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Hydrology and Earth System Sciences Discussions

# *Interactive comment on* "What drives flood trends along the Rhine River: climate or river training?" *by* S. Vorogushyn and B. Merz

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We thank the anonymous reviewer for her/his comments and suggestions on our manuscript. Below the point-by-point response is provided with original comments quoted in italic.

*First of all I want to apologize for the delay in submitting my comments. This study tries to sort out the different drivers responsible for the changes in flooding along the Rhine River (Germany). This is an interesting topic that has been the object of few papers in the recent years. Moreover, HESS would be a good outlet. With that said, I have a number of comments on this manuscript that would require at least a major revision.*



*- The English is fine but there are typos here and there (e.g., chnage) - Pg. 13538: line 6: "This study addresses"*

**Response:** The typing errors will be corrected in the revised version of the manuscript.

*- After reading the manuscript, I ended up finding myself unconvinced about the claims of this work. To me, the last part of the conclusions (caution in interpreting the results) is more appropriate than what is portrayed in the abstract, where the authors make the case for large scale climate variability. Based on the results by the author (and I don't necessarily agree with the methodology; see below), it seems that river training is just a part of the explanation. However, I didn't see results about large scale climate variability that explained the remaining 80%. In the conclusions, the authors actually discuss some of the limitations of the approach. The finding in the abstract should be made less "conclusive" and more along the lines of the conclusions. As far as I am concerned, the question posed in the title is still unanswered.*

**Response:** We agree with the reviewer that the statement in the abstract about a possible role of climate in explaining the remaining 80% of flow increase should be made more rigorous. Also the second reviewer points out exactly the same issue. Although we meant climate as one of the possible large-scale driving forces, we admit that the statement can be erroneously interpreted. We will follow the suggestions of the reviewers and make the statement more accurate as it appears in the Conclusions: "This means that other drivers but river training in the main Rhine channel are responsible for this residual change such as climate variability/change, land use change and also river engineering in the tributaries."

We agree that the question posed in the title is not fully answered. Rigorously saying we have only isolated the role of river training, explained the mechanism how it affects flood flows and hypothesised about the role of climate. However, we would prefer to keep the title (the second reviewer also agrees to keep it), since the question posed exactly points out to the current debate, particularly with respect to the Rhine River as

discussed in Petrow & Merz (2009) and Villarini et al. (2011).

*- Pg. 13540, line 26: "if data are"*

**Response:** Will be corrected.

*- I was very confused by section 2.2. I am going to try to summarize my problems. I would urge the authors, however, to edit it to make it more understandable.*

*- Section 2.2: how do I know that there are the 5 regimes listed by the author?*

**Response:** These regimes are partly marked by the start of construction of the weir cascade (1955) and end of the construction period (1977) as explained in Section 2.1. Other periods are related to the progress in construction of the detention basins (1998, 2005) and the marked years are selected arbitrary for scenario assessment with the respective detention volume constructed by that time. The selection of the specific years (1998, 2005) just mark specific scenarios and do not influence the final conclusions of the paper.

*- Section 2.2: the HWSG data seem to go till 1993. What about the most recent time? This would cover only 3 regimes.*

**Response:** In HWSG (1993), a scenario with detention volume of about $100 \times 10^6 m^3$ that roughly corresponds to the state of 1998 was evaluated. The construction of the detention basins follows the French-German treaty of 1982 and therefore the effect of some detention basins could already be assessed in 1993 for the state of 1998.

*- From Table 1: I have no idea what values to use for homogenization.*

**Response:** The homogenisation relationship which e.g. relates $Q_{1955}$ to $Q_{1977}$ is used to recompute the river discharge after the construction of the weir cascade (in 1977) as it would be for the river state before 1955. We agree that the notations in the table and in the manuscript could be better explained. This will be changed in the revised version of the manuscript.



*- Pg. 13544, lines 6-8: what linear regressions are we talking about here? Can the authors please add a scatterplot to show the results? Based on what is the "unique homogenized dataset" created?*

**Response:** We agree with the reviewer that the description in this section should be made clearer. We refer here to the linear regressions summarized in Table 1. These are the relationships we mention on Pg. 13543, lines 23-25. The 'unique homogenized dataset' is the dataset with homogenized flood flows that we have computed using the linear regressions from Table 1. (It is indeed badly explained here). We are afraid that providing scatter plots of all or even some of the presented homogenization relationships from Table 1 would not enrich the paper but rather overload it with trivial linear regression plots.

*How do we know that the homogenization included only training and no other drivers? To me, this is a very important section to understand, but it is unclear.*

**Response:** The reviewer points out indeed a very important question, whether the homogenised data reflect the effect of river training only and no other effects are considered. The answer is Yes. The routing models used by HWSG (1993) and BfG (1998) implemented and parameterised the river training measure such as additional detention volumes and the weir cascade which influences flood wave celerity. Both studies used solely the routing models and thus cannot consider any further effects related to land use changes and climate.

