

Interactive comment on “Long term variability of the annual hydrological regime and sensitivity to temperature phase shifts in Saxony/Germany” by M. Renner and C. Bernhofer

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Received and published: 9 March 2011

We thank Dr. Orlowsky for his detailed and critical review of our manuscript. He suggested to restructure parts of the text and raised some critical remarks on the used statistics, namely usage of linear and circular statistics. In the revised manuscript we will incorporate his useful suggestions and think that these will contribute to improve the paper. In the following, detailed and justified responses are given.

General remarks

1. I think that leaving out some of the information would make the article much easier to access without weakening the main conclusions, see below.

Authors response: We agree with most suggestions to shorten the manuscript. Details are given below.

2. The authors in some instances do not differentiate clearly between a hypothesis derived from their statistical analysis and causal relations.

Authors response: We thank the referee for pointing this out. Some causal relations might indeed need more elaboration.

3. The phase parameter cannot be assigned to a date such as March 1st. In my understanding, a phase describes a shift in time of some pattern, and this property is lost by such assignment. Transforming the phase from radians to days is of course appropriate.

Authors response: We think that the treatment of phase angles in the manuscript may be not precise, but in the case of an annual period, it is agreed to transform the phase angle into day. Further, the maximum of the cosine representing the annual cycle coincides with the phase angle. Therefore, in this special case of an annual cycle it may be appropriate and more illustrative to use Dates and statements like “earlier” or “later than”.

4. I am not so sure how valid the comparison between snow depth in March and runoff phase is, since the first is linear and the latter is a circular variable. Could particularities of this comparison, e.g., how correlations between these two are calculated, be discussed?

Authors response: This reply also treats the Major comment of the Referee to Section 2.3.

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In the manuscript, we have used the Pearson product momentum correlation coefficient for establishing the link between linear and circular variables. As the Referee notes, this is not mathematically sound. In Jammalamadaka and Sengupta (2001) chapter 8.5, there is a way how to compute the correlation between a linear variable X and a circular variable α . They suggest to transform the circular variable vector α into a linear variable y : $y_i = \cos(\alpha_i - \alpha_0)$. α_0 is estimated by first estimating the coefficients $C1$ and $C2$ applying a regression using ordinary least squares:

$$X = M + C1 \cos \alpha + C2 \sin \alpha \quad (1)$$

Then the $\arctan(C2/C1)$ gives an estimate for α_0 . Significance testing is than based on the test statistic of the Pearson's product moment correlation coefficient, which follows a t-distribution with $n-2$ degrees of freedom. Please note that this test does not regard the properties of the circular - linear variables, but as we use the transformed $y = \cos(\alpha - \alpha_0)$ variable, we believe that the error is negligible.

To illustrate the differences between the methods, the table below presents results from correlating the annual phase of the runoff ratio for Lichtenwalde with snow depth aggregates (such as in section 4.4 P829L11ff):

	Pearson	linear-circular Pearson
Fichtelberg winter snow depths	0.19	0.20
Fichtelberg snow duration	0.25	0.29
Fichtelberg March average snow depths	0.55	0.55

The results are quite similar to ones obtained by using the Pearson correlation coefficient. Also the significance tests results did not change. However, to be mathematically sound, in the revised manuscript we will use the linear - circular correlation coefficients when appropriate and also add the computation to the methods section.

5. The text would strongly benefit from some reader guidance, for example by briefly

summarizing at the beginning of a section the following content.

Authors response: We will follow this suggestion in the revised manuscript, whenever appropriate.

6. The discussion section (no. 5) contains little new information and mainly repeats results and methodological details of the sections before. I'd suggest to merge it into the previous sections.

Authors response: We agree and will merge Results and Discussion into one section.

7. Many of the chosen river basins are probably not independent, since they partly feed into each other. This is not discussed at all, in particular the fact that cluster 2 is basically a collection of the Mulde stations.

Authors response: This comment also refers to major comment at P834L28.

We acknowledge that a range of basins are part of a common river network and are therefore physically and statistically not independent. Still, the head water basins (18 out of 27) may be regarded as independent in terms of watershed properties. However, a separation of effects affecting our results, without leaving out the non-independent river basins, is difficult to achieve. To improve the clarity, we suggest to include a column in Table 1 denoting the connection to other basins in the analysis. This will be combined with a discussion in the Data section and within the Result section.

