

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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This is a very interesting contribution, presenting a comprehensive statistical analysis of seasonal shifts of river runoff regimes in Saxony/Germany. It finds that the annual cycle of runoff ratio has moved towards earlier seasons, and that this effect is stronger in snow dominated catchments at higher elevations. Different causes for this dependence are explored.

Although the results are interesting and the choice of the methodology makes sense, I would encourage a thorough redesign of the text. For me, it was difficult to follow because of a general overload of information (it looks a bit like an attempt to squeeze

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the outcome of an entire PhD thesis into this one article), a structure which might be improved (some suggestions below) and an often imprecise use of English. The amount of editing involved with this makes the revision major, but, once done, I don't expect any further obstacles for publication.

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.
2. The authors in some instances do not differentiate clearly between a hypothesis derived from their statistical analysis and causal relations.
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.
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?
5. The text would strongly benefit from some reader guidance, for example by briefly summarizing at the beginning of a section the following content.
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.

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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.
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.
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?
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 after-wards. 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.

Section 2.3: Circular statistics are not introduced completely. How are correlations between linear and circular variables computed (phase/snow depth)?

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?

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

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important work, I suggest to move these technical details to an 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.

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.

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.

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' 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.

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).

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?

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.

P829L24ff: The CUSUM description to me was not understandable, therefore I don't

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understand the sentence about the rejection of the null in L26ff.

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?

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

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

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.

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

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.

P835L23f: I don't read this conclusion from Fig. 11, see above.

Minor comments

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

P814L20: What do you mean by 'pronounced hydrological regimes'?

P814L24ff: This statement depends on the investigated variable!

P815, Sec. 1.3: Can be replaced by one or two sentences about the features relevant

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for the article, population is probably not.

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

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

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

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

P821L17ff: What do you mean by ‘converge’? What does ‘quarterly’ mean?

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

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

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

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

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?

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

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

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

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.

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

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

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

P831L9: Which differences do you mean?

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

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

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).

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

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

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

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

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

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.

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

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

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

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

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.

- 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?
- 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)?
- 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?
- Fig. 2: Legend incomplete (catchment boundaries?). Would suggest to add

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- elevation, maybe in a second panel. Also locations of precipitation and snow stations would be interesting.
- Fig. 4: Why is the regression line shown only in the right panel?
 - The red MA is dashed in the legend, but not in the figure.
 - 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 cluster 2 line dotted? Anyway, I don't see the added value of this figure given Table 3, perhaps it can be removed.
 - Fig. 7: Legend again cuts through the red line (just above the 'g' of average)
 - 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.
 - Fig. 11: 'Empirical fluctuation process' is not explained anywhere. Legend cuts through lower confidence band.

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