Dear Dr. Parajka,

we gratefully acknowledge your comment on "Attribution of high resolution streamflow trends in Western Austria – an approach based on climate and discharge station data" and thank you for providing very valuable suggestions that will help to improve the quality of the manuscript. In the following, we will address and reply to each of your comments. We will include the necessary changes in the revised version of the manuscript.
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). The referee comment is written in blue colour, our answers to the comments are written in black.

On behalf of all co-authors,

Christoph Kormann

This manuscript analyses and attributes the annual and daily/subdaily streamflow trends in 32 alpine catchments of Western Austria. Results indicate that there is no consistent significant regional trend in annual streamflow. The significant increasing and decreasing trend is found in 7 and 2 basins, respectively. In addition to a classical trend evaluation, daily resolution streamflow trends are derived and linked with the trends (exact day of year) of other hydroclimatological characteristics, such as air temperature and snow height. These results indicate that the main drivers of alpine streamflow changes are increased glacial melt and earlier snow melt. The authors conclude that some further research is needed, which will explicitly determine which processes are related to the summertime streamflow decreases.
Overall, the study is interesting and fits well within the scope of the journal. I enjoy reading it, and like, in particular, the analyses and evaluations based on observed data. However I have also some critical comments which need to be considered before the publication. Statistical assessment of trends and their attribution is interesting (and needed), however, it does not allow a fully robust causal interpretation of hydrological processes (in physical sense). Some interpretations/statements used in this paper are not fully precise and do not consider this feature of statistical assessment. So, I would suggest to consider carefully revising some statements/interpretations made. In particular, following points need to be considered:

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 think the word "*confirm*" is somehow problematic, when talking about hypotheses. It can be used in the sense, that hypotheses are *supported.* Or it can be used in the sense, that hypotheses are *proven true* (*verified*), which is much stronger than the first meaning.
We intended to use "*confirm*" in the sense of "*support*". We agree, that "*prove true*" would be too strong in this context. We will correct and clarify as you proposed.

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 will consider in the revised manuscript. We agree, the sentence is not precise and we will change it accordingly. We intended to point out that there are not many *regional trend assessments* and only few *based on highly resolved data in time* (on a daily basis instead of monthly or seasonal) but mostly trend studies that cover whole countries/continents or the Greater Alpine Region as a whole. We will revise the literature and provide a review on where Western Austria was part of the study area in trend assessments.

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. 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.

We agree, that this might be confusing. Maybe the term "lower-altitude" instead of "low-altitude" stations would be more appropriate.
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 include 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.

Reviewer Dr. Birsan also pointed out the missing discussion. In the actual manuscript version we intented to include this into the results section to keep the manuscript short. The problem is that some of the results (detected trends) were already discussed in the earlier paper (Kormann et al., 2014) and there has not been much other literature on trend attribution, especially not on daily trends. However, we will consider this point and add a separate discussion section in the revised version of the manuscript.

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 a lot for this comment. We will plot “real” hydrographs instead of the schematic illustration.

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 will consider it in the revised manuscript.

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 discharge stations were carefully checked beforehand on whether there was any influence of hydropower on the discharge quantities (Each gauge that is influenced by hydropower is marked by Austrian government authorities. See http://ehyd.gv.at/). Additionally we checked for inhomogeneities in the 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. 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 equipped with 1-day water storage volumes, which means there might be 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 will clarify this and rewrite the according section in the revised version of the manuscript.
Concerning the ice effects on discharge measurements in winter, 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 ...

Thanks for this comment. We will add a better explanation. 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, we found that 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 rates (During spring snowmelt, the soil is generally saturated in a very short time and is not able to hold the excess snowmelt 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 and will better clarify it in the revised version of the manuscript:
http://www.hydrol-earth-syst-sci-discuss.net/11/C2850/2014/hessd-11-C2850-2014- supplement.pdf

References:

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

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, doi: 10.2166/wcc.2014.099, 2014.