A possible redesign of Table 1 is given below:

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Table 1: River stations analysed over the period 1930 – 2009. The column *elev* denotes the mean basin elevation in meters above sea level, *area* denotes catchment area in km^2 , *RR* denotes the long term average runoff ratio and *miss* gives the number of missing months. The column *upstream* denotes the identification number (*id*) of analysed basins, which are upstream of the respective basin.

id	station / river	major basin	upstream	elev	area	RR	miss
1	Kirnitzschal/Kirnitzsch	Upper Elbe		381	154	0.36	0
2	Porschdorf/Lachsbach	Upper Elbe		378	267	0.43	0
3	Neundorf/Gottleuba	Upper Elbe		493	133	0.42	0
4	Elbersdorf/Wesenitz	Upper Elbe		317	227	0.37	0
5	Dohna/Müglitz	Upper Elbe		555	198	0.46	9
6	Merzdorf/Döllnitz	Upper Elbe		168	211	0.21	24
7	Koenigsbrueck/Pulsnitz	Schwarze Elster		274	92	0.34	26
8	Grossdittmannsdorf/Röder	Schwarze Elster		248	300	0.30	36
9	Golzern/Mulde	Mulde	10 - 23	481	5442	0.42	12
10	Niederschlema/Zwick. Mulde	Mulde	13	705	759	0.52	12
11	Zwickau/Zwick. Mulde	Mulde	10 13	631	1030	0.46	12
12	Wechselburg/Zwick. Mulde	Mulde	11 10 13 14	491	2107	0.46	0
13	Aue/Schwarzwasser	Mulde		742	362	0.54	0
14	Goeritzhain/Chemnitz	Mulde		410	532	0.47	0
15	Nossen/Freib. Mulde	Mulde	16	485	585	0.43	0
16	Wolfsgrund/Chemnitzbach	Mulde		629	37	0.60	2
17	Niederstriegis/Striegis	Mulde		374	283	0.36	13
18	Hopfgarten/Zschopau	Mulde	20	701	529	0.50	0
19	Lichtenwalde/Zschopau	Mulde	18 20 22 23	618	1575	0.47	0
20	Streckewalde/Preßnitz	Mulde		744	206	0.47	0
21	Pockau/Flöha	Mulde	23	688	385	0.50	0
22	Borstendorf/Flöha	Mulde	21 23	663	644	0.47	0
23	Rothenthal/Natzschung	Mulde		770	75	0.58	0
24	Adorf/Weiße Elster	Weiße Elster		599	171	0.36	35
25	Mylau/Göltzsch	Weiße Elster		518	155	0.46	12
26	Bautzen/Spree	Spree		357	276	0.37	24
27	Groeditz/Löb. Wasser	Spree		284	195	0.29	12

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8. Some essential conclusions, for example the coincident change points in runoff phase and March snow depth, are not apparent to me from the figures.

Authors response: We reply to this point in the major comment section P830L4.

9. Concerning the decreasing runoff ratio further 'downstream': Is this a consequence of a stronger increase of the precipitation-collecting area compared to the increase of runoff due to more and more tributaries?

Authors response:

The reasons for the observed height dependency of the runoff ratio, are first the higher amount of rainfall at higher elevations in the Ore Mountains rising from 500 mm up to 1200 mm (Bernhofer et al., 2008). Secondly, the average annual atmospheric evaporative demand expressed in potential evapotranspiration (PET) is about 600 mm with a decreasing elevation gradient from 650 mm in lower areas to 525 mm in higher elevated areas. Assuming that PET is a rough indicator for actual basin evapotranspiration, it follows that there is more water available for runoff at higher elevations.

So, in larger basins, having parts in higher and lower elevation ranges, these effects will average out, according to the areal percentages in the respective heights. A description of this is found in section 4.1. We consider to improve this by adding annual average rainfall and potential evapotranspiration numbers in section 3.

10. Tables, figures and their captions are sometimes incomplete, inconsistent or redundant. Some remarks on this follow.

Major comments

In Section 2.1, a lot of space is dedicated to complex demodulation, which is hardly used afterwards. Additionally, I did not understand what this method does, especially w.r.t. the lines P817L14ff. Given the focus of the paper, I would start with the description of Stine, followed by a shortened and more precise summary of complex demodulation.

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Authors response: A note to the complex modulation technique, the complex demodulate Y_t as derived by Eq. (2) is a very noisy signal with high frequent components. Bloomfield (2000) proposes several filters to remove these high frequent components from Y_t , as he assumes that phase and amplitude only change slowly with time. As noted in section 2.1. we applied a moving average filter. Weights and window length of the filter have been chosen according to retain as much of the temporal evolution of the annual phase and at the same time removing high frequent periodic patterns. The found weights and window lengths have been applied to all series.

In minor comment P834L23ff, the Referee doubts the complementarity of complex demodulation to the Stine approach, with the argument that both approaches are based on harmonic functions. Therefore, we intend to remove the complete part of complex demodulation and use moving averages for the smooth lines in Fig. 5 instead.

Section 2.3: Circular statistics are not introduced completely. How are correlations between linear and circular variables computed (phase/snow depth)? Authors response: We replied to this in General Remarks Nr. 4.

