

Interactive comment on “Attribution of high resolution streamflow trends in Western Austria – an approach based on climate and discharge station data” by C. Kormann et al.

Anonymous Referee #3

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General comments:

The paper of Kormann et al. addresses a relevant scientific question within the scope of HESS. It analyzes streamflow trends in an alpine region and attempts to explain to which extent the observed changes are caused by changes in climate variables. The intelligent combination of different methods and process understanding allows the authors to formulate and support hypotheses. For example, the argument that annual trend analyses may not be informative due to the integration of counteracting processes within the annual period is convincing and supported by their results. I also appreciate very much the efforts to introduce process understanding in the design of the study and

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in the analysis of the results. Very illustrative are also Figures 8 and 9, schematically summarizing streamflow changes and the associated drivers.

The paper is a substantial contribution to answering the question of hydrological change in mountain areas. Its novelty lies particularly in the smart combination of different methods. It addresses detection and attribution of change at the same time and advance further than many other papers on hydrological change. Overall, I am very positive and recommend publication in hess.

Besides a number of specific and technical comments, I have the following major criticisms. 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. (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. (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.

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,

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please provide an explanation.

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.

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?

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? 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'?

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?

p6889-Equation 1: I do not understand equation 1 and feel that the explanation of MDT

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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?

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

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?

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.

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 signifi-

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cant - 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?

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

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.

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

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

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

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.

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 field-significant 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.

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

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signal-to-noise ratio?

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 a 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. ..."

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.

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

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

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