Dear Dr. Birsan,

thank you so much for your commitment as a referee, the precious comments and your extensive report. In the following, we want to scrupulously address and answer all of your comments and corrections and later include the necessary changes in the revised version of the manuscript. Excerpts of other publications are quoted in quotation marks. Referee comments are presented in blue, author comments in black. Sometimes we refer to the "first paper", which is our earlier publication on this subject (Kormann et al., 2014).

On behalf of all co-authors,

Christoph Kormann

The manuscript presents a trend analysis in hydroclimatic variables in Western Austria, and gives plausible explanations to changes in streamflow. The methods are good and plenty. I particularly liked the idea of trend timing, used in conjunction with other classical trend analysis methods. The topic is within the scope of HESS. But I have some serious criticisms concerning the manuscript. I refer to the main drawbacks of the manuscript in the "General comments" section (ranked by importance). I also have some specific comments that I would like the authors to address. However, I don't consider them mandatory except those related to the general comments. Some are mere suggestions, corrections, or things that might need better clarification. I consider that the paper needs a major revision, focusing on solving the main four criticisms mentioned below.

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.

We have addressed this point in the specific comments section below and will consider it in the revised version of the manuscript.

Point 3:
The streamflow data in particular have to be better described. Are the data series from independent basins? Is there any nested basin?

Yes, 8 of the 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 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 strongly, we left them in the dataset. Mistakenly we did not mention this. We will add this in the revised version of the manuscript.

A detailed map containing the river network...

With Figure 1, our aim was to reduce the total number of figures in the manuscript via including the *annual* trends in the map of the study area. We first included the river network and the watershed boundaries into this map but it felt overloaded, so we removed it again. The network is roughly evident via the valley bottoms that are shown in the map. However, we will anyway improve the map like proposed by the referee and include it into the revised manuscript. In the Appendix, a first version of the map of the study area with *annual* streamflow trends, river network, watersheds and climate stations is shown.

...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 will clarify this and rewrite the according section 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 will add this information as well in the revised version of the manuscript.

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

We intented 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 other referees also asked for this.

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 <u>about twice as large as the global trend</u> 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 will change 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 only few studies look at regional trends (and in a temporally high resolution). However, we reflected this statement and we will remove it in the revised version of the manuscript, as it is probably impossible to verify.

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.

We agree, that Viviroli et al. (2011) did not explicitly write about insignificant trends, but they pointed out the inconclusiveness of the trend signal in mountain regions: "...often lead to inconclusive or misleading findings" (Viviroli et al., 2011). We will correct the wording to avoid misunderstandings.
However, Pekarova et al. (2006) wrote: "Generally, we can comment that the trend analysis did not show any significant trends in cumulative runoff series of the major European rivers in last 150 years."

Further citations:
- "The majority of *annual* flow records in CE (remark: Central Europe) do not show significant trends, but spatially coherent trends in separate months have been reported." (Renner and Bernhofer, 2011)
- "The *annual* streamflow in the period 1976-2007 does not show significant trends for the majority of stations (81%)" for whole Austria, translated from Schimon et al. (2011), p. 4_13. The same is valid for the period 1950-2007.
- "The trend analysis of the long discharge time series (more than 180 years) of the large West/Central European rivers (Goeta, Rhine, Neman, Loire, Weser, Danube, Elbe, Oder, Vistule, Rhone, and Po) shows no significant trend of the *annual* mean river discharge." (Pekarova et al., 2003)

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 will change it accordingly in the manuscript.

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 will be 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?

Our statement is based on literature such as:
- "We have found no coherent regional effects in trend behaviour throughout Switzerland despite the different hydrological regimes to the North and South of the Alpine range." (Birsan et al., 2005)
- "Past trends in mountain runoff: Analysis of runoff trends over the historical record are very difficult for both mountainous and lowland areas. Results depend heavily on the methodology implemented and the timeframe of the study (...), and the high variability of precipitation and temperature often lead to inconclusive or misleading findings." (Viviroli et al., 2011)
- "...This heterogeneity of these trend signals is also highlighted by the recent analysis by Barben et al. (2010) for Switzerland." (Viviroli et al., 2011)
- For several, *mostly nationwide studies* in Europe, Stahl et al. (2010) state that "These studies all report considerable spatial variability in the changes detected in streamflow".

