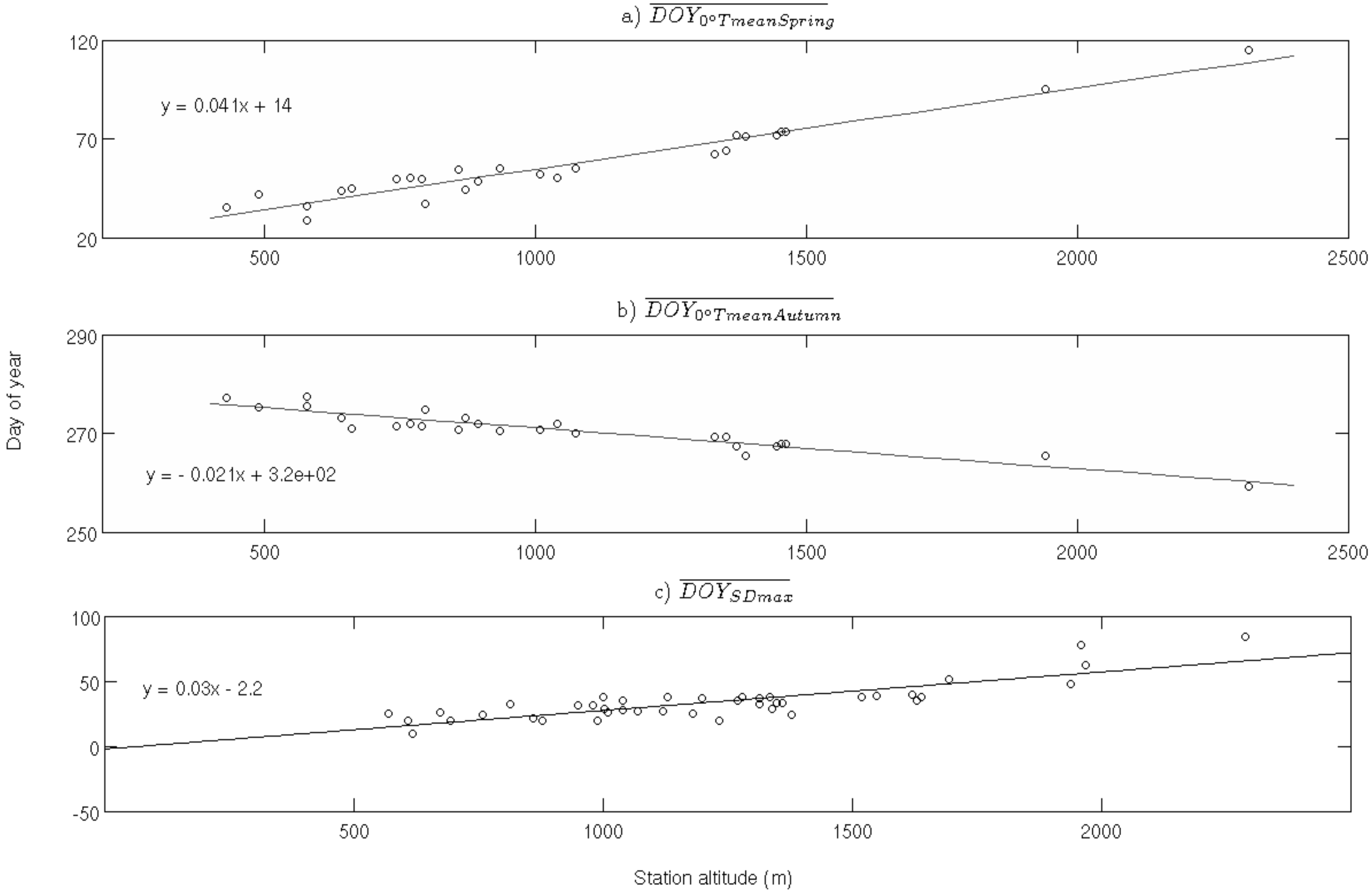

a) $\overline{DOY_{0°TmeanSpring}}$
120
70
20
y = 0.041x + 14
500
1000
1500
2000
2500
b) $\overline{DOY_{0°TmeanAutumn}}$
290
270
250
y = - 0.021x + 3.2e+02
500
1000
1500
2000
2500
Day of year
c) $\overline{DOY_{SDmax}}$
100
50
0
-50
y = 0.03x - 2.2
500
1000
1500
2000
Station altitude (m)