Section 2.4: How does CUSUM work? The description so far is too poor to understand what it does. The quote P821L5 was confusing to me. How does CUSUM work with circular variables?

Authors response: The CUSUM method is based on the analysis of cumulative standardised regression residuals or in our case standardised anomalies. The methods allows to find structural changes, i.e. instationarity in the mean behaviour of the anomalies. To test for non-stationary behaviour, tests have been developed for these so-called empirical fluctuation processes based on Standard Brownian Motion (see e.g. Brown et al. (1975) or Zeileis et al. (2002)). The resulting test statistic is drawn as horizontal line in Fig. 11. The quote from Brown et al. (1975) that these boundary lines “should be regarded as yardsticks” is used to emphasise that visualising the CUSUM lines

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may be more important than just applying the test. Now, to the question of how to apply CUSUM with circular variables. In the manuscript, the annual phases have been treated as linear ones, which is not fully correct. Further review of circular statistics resulted in the following strategy:

First the circular variables (time vector of phase angles in radians α) need to be de-measured and transformed into linear variables using the sine function.

$$y = \sin(\alpha - \hat{\alpha}) \quad (2)$$

This transformation is also used in Jammalamadaka and Sengupta (2001, chapter 11.2).

Then the CUSUM C is computed as follows (Zeileis et al., 2002, function efp in the strucchange package in R):

$$C_i = \sum_{j=1}^i y / (\sigma_y * \sqrt{n}) \quad (3)$$

whereby σ_y is the estimated standard deviation of y with length of the series n .

Regarding our results and the resulting differences, when comparing the transformed CUSUM and the linear treatment have been small and without changing any of the derived conclusions. The differences are larger, in cases when the angular variable crosses its zero, e.g. from 10° to 350° . In favour of preciseness we intend to use the transformed circular variables for CUSUM computation in the revised manuscript.

Section 3.2: The entire description of the station network and its homogeneity evaluation is too long for this paper. Although it's appreciated that you have done this important work, I suggest to move these technical details to an appendix.

Authors response: We agree with the authors opinion and move the homogeneity evaluation part into the appendix.

P825L18: Generally, there is (to my knowledge) no universal way of identifying an ‘optimal number’ of clusters. Please be more specific. Furthermore, cluster 2 consists almost entirely of Mulde basin stations, which is hardly discussed in terms of introducing correlations between the catchments.

Authors response: To estimate the number of clusters, we have used a computation which is based on the optimum average silhouette width. In detail we applied the function **pamk** from the **R** (www.r-project.org) package **fpc**, (Hennig, 2010).

The second comment on the dependence of the basins has been already replied in the Major Remarks number 7. However, for the classification the dependence of basins which are directly connected, has not been taken into account.

P825L20: I find it problematic to assign a date to a phase. A phase is simply a Δ , so expressing it as days is ok, but a date like March 1st is something really different – it loses the property of temporal shift or lag that is inherent to phases. Based on this, statements like in P826L12 (‘later than...’) don’t make sense.

Authors response: We replied to this point already in remark number 3.

P826L15ff: The PCA paragraph is difficult to understand without showing some results, and it does not add anything to the discussion later on. I suggest to remove it.

Authors response: We agree and will remove the respective paragraph. Using the PCA technique has been thought of as a complementary method. It supports both the classification results and the common trend patterns found within high elevated basins. However, in terms of space we did not graphically present the results of the PCA method.

P826L28ff: The circular density plots in my view do not add anything to the statements already derived from Table 3. Furthermore, I find it difficult to confirm the authors’

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conclusions from this plot. Also, one density estimate is based on only 8 points, which is probably not enough. I suggest to remove Fig. 6 and its discussion.

Authors response:

For the sake of brevity we agree to remove Fig. 6 and its discussion. For clarity, the density estimates are based on the number of stations times the number of years, e.g. $8 * 30 = 240$. The density plots are intended to show the form of the frequency distribution of the annual phase estimates, i.e. skewness and tails.

P828L1: The explanation at this stage is a hypothesis. E.g., snow accumulation requires also precipitation, not just cold temperatures, although I don't know whether precipitation is a limiting factor as well in this region. Hypothesis should be formulated as such (this concerns several instances in the manuscript).

Authors response:

At this stage we discuss the average effect of winter temperatures on snow accumulation and snow melt. Also on average, precipitation has a weak seasonality compared to its variation, and precipitation in winter is typically not a limiting factor in the study area. During late years (according to the annual phase of temperature) distinct lower temperatures in January to March, being lower than 0°C on average (see Fig. 8), are a precondition for snowfall, and subsequently for larger than average snow accumulation in late winter.

To rephrase, this is not a hypothesis, but an assumption based on observations in this low range mountains with temperatures around typically 0°C in winter time.

