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Hydrology and Earth System Sciences
Discussions

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 #4

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I side with my predecessors to commend the authors for their detailed analysis and attempt at attribution. The study is certainly timely and in the scope of HESS. However, the manuscript still requires considerable work to improve writing and scientific rigor in all aspects and in all sections, as well as resolving the relation to the Kormann 2013 paper, before it can be re-considered for publication.

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).

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.

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.

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.

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. 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.

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

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.

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

Detailed comments:

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

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.

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

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

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.

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.

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.

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.

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.

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.

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.

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!

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

22) 6890 line 22 where they taken from Kormann 2013? Or really calculated following ....? Earlier it says that there 'only 30day means were looked at. Very confusing and needs to be clarified.

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

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)?

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.

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. Hypotheses are falsified, not verified.

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?

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.

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

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.

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.

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

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

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

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

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

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.

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

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

References

Bard, A., Renard, B., and Lang, M.: The AdaptAlp Dataset. Description, guidance and analyses, Final Report, UR HHLY,Hydrology-Hydraulics, Lyon, 15 pp., 2011.

G. Blöschl, A. Viglione, R. Merz, J. Parajka, J. L. Salinas, W. Schöner 2011 ÖWAW 1-2, 2011

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6881, 2014.