Many thanks to referee #4 for his/her detailed review report, that will certainly help to improve the overall quality of the manuscript under review. In the following, we will address and reply to each of his/her 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.

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).

Thanks for the comment. We will address all of the issues in the specific comments 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.

The structure of the introduction is
1. observed climate change in mountains
2. observed hydrological impacts of these changes, i.e. trends
3. possible drivers of these trends
4. trend attribution as such, compared to trend detection
5. our way of dealing with trend attribution in this paper
6. information on the first paper about the detection of daily trends (which is necessary somewhere in the manuscript)

The first part of this order is actually similar to the first proposition of the reviewer:
- "known trends": → 1. and 2.
- "hypotheses for attributed processes": → 3.
- "attribution knowledge gaps": → 3. and 4.
We will try to improve the readability as good as possible and add further literature, especially on the last point proposed by the referee.

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. We will include the technical report by Bard et al. (2011) into the introduction section of the revised manuscript. Indeed they found coherent trend patterns, that other studies often did not find. However, the “analyses carried out in this study do not establish a formal link between the detected trends and climate change: this would require an additional attribution study“ (Bard et al., 2011).
The ÖWAW report is only available in German (please correct if this is wrong), which makes it less accessible for non-German-speaking readers. Nevertheless, we agree it is worth to be cited here. However, one should see that it only fits partly into the topic covered here, as it treats high- and low flows, and in our study, we explicitly look at the change in mean values.
Concerning streamflow changes, the review paper of Iris Stewart does not report about *observed streamflow trends* in the *European Alps*. It only refers to studies that were analysing *projected* streamflow changes via modelling approaches (cf. p84 and Table II (although the caption says something different, only snow depth changes are reported for the *European Alps*) in Stewart, 2008). This reflects the fact that there are basically only few studies for the European Alps with *coherent results* in *streamflow trend analyses* (for Western US, this is different, as also presented in Stewart, 2008). In our introduction, we intended to explicitly point out this lack in the literature.
Concerning the field of *trend attribution*, we will include some of the points mentioned in Stewart et al. 2008. However, many studies mentioned (e.g. Barnett et al., 2008) attribute observed trends via modelling approaches and we prefer not to include those studies into the review, as our methodology is based on observed station data only.
Anyway we will try to improve the review section of the introduction as good as possible.

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.

Thanks for this comment. We only looked at results that are in direct relation with our study. We agree, that this approach can cause issues as certain information is lost. However, it is probably impossible to give a complete overview and short summaries of all relevant publications and to say which publications are more important than others. Already now, the introduction section of the manuscript spans three pages, so we have to somehow limit the information contained there.

Specific comment on Whitfield (2013) and Déry et al. (2009):
The question on whether to use the COV or not is not a main issue of our paper. For this reason, this section is kept very short. We intended to reflect that the studies using measures such as COV should be taken with care, as other studies discourage their use:
Whitfield (2013) write in the abstract (elsewhere in their paper similarly): "...the use of COV as an indicator of snowmelt timing should be avoided. "
Déry et al. (2009) state in their conclusion "Assessing phase shifts in river runoff using either the day of occurrence of the annual peak flow or the center of volume may lead to inconclusive or misleading results..."
Based on these statements and the reasons given in the studies, we wrote "newer studies like Déry et al. (2009) and Whitfield (2013) claim that these metrics should be avoided, because they are sensitive to other factors such as record length, streamflow seasonality or data variability."
We plan to elaborate on this point more in detail in a revised version of this manuscript.

Specific comment on Stahl and Moore (2006):
We partly disagree, that our reference is wrong: In the conclusion section, Stahl and Moore (2006) write "Analysis of August streamflow in British Columbia, Canada, revealed *significant negative trends* for glacier-fed streams, both with and without corrections for interannual variability in climatic forcing." In Figure 2 of this paper ("Caption: Relation between trend in annual August streamflow and glacier cover") the significant negative trends are only found for watersheds with a glacier cover that is smaller than roughly 25 %.
Based on these facts, we wrote: "Stahl and Moore (2006) interpreted decreasing August streamflow in Canadian watersheds with less than 25 % glacier coverage as a sign of decreasing meltwater volumes." We agree, that the word "interpreted" was not precise enough as they added a model experiment to attribute the trends to glacial influences. We will give more consideration on this in order to avoid any confusion on this topic.

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 will change the manuscript accordingly. The problem is that some of the approaches that we applied are not standard methods (the whole issue of *trend timing*), so this has to be mentioned somewhere.

