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Discussions

Interactive comment on "65-year changes of annual streamflow volumes across Europe with a focus on the Mediterranean basin" *by* Daniele Masseroni et al.

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COMMENT: The study analyses trends in annual streamflow over the period 1950-2015 in Europe. This is a relevant topic certainly within the scope of HESS. The study generally applies standard methods for trend analysis (Theil-Sen slope, Mann-Kendall test). The spatial patterns of the trends are compared to spatial patterns of air temperature and precipitation. The study extends previous work on observed streamflow trends in Europe by including a higher number of catchments, particularly in Portugal, Spain, France and Italy. This was possible through assembling the database of streamflow records from various sources. The results largely confirm previous studies

with dominant positive trends in northern Europe and dominant negative trends in the Mediterranean region.

REPLY: We thank Rev#2 for his/her comments and suggestions which allowed to improve the quality of the manuscript in this revised version.

COMMENT: Since the study states that records with missing data for more than two years were excluded from the database (L 107), I initially assumed that the calculated trends all relate to the period 1950-2015, which, looking at Fig. 2a, is apparently not the case.

REPLY: Fig.2a was deleted because it created misunderstanding both in Rev#1 and #2. The original dataset included about 3,900 stations and after the checks on reliability, consistency and uniformity of series of data, 428 stations were discarded. The 65-year study period (from 1950 to 2015) has been chosen and the optimal threshold between maximizing series length and avoinding missing data, as shown in new figure RC2.1.

COMMENT: This has of course a strong influence on the results and needs to be clarified. If the series length vary between catchments it will probably be more useful to analyze trends for different periods with nearly complete records, as the trends of course depend on the period analyzed (as discussed in the introduction).

REPLY: We agree with the Rev#2 about the influence of length of series of data on trend identification. Dixon et al. 2006 coped with this problem by splitting the dataset in time frames of different length, with a different number of stations for each period (see also Birsan et al. 2005). Nevertheless, one of the added values of our work was to consider a continuous dataset as large as possible over the entire study domain in order to evaluate spatial trends over European basins with a consistent sample size. It was the same approach proposed in the recent work by Durocher et al. (2019) where stations with missing data were discarded and a single time frame for all study domain was considered. In any case, we will compute trends for two subsequent time periods (1950-80 and 1980-2015) to check for effects of absolute dating in the available series.

Additional references Dixon, H., Lawler, D. M., Shamseldin, A. Y., & Webster, P. (2006). The effect of record length on the analysis of river flow trends in Wales and central England. IAHS-AISH publication, 490-495.

Birsan, M. V., Molnar, P., Burlando, P., & Pfaundler, M. (2005). Streamflow trends in Switzerland. Journal of hydrology, 314(1-4), 312-329.

Durocher, M., Requena, A. I., Burn, D. H., & Pellerin, J. (2019). Analysis of trends in annual streamflow to the Arctic Ocean. Hydrological processes, 33(7), 1143-1151.

COMMENT: The criteria for inclusion/exclusion from the database should be described very clearly. It is not so clear whether the study aimed at only including near natural catchments. How were gaps smaller than 2 years treated? The steps that were undertaken to exclude inhomogeneous series, or series strongly affected by human interventions need to be mentioned clearly. For example, did the authors try to get information from the data providers on human interventions such as changes in flow abstractions etc. It should be described clearly how the database was 'consolidated and validated'. Did you apply any automatic screening tests to systematically check the series for possible inhomogeneities?

REPLY: We thank the reviewer for raising this issue. In the revised manuscript we will add details about the pre-processing activity done to select the discharge time series used for the analysis. In particular, to ensure quality of discharge observations, the following steps were followed: 1) check on data availability; 2) check for outliers (i.e. five st.deviations higher or lower than the means; 3) check on the presence of inhomogeneities through automatic screening tests. In order to filter out catchments affected by human disturbance, each discharge time series was accurately scrutinized through visual hydrograph inspection to identify disturbed hydrographs due to e.g. the presence of dams/reservoirs. Discharge time series characterized by disturbed hydrographs were discarded from the analysis. It should be noted that most of the basins considered in the analysis are taken from the EWA

database, i.e. a discharge data collection of near-natural streamflow records from small catchments (Stahl et al, 2010). Moreover, the Global Reservoir and Dam (GRanD, https://sedac.ciesin.columbia.edu/data/set/grand-v1-dams-rev01) will be downloaded and a further analysis will be carried out in the revised manuscript to identify if (how many) dams/reservoirs are actually present in the selected basins. At the end of this analysis we expect that no substantial differences will be found between the basins retained for the analysis and the basins for which a certain degree of disturbance can be tolerated. Only stations with low human impact (no presence of dams/reservoir in the analysis period or no appreciable dam impact in the hydrograph); with less than 20% of missing data, showing no inhomogeneities in the time series were retained in the compiled dataset. Gaps smaller than two years were retained as missing data; during trend calculations, missing data were discarded on a case-by-case basis.