Fig. 4

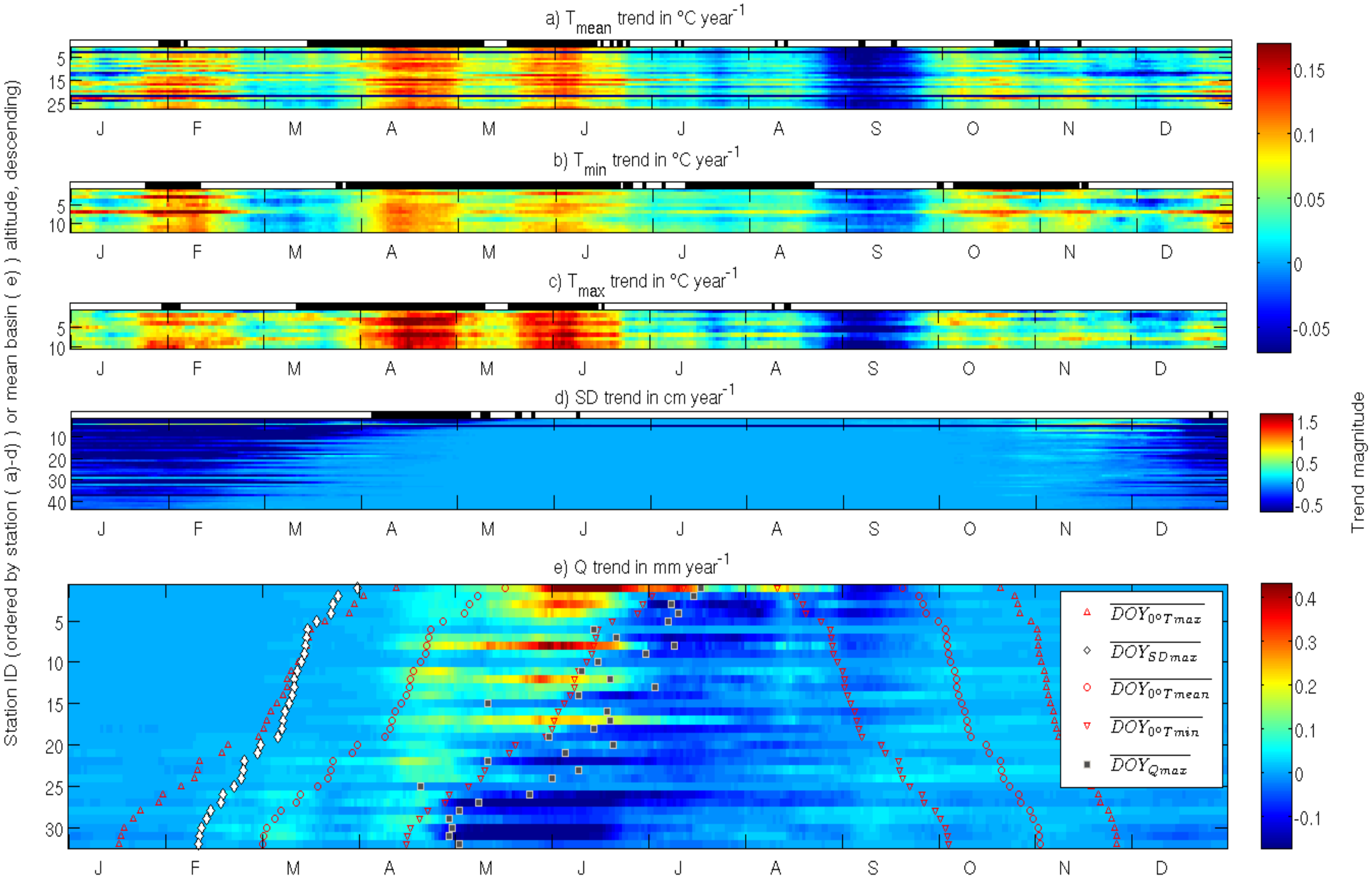

a) T_{mean} trend in °C $year^{-1}$
b) T_{min} trend in °C $year^{-1}$
c) T_{max} trend in °C $year^{-1}$
d) SD trend in cm $year^{-1}$
e) Q trend in mm $year^{-1}$
Station ID (ordered by station (a)-d)) or mean basin (e)) altitude, descending)
Trend magnitude
J F M A M J J A S O N D
5 15 25
5 10
5 10
10 20 30 40