P829L11ff: Why are the correlations with snow so low, compared to temperature? Does this really support or rather weaken the hypothesis that snow melt is the key mediator between temperature and runoff?

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Authors response: The weak correlations of snow cover duration or snow depths to the annual phase of runoff, suggest that there is some connection, but this link is generally not too important to determine the timing of runoff. Instead, the average snow depths in late winter, showing the largest correlations, is more important. It is a measure of the volume of water stored in the snow cover, which will, once melted, contribute to the river runoff and induce the seasonal peak in the runoff ratio. Naturally, the annual phase estimation of runoff ratio is sensitive to this signal.

P829L19ff: It should be clarified that the identified links are only statistical. A causal dependence has not been established, and in particular the snow related explanations is not so well supported by the correlations.

Authors response: The short discussion above, should have demonstrated that the significant links between late winter snow depth and the timing of runoff ratio have a physical explanation.

P829L24ff: The CUSUM description to me was not understandable, therefore I don't understand the sentence about the rejection of the null in L26ff.

Authors response: The CUSUM graph is a means to detect instationary changes of the mean in a time series (Kleiber and Zeileis, 2008). The horizontal lines in Fig. 11 depict the resulting significance levels ($\alpha = 0.05$) of a stationary empirical fluctuation process (Null Hypothesis) crossing these lines.

P830L4: I do not find that snow depth peaks in 1971, or if it does, then it does so even stronger a few years later. What happened here?

Authors response:

For Fig. 11 we have chosen the March snow depth series from the station Fichtelberg. In this case it was thought to use raw station data, instead of interpolated basin average

data. The CUSUM plot shows enough similarity to the annual phase of runoff ratio, however, the referee is right, the small peak in 1971 is not exactly matching with the other. As already mentioned, there is also interpolated basin average snow depth data available. For clarity, we will add a CUSUM plot of the basin average snow depth for Lichtenwalde.

As can be seen from Fig. 1 (attached to this reply), there are two distinct peaks, concurrent with the one of runoff ratio. In this case there are 10 snow depth observation stations available within the basin boundary to support the basin average. Further, the selection of the station Fichtelberg, a mountain peak station, is likely to have a different climatological evolution, than a basin average, established for an area of 1500 km^2 .

P833L1ff: I find this a too strong statement. In my view, the change point of 1971 is not apparent in the potential explaining variables.

Authors response: This argument accompanies the one from above. Therefore, we choose to replace the CUSUM graph of snow depth at Fichtelberg in Fig. 11, with the basin average March snow depths. Further this signal appears in other basins originating in the Upper Ore Mountains.

P833L8ff: This is too strong, the change points do not agree that well, and even if they did, this would not establish causality.

Authors response: We agree with the referee's opinion and weaken the statement:

Original:"That means that the impact of the timing of temperature on the timing of hydrological regimes is one of several relevant processes which in combination explain the above average departure of the phase of runoff ratio."

New:"That means that the impact of the timing of temperature on the timing of hydrological regimes is one of several relevant processes which in combination *might* explain the above average departure of the phase of runoff ratio."

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P833L13ff: The NAO reasoning in its present form appears rather speculative. The last statement about extreme and enduring NAO effects is not founded in the paper. Maybe the paper can go without the NAO discussion (my preference), or some more discussion on the effect of NAO on the individual variables would be necessary. E.g., the Dresden annual temperatures of these years are not extremely low.

Authors response: For the sake of brevity, we will follow the Referees guidance and remove the NAO discussion.

P834L1ff: The timing of the detected inhomogeneities does not coincide with the anthropogenic influence. Please be precise.

Authors response: The sentence in P834L1ff refers to the homogeneity tests procedure (cf. section 3.1) applied to the runoff data. Thereby two basins, namely Streckewalde and Neundorf, showed to be quite inhomogeneous. Please note, that stronger alterations of streamflow may be expected, when the dam went under operation.

P834L28: The river basins are certainly not independent. The majority of them belongs to the Mulde catchment, and many basins are tributaries to others in your data set.

Authors response: We agree with the comment, that the river basins are not independent and remove this phrase from the sentence. Still, we want to point out, that the temporal evolution, the found change points as well as the dependency to the phase of temperature appears in several head basins in the Ore Mountains (Aue, Pockau, Hopfgarten, Rothenthal, Streckewalde).

Minor comments

P813L20: The Thomas-Fiering model is never used again, suggest not to mention it.

Authors response: We agree. “for water resources management, e.g. the use of the

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Thomas-Fiering simulation model (Maniak, 2005).” changed to “for water resources management (Maniak, 2005).” Authors response:

P814L20: What do you mean by ‘pronounced hydrological regimes’?

Authors response: We meant hydrological regimes with pronounced seasonality.