COMMENT: Some results are not very clear. The results section reports significant trends in 95% of the stations, which disagrees with results reported in Table 1. In the results section, it is not always clear whether results on trends also include non-significant trends.

REPLY: The number of basins reported in tab 1 (tab. 2 in the revised version of the manuscript) were incorrectly transcribed by the authors. They referred to the total number of stations in each macro-region (i.e. 3,485). In tab. 1 only significant positive or negative trends will now be shown. These were 95% of total gauged stations (i.e. 3310 stations). In the revised manuscript, the number of stations in each macro-region has been corrected. The manuscript will also clearly state whether any summary result includes non-significant trends.

COMMENT: I disagree with the finding of an inversion point in 1985 for the average series in the Mediterranean region. I do not see a change in the trend direction or trend slope in 1985. The fact that streamflow is above average before and below average after 1985 is a rather arbitrary result that depends on the selected study period. Streamflow has been decreasing since about 1965, and if anything, the rate of decrease has rather slowed down since the late 1980s.

REPLY: The reviewer is right. Figure 7 in the manuscript highlights that streamflow has been decreasing since about 1965 and the rate of decrease has rather slowed down since the late 1980s. In the revised manuscript the sentences related to Figure 7 will be modified accordingly and supported by new statistical trend analyses on the entire time period.

COMMENT: The calculation of the Sen's slope from annual streamflow anomalies is described as innovative, but if I do not overlook something this should not affect trends (and has probably been done in many studies).

REPLY: using anomalies to detect trends minimized absolute random error, but the reviewer is right in that it does not affect the trend (i.e., regression slope against time). Also, it is routinely carried out in both hydrologic and climate research. The methods section will be amended accordingly.

COMMENT: The introduction should be improved. The introduction should clearly convey what has been found previously on annual streamflow trends in Europe? What is the gap in the current literature? How is this approached by this study? Please also check the logic of individual sentences and the subdivision of the introduction into paragraphs.

REPLY: We thank the reviewer for this suggestion also underlined in the short comment by Adriaan Teuling. In the revised version of the manuscript, the introduction will include a more complete summary of what has been found by past studies on annual streamflow trends in Europe, what is missing in the current literature and in which way this study will fill the gap. The revised introduction will also rely on a more logical paragraphation.

COMMENT: The explanation of streamflow trends by trends in air temperature and precipitation remains a bit vague and overlooks areas where it is probably not possible

to explain streamflow trends with trends in air temperature or precipitation (such as positive streamflow trends in northern Spain). Some arguments need to be clarified e.g. it is not clear to me how groundwater or snowmelt effects would affect annual (and not only seasonal or monthly) streamflow.

REPLY: The discussion on groundwater and snowmelt roles will be improved, also specifying that it will rely on speculation and literature and not direct measure or testing of such variables. The cases in which the observed discrepancies between river discharge and weather series could be explained by based on logical and science-supported hypotheses using likely drivers, will be highlighted with their most relevant examples (eg Northern Spain).

COMMENT: P1, L28-30: The logic of the sentence is not clear. There is no contrast between a lot of research and not finding uniform streamflow trends in Europe. When mentioning a lot of research that aimed at investigating streamflow trends in Europe, this should be backed up by some references and their main findings (e.g. Stahl et al., 2010, Stahl et al., 2012).

REPLY: The introduction, and in particular the review of past studies and their findings, will be deeply improved in the revised version of the manuscript. References will be added, including those suggested by the reviewer.

Additional references: Stahl, Kerstin, et al. (2012). Filling the white space on maps of European runoff trends: estimates from a multi-model ensemble. Hydrology and Earth System Sciences 16.7: 2035-2047.