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 will cite the objectives from the manuscript in order to address this point:

“(1) to explain the spatially incoherent streamflow trends in Alpine regions based on *annual sums*;” The first objective refers to the incoherent *annual* trends that often have been reported in trend studies in the Alps (in the answer to the review report of Dr. Birsan we present some references on this issue).

“(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.“
With the first point of the two we intended to point out the following: During a trend attribution process in hydrology, first, possible drivers have to be identified and hypotheses have to be formulated (2). Later, certain arguments have to be found to support (or falsify) these theories (3). We agree that the last two points appear similar and we will clarify.

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 again want to point out that this manuscript is partly based on the results presented in the first paper, and therefore one needs to find a compromise between not to repeat parts of the first paper and producing a “stand-alone” paper. However, we agree (as the referee proposes), that it is nicer for the reader of the present paper to have the essence of the earlier paper available here. Therefore we will try to integrate the reference better into the introduction.

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.

This issue had been addressed already by other referees. 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. 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, ~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 (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 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 will clarify this and rewrite the according section in the revised version of the manuscript.

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

Referee Dr. Birsan also pointed out the issue of nested catchments. Eight of the catchments analysed are nested. We used the approach that was applied in Birsan et al. (2005): To assure spatial independence of the station data, we checked for "a substantial increase in drainage area between the stations". 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 (29.4 and 19.4 percent, respectively) and the requirements of station independence were not violated too strongly, we left them in the dataset. Mistakenly we did not mention this. We will add this in the revised version of the manuscript.

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 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 will add more information.

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 intended to keep the manuscript short, thus we refer the reader to the earlier paper for the full station information, maps etc. Anyway, we will carefully revise and after consulting the co-authors, add more information where it is necessary.

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

Thanks for the comment, we will do so.

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

Reviewer #3 had similar concerns. 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).

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. This belongs to 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.

As we mentioned in the manuscript, the data availability is certainly an issue, but not the biggest one (we have several stations that date back until the 1950s). However, we do not see the necessity to analyse longer periods and the arguments presented in the paragraph are no lies or excuses.
As also pointed out in the answer to referee Dr. Birsan: One main argument for our selection is that we found very similar trends for all periods analysed in the first paper (1950/1960/1970/1980-2010), so there is no real need for analysing longer or more periods. We found, that the trend magnitude is strongest in all of the hydroclimatic variables when looking at the period 1980-2010.

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.

Abermann et al. (2009) refer only to the Ötztal Alps. However, we will change it for Abermann et al. (2011) who reported about mostly negative mass balances since 1980 for whole Austria: “Since 1980, only a few years (e.g. 1984 or 1989) interrupt the generally negative trend.” (Abermann et al., 2011)

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 will further improve the manuscript as the referee recommends (e.g. with a schematic illustration on the different methods used, see Appendix, or a restructuring of the Methods section). However, as already pointed out by Reviewer #3, the overall analysis was quite complex. Many different methods have been used in a combined approach. This is one of the main draws of our study, but also one of the problematic issues.

The subheadings of the methods section are the names of the methods that we applied. They are structured according to the variables analysed, which are the headings of the methods section:

At first, *annual features (trends and phases/amplitudes)* are analysed:	3.1
- The Mann–Kendall test and the Sen's Slope Estimator for trend detection	3.1.1
- Minimum detectability	3.1.2
- Fourier form models	3.1.3
Later, the *seasonal trends* (30-day-moving averages) are analysed:	3.2
- 30DMA trends and characteristic dates	3.2.1
- Multiple linear model	3.2.2
- Diurnal streamflow trends	3.2.3

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 will clarify as you proposed.

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 will change the paragraph accordingly.

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.

We will improve this.

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 will change it.

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 will change it to “partly calculated and partly taken from Kormann et al., 2013”.

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 will rephrase.

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. The serve as indicator, e.g. for the average timing in the year when snow melt 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.

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 intended to include the Discussion section in the Results section with the aim of having a shorter manuscript. However, we now plan to separate the results and discussion, i.e. add a separate discussion section, as the other referees also asked for this. We will move text that includes methods to the Methods section and improve overall readability and conciseness.