“these metrics are only useful for very pronounced hydrological regimes such as those dominated by snowmelt.” changed to: “these metrics are only useful for hydrological regimes with pronounced seasonality such as those dominated by snowmelt.”

P814L24ff: This statement depends on the investigated variable!

Authors response: That is true. We change the sentence by adding “many climate records”

“Relatively few studies have studied the variability of the annual cycle, being the strongest signal in climate records at mid to high latitudes” changed to “Relatively few studies have studied the variability of the annual cycle, being the strongest signal in many climate records at mid to high latitudes”

P815, Sec. 1.3: Can be replaced by one or two sentences about the features relevant for the article, population is probably not.

Authors response: We agree.

P816L19: It should be clear that it is two parameters per frequency!

Authors response: We agree and incorporate this into the revised manuscript.

P817, Eq. 2: What is x ? Eq. 3: Im and Re should not be in italics. Φ should be Φ_t ?

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Authors response: “x” in Eq. 2 is a mistake, it should be the time series X_t as in Eq. 1. Also Φ in Eq. 3 must be Φ_t .

P818L24: What happens if some of the data is missing?

Authors response: Then the involved months of the smoothed series are also missing.

P819, Eq. 6: These are not used after-wards, remove.

Authors response: Eq. 6 will be removed in the revised manuscript.

P821L17ff: What do you mean by ‘converge’? What does ‘quarterly’ mean? Authors response: Quarterly refers to the aggregation level, i.e. 3 months. Converge means, that with increasing the aggregation window, the annual phase estimates tend to a common annual phase estimate.

P822L10f: What do you mean by ‘severe’? The detected inhomogeneities in Table 2 do not agree with the dam constructions in time as stated in the text.

Authors response: The use of “severe” was wrong, we mean “strong inhomogeneities” instead. Strong inhomogeneity (Streckewalde, Neundorf) refers to Alexandersson and Pettitt test statistics of runoff ratio, with a probability of the stationarity hypothesis being smaller than 0.025 and 0.05, respectively.

To the second comment, the times in Table 2 refer to years, when the respective test statistics (Alexandersson, 1986; Pettitt, 1979) are above the significance level. Further, the column “reason” in Table 2 lists some information of larger construction and water management sites with possible impacts on streamflow in a basin. Probably larger impacts on the streamflow series are found, when a dam went into operation, which might

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explain the temporal difference in the detected inhomogeneities and the construction time.

P824L7: What does ‘similar’ mean? Please be more precise, possibly in an appendix describing the homogeneity issues (see major comments above).

Authors response: The basin average computation of snow depths and temperature has been identical to the one done for precipitation. The difference is that after the spatial interpolation of snow depths and precipitation negative values have been set to zero. Then from the gridded data basin averages have been computed. Such details will be placed into the appendix in the revised manuscript.

P824L22: ‘semiannual’? Maybe half-year is easier? And how is this peak interpreted?

Authors response: We will use the term “half-year” as suggested. The peak at the half-year cycle is apparent from our runoff ratio data. However, we would rather leave the interpretation of this pattern to others.

P825L12: Fig. 4 has two panels, which are not addressed both here.

Authors response: We intend to change the sentence on P825L11f as follows: Similarly to the runoff ratio, its annual phase is also quite dependent on the basin elevation, which can be seen in Fig. 4 on the left and right panel, respectively.

P825L28ff: I don’t understand the last sentence. & P826L2: Do you mean ρ (as suggested in the text) or R^2 (which usually is used for explained variance, strictly positive)? Is the -0.5 a typo?

Authors response: Indeed this sentence is quite ambiguous. We compared the annual phase estimates and the Q_{50} timing estimates using circular correlation. We found that

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the correlation coefficient is increasing from -0.5 to 0.2 with basin elevation. Note that there is a mistake on line P826L2, which should be “ ρ ” instead of “ R^2 ”.

P826L12: What do you mean by ‘average phase’?

Authors response: “later than average phase” is ambiguous here, would replace this with “later annual phase”.

P827L10f: Do you mean ‘increased’ or ‘increasing’? I would say that the temperature increase starts much earlier.

Authors response: We agree with the Referees comment on the onset of the increasing annual temperatures in Dresden. A linear regression trend test ($p < 0.001$), as well as the Mann Kendall test ($p < 0.001$) support the rejection of the Null Hypothesis of no linear trend. Therefore we would state in the revised manuscript, that there is a linear trend with increasing temperatures over the period 1930 to 2009.

P827L20: ‘One representative...’ Actually, it is the same basin which is used before, but your wording does not suggest that.

Authors response: Indeed, they are the same basins as used before. The text will be changed accordingly.

P828L22: You give the justification why this is interesting only in the discussion in a later chapter, at the first reading I was confused here.

Authors response: As the Referee suggests to merge the results and discussion sections of the manuscript, these points will be brought together.