Stahl, K., Hisdal, H., Hannaford, J., Tallaksen, L., Van Lanen, H., Sauquet, E., ... & Jordar, J. (2010). Streamflow trends in Europe: evidence from a dataset of near-natural catchments. Hydrol. Earth Syst. Sci., 14, 2367–2382

COMMENT: P2, L33-34: Did these studies also analyze changes in annual streamflow volume? What were the main findings? How did seasonal streamflow change?

REPLY: As for the previous comment, the review of past studies and their findings will be deeply improved in the revised version of the manuscript.

COMMENT: P2 L40-47: The section on potential drivers of the streamflow trends remains a bit vague. Are changes in river cross-sections or boat tourism relevant for annual streamflow volumes?

REPLY: yes, if the shape of the river section is altered, or if flow itself is altered with recreational basin or locks for navigation. These sentences will be however moved to the Discussions to streamline the logical flow of the introduction. A missing reference to Vag et al. will be added in the Bibliography.

COMMENT: P4, L97: I would suggest to first clearly list the criteria for selecting catchments and then mention the final number of selected catchments at the end.

REPLY: The methods will be amended accordingly – filtering criteria will be described in the methods, while the resulting number of catchments retained for analysis will be reported in the Results.

COMMENT: P4, L101-102: You may use this in the introduction in order to emphasize your contribution in comparison to previous studies.

REPLY: Suggestion accepted, the sentence will be integrated in the introduction

COMMENT: P4, L103-109: The description of the criteria for inclusion/exclusion from the database should be very clear. It is not very clear whether you aimed at including only near natural catchments. Did you check information from the data providers on human interventions such as changes in flow abstractions etc. (that would directly influence the trends)? Your database contains _3900 series of 65-years data. It is a lot of work to visually scan daily data of all these series. Could you provide some detail on how this was achieved? Did you apply any automatic screening tests? How were inhomogeneities identified?

REPLY: accepted - see reply to R2 comment 2 above.

COMMENT: P5, L123ff: Why would it make any difference in terms of trend slope whether you calculate it on the original data or on the anomalies?

REPLY: accepted - see reply to R2 comment 5 above.

COMMENT: P5, L128: Delete “To homogenize the annual streamflow series”, since dividing by catchment area cannot homogenize a time series.

REPLY: deleted

COMMENT: P5, L132ff: Have you checked the streamflow series for autocorrelation? How did you deal with series that contain significant autocorrelation?

REPLY: The streamflow series of data were checked with lag-1 autocorrelation coefficient as proposed by Khaliq et al. (2009). The autocorrelation levels are reported in the picture in response to comment 4 of Rev#1. No series was significantly autocorrelated.

COMMENT: P6, L138: Since the streamflow volumes were divided by area, runoff depths would be more appropriate (instead of streamflow volume), no? (adjust throughout the paper)

REPLY: suggestion rejected – the reviewer is right, but streamflow volume is a widespread measure which is readily understandable by managers and citizens. We decided to keep it that way.

COMMENT: P6, L145 and 146: This seems not correct, Table 1 shows positive trends in 7% and negative trends in 5% of the catchments?

REPLY: the overall figures were corrected – 52% of positive trends and 48%, consistent with Table 1

COMMENT: P6 Fig. 3: These figures are not necessary in my opinion.

REPLY: These figures will be deleted.

COMMENT: P6, L151: The unit of annual streamflow per area is length/time (e.g.

m3/(km2 year), or mm y-1). Therefore the change in runoff over a certain period is length/time2 (e.g. m3/(km2 year2)).

REPLY: accepted – the values and units will be updated to reflect yearly change expressed in m3/(km2 year2).

COMMENT: P7, L170; legend and caption of Fig. 5: replace rainfall by precipitation (assuming that snow is included).

REPLY: snow is included. Suggestion accepted.

COMMENT: P7, Fig. 4: Please add trend significance to the figure, e.g. different symbols for significant/insignificant trends.

REPLY: accepted – the figure will be amended using two set of symbols for significant/insignificant trends ($p < 0.05$).

COMMENT: P7, Fig. 4: I assume that the former Yugoslavian countries should also be part of the Mediterranean region?

REPLY: there was a mistake in the background graphics – the figure will be amended by adding former Yugoslavian countries

COMMENT: P9, L175-177: Please add time periods, are you discussing observed or future projected air temperature changes ("expected to increase" points to future changes)?