*- I have a series of issues with the methodology employed in Section 2.3. First of all, the authors use Mann-Kendall but then use the Theil estimator. As written in the paper, Mann-Kendall tests for the presence of monotonic patterns, which is more general than the linear behaviour assumed by the Theil estimator. Why should the "monotonic pattern" be linear and not exponential, for instance? Also, how sensitive are the results to other robust linear regression methods? More results are needed to support this.*

**Response:** Mann-Kendall test is usually combined with Sen's or Theil estimator which

gives an estimate of the magnitude of the gradual change. Sen's estimator is the median of all pairwise slopes in the time series and is more robust than a linear regression line. It does not assume that the trend is linear, although for visualization purposes Sen's estimator is frequently plotted along the time axis as linear function. We do not see it reasonable to investigate other linear regression methods and their impact on the results in the framework of this study, particularly from the stand-point that the aim is to assess the relative difference in trends in original and homogenised series and not the absolute changes. It can be expected that the relative change is even less sensitive to the assumed estimator than the absolute one.

*- Pg. 13545, line 22: Why are the records "pre-whitened"? Are they auto-correlated? If not, there is no need to do it. If they are, why is an AR1 model assumed? The selection of the "de-trending" model is going to have an effect on the trend results. Please justify the use of an AR1 model in this case by showing that the residuals are Gaussian white noise.*

**Response:** There is an ongoing debate whether to pre-whiten or not to pre-whiten the time series prior to the statistical trend test (e.g. Bayazit & Önöz (2007)). On one hand the presence of auto-correlation may corrupt the results and indicate statistical trends where there is none. On the other hand, pre-whitening may reduce significance results provided by Mann-Kendall test. In this study we follow the conservative way and decide to apply pre-whitening procedure in order to exclude the possibility of trends where there is none. Furthermore, in the context of this study it is not a crucial issue whether to apply or not to apply pre-whitening procedure since it is applied to both original and homogenised data. The paper results are rather based on comparison of homogenised and non-homogenised flood series than on the absolute values. Of course, the pre-whitening is applied to flood flow series that are auto-correlated. We will check the residuals in order to assess the appropriateness of the AR1 model in the revised version of the manuscript.

*- Pg. 13545, line 21: if I remember correctly, Bormann et al. (2011) used a significance*



*level of 5% and 10%, while Villarini et al. (2011) a significance level of 5%. In order to make a meaningful comparison between these results and those in the literature, a 5% level should be used as well. Please show the results with a 5% level.*

**Response:** We agree with the reviewer that to use the significance level of 5% would be more consistent with other studies. We will recomputed the significance results and modify the contour lines in Figures 2 and 7.

*- Equation 1: I don't understand why the linear trend is assumed. Moreover, a lot of the results are based on this selection. Also, what about data until 2011? Why stopping at 2005?*

**Response:** The Equation 1 is misleading and should be changed to reflect any possible period with different start and end periods. This is also mentioned by the second reviewer and will be changed accordingly. The analysis is carried out for the period from 1952 till 2009 for which the discharge data were available from the Global Runoff Data Centre (GRDC) at the time of development of this study.

*- Pg. 13546, lines 16-18: I don't understand this sentence, what a "robust resampling method" is, and why it is needed.*

**Response:** As proposed by Pettitt (1979) the significance probability can be determined by an approximation formula. This formula however delivers appropriate results for a specific range of resulting probability values (up to 0.5). Thus it is not suitable for determination of significance probabilities over the entire range as used in this manuscript (Figure 3). We therefore applied a simple and robust resampling in order determine the significance probability of having a change-point at any particular point in time. For this, 10000 permutations of the original time series were generated, from which the Mann-Whitney statistic (Eq. 2) was computed. The Mann-Whitney statistic based on the original time series was compared to the empirical distribution of Mann-Whitney statistic resulting from the permutations. From this, one can conclude on the probability to have a change-point at the particular point in time.

We agree that the formulation provided in the manuscript is rather cryptic and difficult to understand. We will rework this part of the paper in the revised version of the manuscript.

- Pg. 13548, line 25: why is Figure 7 mentioned here before many others?

**Response:** At this place, Figure 7 is just used to support a specific statement that also tributary discharges that correspond to peak flows in the main channel experienced strong positive trends. Just to extract and place the Figures (A4-F4) at this place would make Figure 7 meaningless for section 3.1.2. We would therefore prefer to keep Figure 7 at its place because its main purpose is to support the results described in section 3.1.2.

- These are some of the major comments I have. A further and more in-depth evaluation of the results would require sorting them out first.

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