P828L24: What do you mean by ‘change with time’? Have you calculated correlations in sliding windows?

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Authors response: The statement was not precise and has nothing to do with rolling correlations. We looked at correlations of the annual phases of runoff ratio to monthly temperatures (i.e. every January, February, ...) and found that the correlation depends on the respective month.

We intend to change the lines: “ , but these change with time.” to “ , but these changes from month to month.”

P830L12: What do you mean by ‘withdrawing the null’? Rejecting? Accepting?

Authors response: We meant rejecting.

P830L13: In Fig. 11, according to the caption $\alpha = .05$. Is this inconsistent or a typo?

Authors response: This is an inconsistency, for the table we have used $\alpha = 0.1$ to mark significant changes bold. For the horizontal lines denoting the boundary of stationary processes in Fig. 11 $\alpha = 0.05$ has been used. To overcome this inconsistency, we will use $\alpha = 0.05$ for both in the revised manuscript.

P831L9: Which differences do you mean?

Authors response: We meant the differences when comparing annual phases with half-flow dates.

P831L14f: This statement is neither shown in the plots nor discussed.

Authors response: The statement was, that the low frequent variability of Q_{50} and ϕ_{RR} is similar. For illustration see the attached Fig. 2. It shows annual values and 11-year moving averages (MA) for both timing measures. It can be seen that both series have a tendency towards earlier timing over the whole period. Differences are induced by different calculation schemes and additional variability introduced by precipitation in the phase of runoff ratio.

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With respect to the complementary usage of half-flow dates, it may be valuable to add such a graph to the analysis.

P831L21: 'It is clear...' well, it's still a hypothesis. Suggest to rephrase it here and elsewhere, maybe like 'This hints at effects of ...'

Authors response: We agree and will change this phrase appropriately.

P831L27: PCA does not yield two groups per se. How dominant are the first two modes? This should be said, unless the PCA is removed from the manuscript (which I'd suggest).

Authors response: We already stated above to remove the PCA paragraph.

P832L19f: It's a hypothesis, rephrase maybe as '... probably due to snow storage...'

Authors response: We agree and will change this phrase appropriately.

P833L7: What do you mean by 'significant non-stationary behavior'? This question probably arises because of the incomplete description of CUSUM.

Authors response: Yes, this question is related to the CUSUM graph and its test procedure.

P834L6f: The interpretation is a hypothesis. Please rephrase accordingly.

Authors response: We agree and will change this phrase appropriately.

P834L9: What follows from the normal distribution? Why is it relevant here?

Authors response:

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We intended to argue, that if the errors of the spatial interpolation are normally distributed, than these effects will average out on longer time scales such as annual sums. For the revised manuscript, we will add this assumption.

P834L10: This had already been said before. Such detail should not be part of the discussion section.

Authors response: We agree and will remove these parts from the discussion.

P834L23ff: Do you mean ‘But’ instead of ‘And’? As far as I understood, the basic function is a sine, also for the complex demodulation? This would mean that the confirmation of the Stine-approach results by the complex demodulation results is a necessary consequence of the approach design and does not mean any robustness of the results.

Authors response: Indeed both methods, the one from Stine et al. (2009) and complex demodulation are based on harmonic functions such as the sine. Thus, to follow the arguments of the Referee, the methods are not complementary regarding deviations of the data from the sinusoidal form as stated in P835L24. Therefore we consider to remove this statement and substitute it with a discussion comparing the low frequent variability of the Q_{50} estimates.

P835L13ff: I don’t understand the sentence ‘It has been...’

Authors response: We tried to rephrase the sentence. Original: “It has been discussed that the trend in the phase of runoff ratio explains the opposite linear trends in winter and spring found by previous studies on streamflow changes.”

New: “It has been discussed that the trend in the phase of runoff ratio explains the reversal of the sign of linear trends from winter to spring months, which has been found by previous studies on streamflow changes.”

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P835L18ff: In which way should one be careful about seasonal statistics? What could effects of an improper treatment be?

Authors response: A discussion of probable effects of improper treatment for temperature records is found in Thomson (1995). For example he notes one effect regarding seasonal trend estimation, which would also apply to streamflow, or other series with a seasonal component. The effect of changes in the phase during a reference or trend estimation) period, is that seasonal trend slope estimates contain an undesired bias. Further, Thomson (1995) notes that such spurious components increase the variance of seasonal averages.

We intend to include such a discussion in the conclusion section of the revised manuscript.

P835L27: I don't understand the sentence 'In the course of the discussion...'

Authors response: Original: "In the course of the discussion on climate impacts on hydrological systems, it is notable that an amplified effect of ..."

New: "Regarding the discussion on climate impacts on hydrological systems, it is notable that an amplified effect of ..."