5 10 15 20 25 30
0.15 0.1 0.05 0 -0.05
1.5 1 0.5 0 -0.5
0.4 0.3 0.2 0.1 0 -0.1
$\overline{DOY_{0°T max}}$
$\overline{DOY_{SD max}}$
$\overline{DOY_{0°T mean}}$
$\overline{DOY_{0°T min}}$
$\overline{DOY_{Q max}}$

Fig. 5

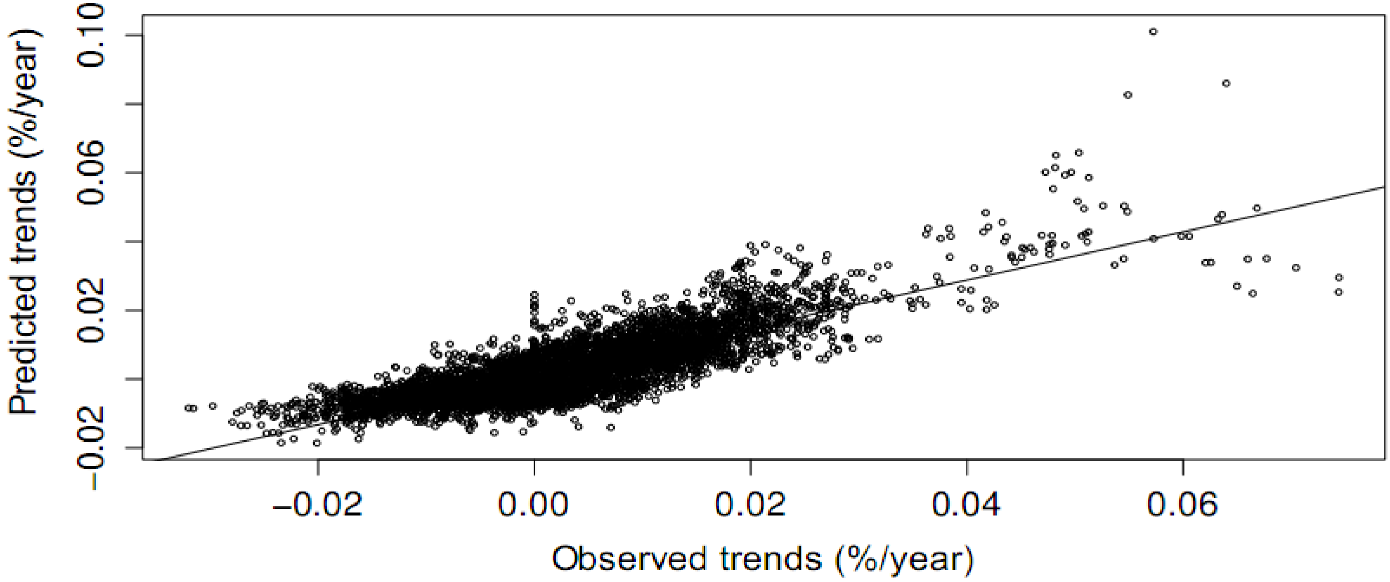

Predicted trends (%/year)
−0.02
0.02
0.06
0.10
−0.02
0.00
0.02
0.04
0.06
Observed trends (%/year)