We want to point out that we refer only to trends of *annual sums* with this statement. Maybe we should better emphasize and clarify the role of the aggregation sum, i.e. if annual or seasonal trends were analysed.

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).

We thank the referee for this comment, we will consider this in the revised version.

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;

Thanks for the comment, we will clarify this. Basically, our interpretations are only valid for the study area. However, due to the physical processes that cause the trends, it is highly probable that similar results can be found in other Alpine regions as well.

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.

With the 2nd objective 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. Later, certain arguments have to be found to support (or falsify) these theories (3rd objective). We agree that the two points appear similar and we will clarify.

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

Thanks, this will be 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 will be corrected. With “robust” we meant that trend timing is a more stable measure than trend magnitude, as the magnitude is fluctuating much stronger. We will rewrite this sentence.

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. In the earlier paper, we did not find many significant trends. However, the high variability of precipitation data might mask possible changes (→ issue of minimum detectability, Morin, 2011). For this reason and the other reasons mentioned in the corresponding paragraph, we somehow had to assume this. We will rewrite this for better understanding.

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 will add a table or an extra section with further detailed information on the catchments used.

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, this will be clarified.

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. However, in the same section we gave reasons for analysing only the period 1980-2010:

According to Kundzewicz (2004), the probability of detecting change signals increases with intensifying climate change because the impacts may be greater and persist longer. Many studies that were published e.g. in the 1990s did not have the availability of sufficient data during a phase of a strongly changing climate, as was the case since the 1980s. Only during the 30 year span between 1980 to 2010 has temperature in the Greater Alpine Region increased by about 1.3 C, compared to about 0.7 C between 1900 and 1980 (Auer et al., 2007). In addition, glacier mass balances have been completely negative only since the 1980s (Abermann et al., 2009). In the 1970s, there were some years with positive glacier mass balances, which could obstruct the probability of detecting a clear change signal in the hydrological time series. For these reasons and also for reasons of data availability, the present analysis was carried out for the period 1980 to 2010.

Another argument for our selection is that we found very similar trends for all four periods analysed in the first paper, 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.
Furthermore, there are many publications that work with trend analyses of only 30 years or shorter (e.g. in Birsan et al. (2005), 30 years is the shortest period analysed, amongst other periods).

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 will correct 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 analysing monthly values, but with daily resolution. If we analyse daily trends, the high variability of the daily data will result in a low detectability, which is especially 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 not testing for trend significance, the 30-day averaging will help interpreting the trends, as the changes found are less fluctuating. For further information, we refer to the earlier paper. We will cite Kim and Jain (2010) as you proposed.

Tables and figures :

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

Thanks, we will do so.

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 will consider it if this does not 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 height, and not station height 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.

In the general comments section, we have responded already on this point.

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

Thanks for this comment, we will consider it.

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 will change it accordingly.

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

With “seasonal”, we intended to point out that this is a mean annual cycle that is shown. We will clarify this.

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 will plot real hydrographs instead of the schematic illustration. And yes, there is an increase in rain/snow ratio as well, so we will correct this.

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.

Auer, I., Böhm, R., Jurkovic, A., Lipa, W., Orlik, A., Potzmann, R., Schöner, W., Ungersböck, M., Matulla, C., Briffa, K., Jones, P. D., Efthymiadis, D., Brunetti, M., Nanni, T., Maugeri, M., Mercalli, L., Mestre, O., Moisselin, J. M., Begert, M., Müller-Westermeier, G., Kveton, V., Bochnicek, O., Stastny, P., Lapin, M., Szalai, S., Szentimrey, T., Cegnar, T., Dolinar, M., Gajic- Capka, M., Zaninovic, K., Majstorovic, Z., and Nieplova, E., 2007. HISTALP – Historical Instrumen- tal Climatological Surface Time Series of the Greater Alpine Region, Int. J. Climatol., 27, 17–46.

Kim, J.-S., and Jain, S., 2010. High-resolution streamflow trend analysis applicable to annual decision calendars: A western United States case study. Clim. Change 102, 3–4, 699– 707. doi:10.1007/s10584-010-9933-3

Kundzewicz, Z. W., 2004. Searching for changes in hydrological data, Hydrol. Sci. J., 49, 3–6.