P836L3ff: I don't understand the link to the annual temperature here.

Authors response: We intend to remove this sentence at P836L3f.

Comments on tables and figures

The tables, figures and their captions in general can be improved, for example by adding titles to the figures. Also, the captions do not always mention all elements in the figures, and the choice of colors/line-types lacks consistency sometimes.

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– Table 2: Title line: capitalize first letters. Would suggest to swap 2nd and 3rd columns. In column 3: ‘strong’, ‘large’, ‘weak’ inhomogeneities – what do these attributes mean? They are not defined in a statistical sense. The timing of the detected inhomogeneities does not correspond to the breaks expected from the station histories. Is hm^3 a common unit, what does it mean?

Authors response: We will follow the Referee’s suggestions to improve the table. The subjective definitions of inhomogeneities will be improved according to our reply to major comment P822L10f.

The unit hm^3 is referred to as cubic hectometre, whereby 1,000,000 m^3 equals 1 hm^3 , see e.g. http://en.wikipedia.org/wiki/Cubic_metre.

– Table 3: Where does the standard deviation of Tcoef come from? What are the deviations σ_{P1} ? σ usually denotes the standard deviation, but this would be strictly positive. What does the bold face for some numbers mean? Statistical significance (which level)?

Authors response: We will follow the Referee’s questions to improve the table. Tcoef has been estimated using a linear regression between the annual phases of temperature and the annual phases of runoff ratio. From this the standard deviation is computed. The coefficients are very close to the one yield by a linear transform of the coefficients of a circular-circular regression.

σ_{P1} does refer to the deviation of a peak reported from the CUSUM lines, such as shown in Fig. 11. To avoid confusion with standard deviation, we intend to use dev_{P1} instead.

Numbers marked bold refer to the statistical significant level $\alpha = 0.05$. Besides the last 4 columns where $\alpha = 0.1$ has been used. This will be changed to $\alpha = 0.05$ for consistency reasons.

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– Fig. 1, lower panel: Legend masks gray line out. What happens to the gray line in 2002? You should really explain every element of the figure, e.g., R^2 is not. Does it stand for the correlation coefficient as suggested by the text or really the explained variance?

Authors response: The notation of R^2 in the lower panel of Fig. 1 is a mistake, it is the Pearson correlation coefficient. The figure will be improved according to the Referee's suggestions.

– Fig. 2: Legend incomplete (catchment boundaries?). Would suggest to add elevation, maybe in a second panel. Also locations of precipitation and snow stations would be interesting.

Authors response: We will follow the Referee's suggestions to include a legend entry for basin boundaries.

Adding elevation and precipitation and snow stations may overload to map and is therefore not planned. We intend to add an elevation raster as well as points of precipitation stations to the small boundary map of Germany in Fig. 2.

– Fig. 4: Why is the regression line shown only in the right panel?

Authors response: Because we report the slope and its standard deviation in the text at P825L14. The other relation of runoff ratio to elevation is not central in the paper.

– The red MA is dashed in the legend, but not in the figure.

Authors response: Will be corrected.

– Fig. 6: It's probably not kernel densities but kernel estimates of densities. Why are phases expressed in radians suddenly? Why is the cluster 1 line dashed and the

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cluster 2 line dotted? Anyway, I don't see the added value of this figure given Table 3, perhaps it can be removed.

Authors response: For the sake of briefness we agree to remove Fig. 6 and its discussion.

– Fig. 7: Legend again cuts through the red line (just above the 'g' of average)

Authors response: Will be corrected.

– Fig. 8: Legend cuts through outliers in right panel. Would suggest to include the outliers in the whiskers of the box-plots, all these circles become confusing. Also, adding the differences between early and late years instead of the two annual cycles themselves would make the differences more discernible.

Authors response: We will follow the Referee's suggestions to improve the figure.

– Fig. 11: 'Empirical fluctuation process' is not explained anywhere. Legend cuts through lower confidence band.

Authors response: The CUSUM method as well as "Empirical fluctuation process" will be discussed in section 2.4.