REPLY: we are discussing future climate scenarios. The sentence will be clarified by adding time periods (e.g. "in 2020-2050")

COMMENT: P9, L177ff.: Please explain why earlier snowmelt would result in increased annual streamflow. This is not so straightforward and there are studies pointing to the opposite (e.g. Berghuijs et al., 2014).

REPLY: the study cited by the reviewer states that "A precipitation shift from snow

towards rain leads to a decrease in streamflow", which is not the point being made here. The role of snowmelt in altering streamflow is in fact debated in in the literature. Concurrent findings will be better explained and supported by additional references, in particular highlighting the hypothesis that earlier snowmelt reduces summer streamflow due to the prolongation of the growing season and increased cumulated water uptake by vegetation.

Additional references:

Berghuijs, W. R., R. A. Woods, and M. Hrachowitz. "A precipitation shift from snow towards rain leads to a decrease in streamflow." Nature Climate Change 4.7 (2014): 583-586.

Elias, E. H., Rango, A., Steele, C. M., Mejia, J. F., & Smith, R. (2015). Assessing climate change impacts on water availability of snowmelt-dominated basins of the Upper Rio Grande basin. Journal of Hydrology: Regional Studies, 3, 525-546.

Regonda, S. K., Rajagopalan, B., Clark, M., & Pitlick, J. (2005). Seasonal cycle shifts in hydroclimatology over the western United States. Journal of climate, 18(2), 372-384.

Stewart, I. T., Cayan, D. R., & Dettinger, M. D. (2004). Changes in snowmelt runoff timing in western North America under a business as usual climate change scenario. Climatic Change, 62(1-3), 217-232.

COMMENT: P9, L182: Replace rainfall by precipitation.

REPLY: accepted and edited throughout the manuscript.

COMMENT: P9, L184/185: There are large agreements between the changes in runoff and precipitation/ air temperature. However, I do not agree that streamflow changes are "perfectly congruent" with the patterns of changes in air temperature and precipitation. For example, despite increases in air temperature and decreases in precipitation, streamflow has increased in northern Spain.

REPLY: see reply to R2 comment 7 above. The tone of this conclusion will be de-emphasized.

COMMENT: P9, L186-195: The discussion is not very clear. Please explain how groundwater or snowmelt effects would affect annual (and not only seasonal or monthly) streamflow. Furthermore, I would suggest keeping the different factors that may explain mixed positive and negative trends apart. For example, glacier melt processes are unlikely to be relevant in Northern Germany.

REPLY: see reply to R2 comment 7 above. Positive and negative effects will be better distinguished.

COMMENT: P10, Fig. 6, lower panel: Better only show significant trends. Also, better show percentage of positive/negative trends and add the number of stations, e.g. to the labels for each bar.

REPLY: Accepted – the figure will be amended accordingly.

COMMENT: P10, L213: Looking at the 1950-2015 series, streamflow is above average 1955-1985 and below average 1985-2015. However, I do not see any particular change point in 1985. Streamflow has been decreasing since about 1965, and if anything, the rate of decrease has rather slowed down since the late 1980s.

REPLY: see reply to R2 comment 4 above.

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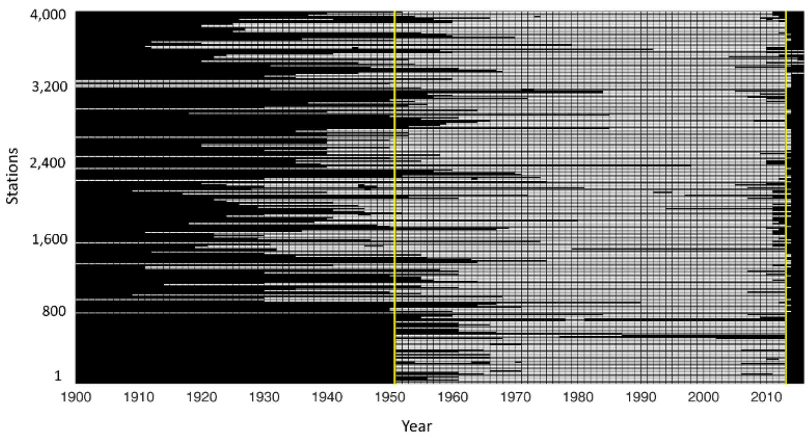

Fig. 1. Time coverage of individual water discharge series in this study