Fig. 6

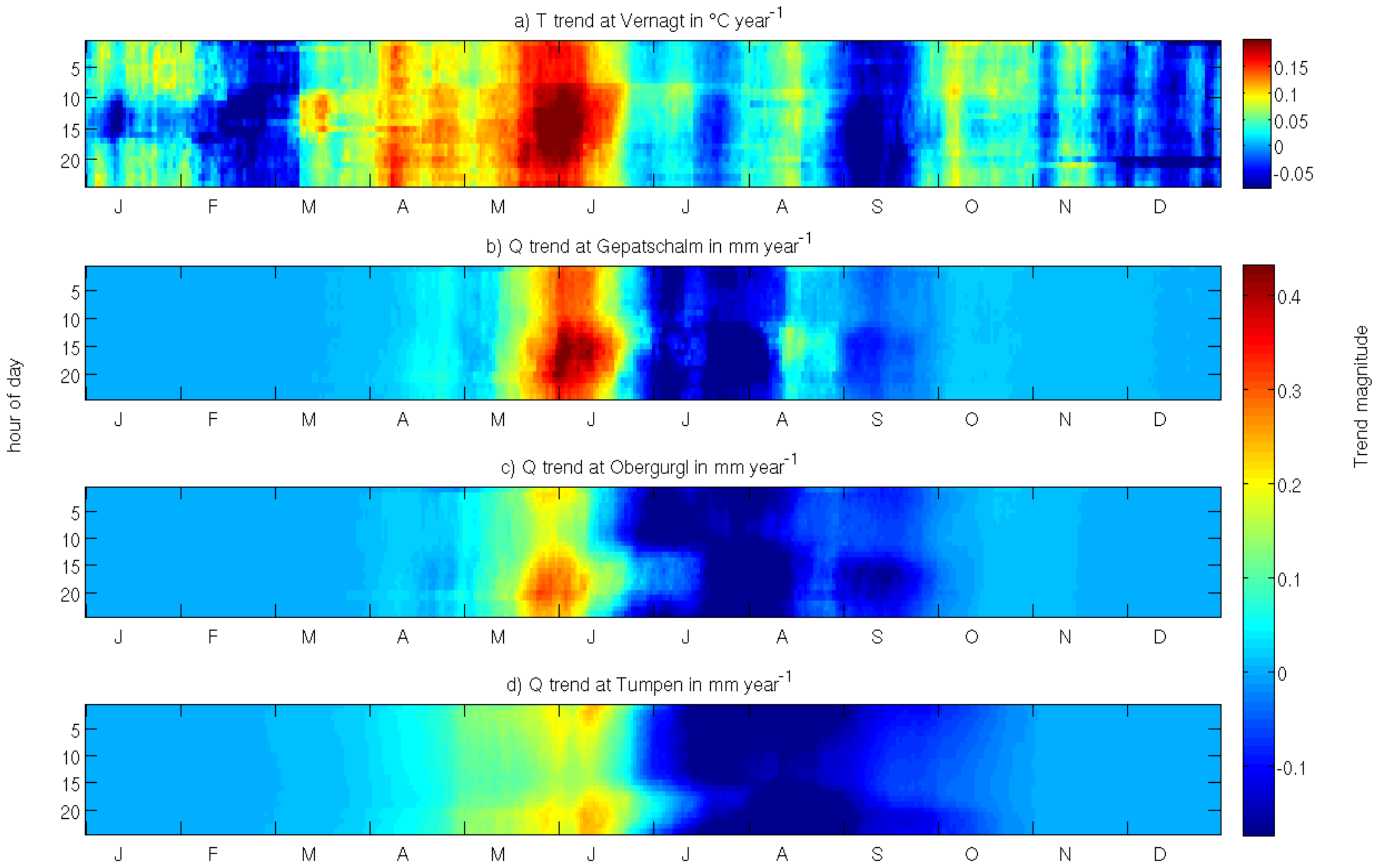
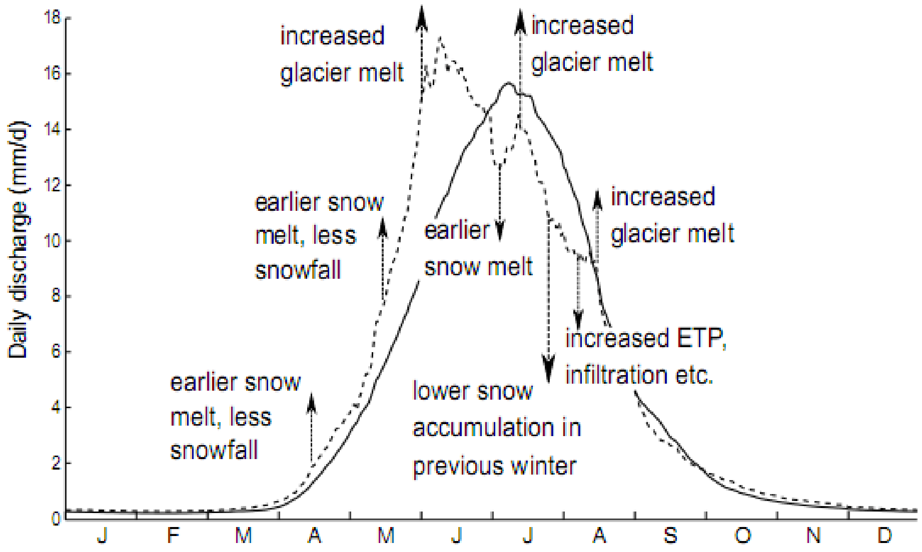