Pekarova, P., Miklanek, P., Pekar, J., 2003. Spatial and temporal runoff oscillation analysis of the main rivers of the world during the 19th–20th centuries. J Hydrol 274: 62–79.

Pekarova, P., Miklanek, P., and Pekar, J., 2006. Long-term trends and runoff fluctuations of European rivers, in: Demuth, S., Gustard, A., Planos, E., Scatena, F., Servat, E. (Eds.) Climate Variability and Change – Hydrological Impacts, proceedings of the 5th FRIEND World Conference. IAHS, UK; 520–525, 2006.

Renner, M. and Bernhofer, C., 2011. Long term variability of the annual hydrological regime and sensitivity to temperature phase shifts in Saxony/Germany, Hydrol. Earth Syst. Sci., 15, 1819–1833, doi:10.5194/hess-15-1819-2011.

Schimon, W., Schöner, W., Böhm, R., Haslinger, K., Blöschl, G., Merz, R., Blaschke, A. P., Viglione, A., Parajka, J., Kroiß, H., Kreuzinger, N., Hörhan, T., 2011. Anpassungsstrategien an den Klimawandel für Österreichs Wasserwirtschaft. Bundesministerium für Land- und Forstwirtschaft, Umwelt und Wasserwirtschaft, Vienna, Austria.

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.

Appendix:

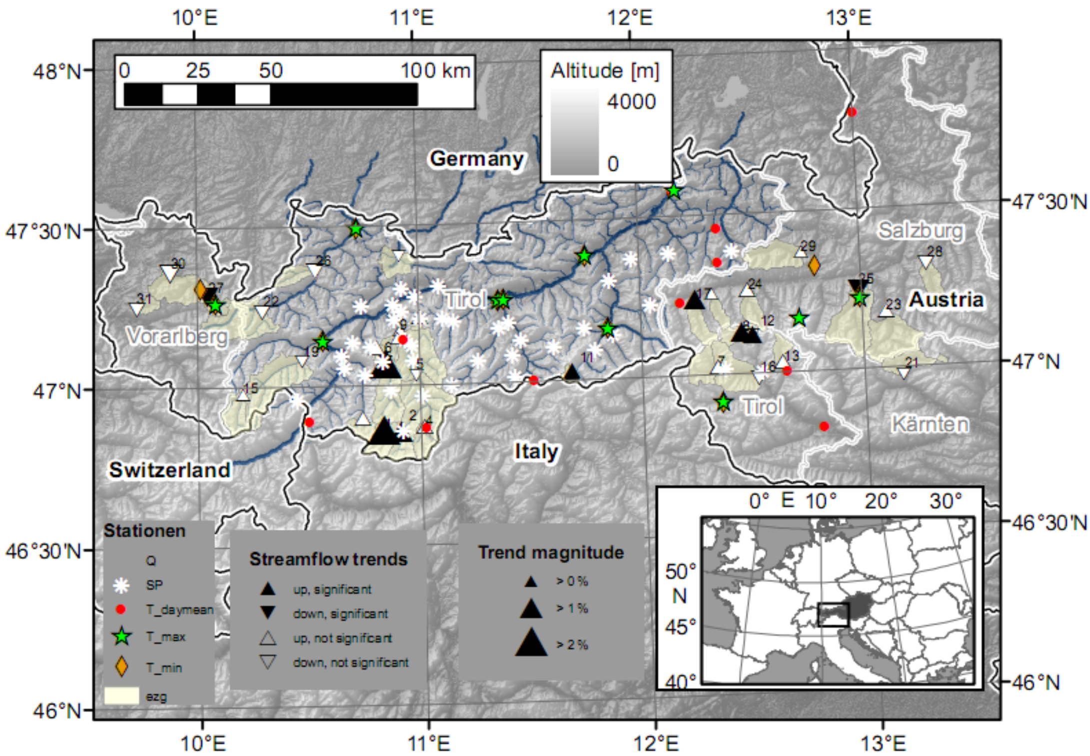

Figure 1: First version of a map of the study area with *annual* streamflow trends, watersheds and climate stations depicted. River network will be added for the whole area.