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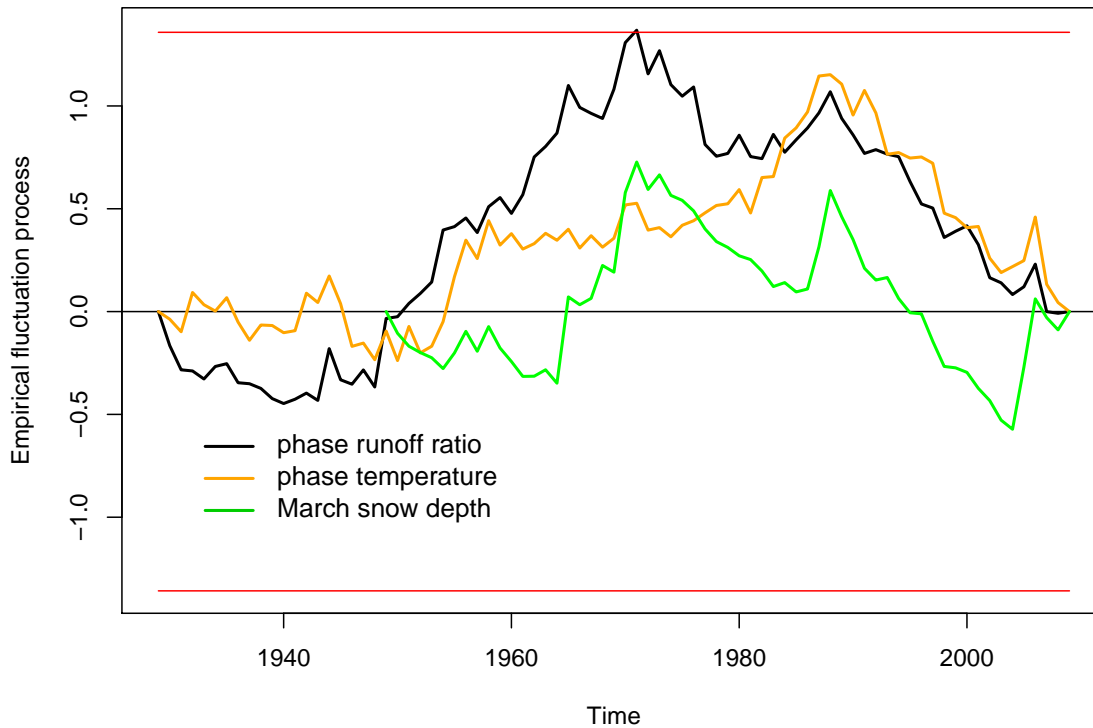


Fig. 1. CUSUM graphs of time series of the annual phase of runoff ratio, the phase of basin temperature and interpolated basin average March snow depths for the basin Lichtenwalde.

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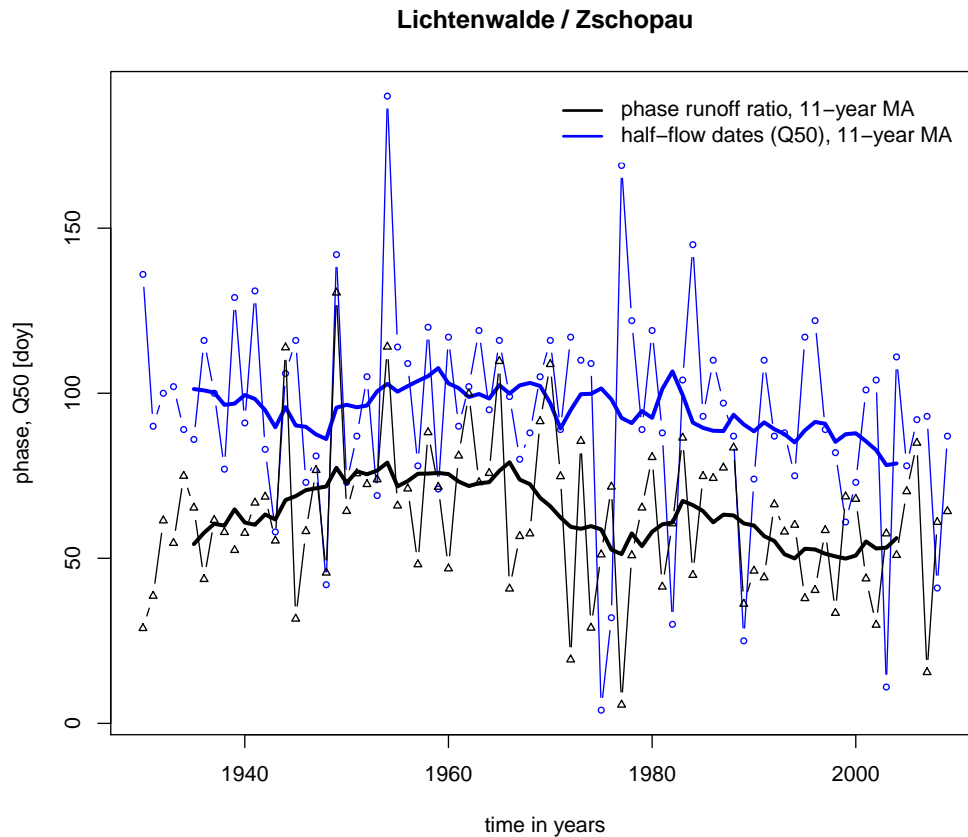


Fig. 2. Annual values and 11-year moving averages for the annual phase of runoff ratio and half-flow dates.

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