a) T trend at Vernagt in °C year^{-1}
b) Q trend at Gepatschalm in mm year^{-1}
c) Q trend at Obergurgl in mm year^{-1}
d) Q trend at Tumpen in mm year^{-1}
hour of day
5
10
15
20
J
F
M
A
M
J
J
A
S
O
N
D
0.15
0.1
0.05
0
-0.05
0.4
0.3
0.2
0.1
0
-0.1
Trend magnitude

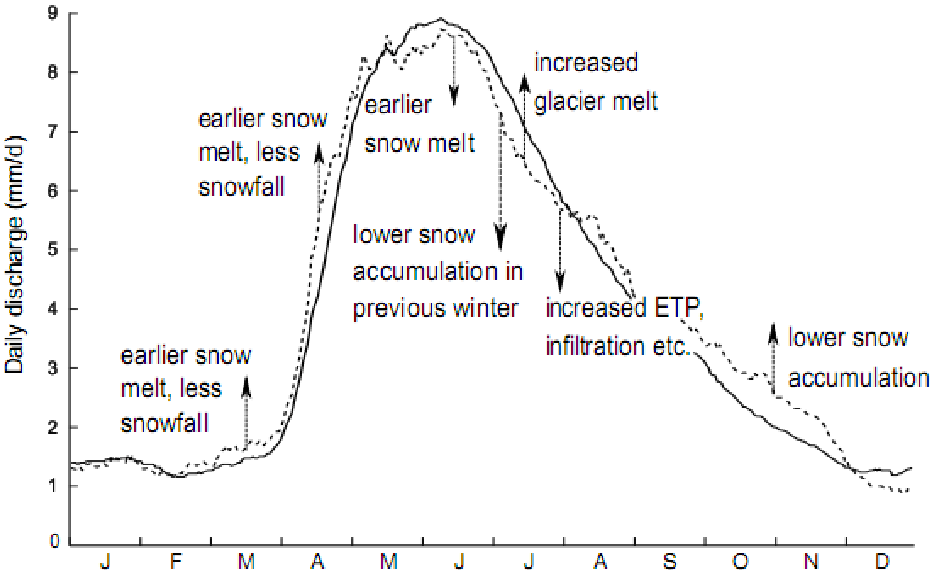
Fig. 7

Fig. 8

a) higher-altitude, highly glaciated watershed



b) mid-altitude, little glaciated watershed



c) lower-altitude, unglaciated watershed