Furthermore, we will add further explanations of the structure of the Results section, to help the reader to follow the overall analysis:
First, we analysed <u>trends based on annually averaged streamflow</u>. For this purpose, three different approaches were used: (1) mapping of annual trends in the study area, (2) height dependency of the annual trends and (3) analyses of streamflow timing (by assessments of the phase and the amplitude). Based on the outcomes of this analyses, we defined research hypotheses.
To support these hypotheses, we derived seasonal trends, i.e. <u>trends in a daily resolution</u>, of not only streamflow but also temperature (mean, max, min) and snow depth. These seasonal trends were then further applied in other approaches: (1) a combination of characteristic dates and trends, (2) a multiple regression model for streamflow trends and (3) subdaily trends.

We will additionally add a schematic illustration (see Appendix).

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, it will be considered. We somehow first have to show the results of the *annual* streamflow trends to later define the hypotheses based on the results of these. In the literature, an explicit height dependence of *annual* streamflow trends has not been found so far.

Hypotheses are falsified, not verified.

With this statement, the referee probably points to the work of e.g. Popper (1998), who emphasizes falsifiability instead of the classical positivistical inductivism. 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?

We agree that our schematic illustration of trend drivers is not precise enough and we will change it accordingly.
However, 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 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.

Lastly, as we also pointed out in the manuscript, it is probably impossible to explicitely 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. We will further emphasize this in the manuscript.

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.”. We will add a sentence as well in the 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.

We agree that this issue can be improved, so we will change Fig. 5 and Fig. 7 accordingly. We actually always used the same colormap (blue colours for strong negative values, red ones for strong positive values). However, we will change it in the way as you proposed, so that the value of the colours will always have the same trend signs throughout the manuscript.

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 will further increase the size of the letters. Captions will be 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 agree with the reviewer. This point was also critisised by another referee. We will change 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.

These trends all belong to the highest station (ID 1, Vernagt) where discharge is still zero when the multiple regression model already predicts that there is a trend when there is no trend. This happens during earlier days of year, when all water in the watershed is still bound in snow or ice. We will explain this in a future manuscript version.

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 try to improve wording and terminology as you proposed.

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

We will correct.

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

We will clarify.

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

It relates to the paragraph above. We will change this.

35) 6893 line 2: why suddenly ' water yield' – previously you used 'annual sums'. Better

would be to have a variable named.

We will do so.

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.

With annual *totals*, we refer to both *sums* and *averages*. When analysing trends, one first *sums up* or *averages* the dataset under study over a certain period of time (annual, seasonal) and afterwards trends are calculated. However, we agree that this might be misleading and will change accordingly.

37) Commonly used is "snow depth" (not: snow height)

Thanks, we will change, although many publications use the term "snow height" as well.

38) It should be "basin/station elevation" (or "altitude", but definitely not 'height') – in-consistent use throughout

We will change accordingly.

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Appendix:

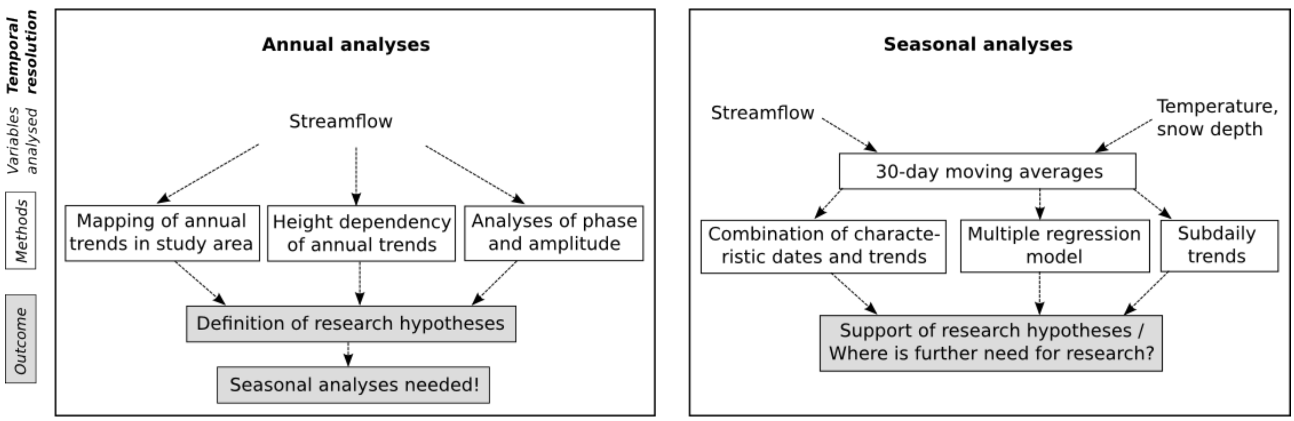

Figure 1: Schematic illustration on the approaches applied in the manuscript.