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Hydrology and Earth System Sciences Discussions

Interactive comment on "Paleoclimatological perspective on the hydrometeorology of the Mekong Basin" *by* T. A. Räsänen et al.

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We appreciate a lot the referee's encouraging and constructive comments on our manuscript. They helped us to significantly improve the manuscript. We have addressed and responded to all the comments below. We hope that our answers are adequate. Please see also our responses to editor comments and comments from other referees.

General comments from the referee: Räsänen et al. analyse the time series of discharge (Q) in the context of climate, quantified by means of the Palmer drought index (PDSI), to investigate to what extent current occurrences of floods and droughts can be considered as 'normal', given climate variability.

As such, it is a welcome contribution to the ongoing debate on basin hydrology in a climate-change context. Also, the use of paleoclimatological data as an extra source of information is highly welcomed.

The paper is generally well written, and I would recommend publication after minor revisions, based on my questions below.

Comment 35: According to the abstract and the introduction, one of the (minor) aims of the authors is to distinguish between climatic and direct man-made effects on the hydrology of the Mekong River. Dams are explicitly mentioned. In the main part of the paper, however, only the correlations between climate (PSDI) and discharge (Q) are analyzed. No attempt is made to analyse the effect of dams in the revised paper.

Answer 35: This has been clarified in the revised manuscript. There has been no attempt to distinguish between climatic and direct man-made effects on the hydrology of the Mekong River. The purpose was only to highlight that the results from basin-wide approach based on MADA are not affected by the dam operations whereas the analysis using observed discharge may be affected by those, and other human induced actions (see also response to editor's comment). Discussion on dams has been removed from the revised paper.

C36: The use of the PDSI to assess long-term trends in droughts is recently being criticized (Sheffield et al, 2012, Little change in global drought over the past 60 years, Nature, 491, 435–438). It seems appropriate that the authors at least discuss the potential effect of Sheffield's findings on their results.

A36: This is a very valid comment, and we appreciate that referee brought this up. While Sheffield's critique is valid for using PDSI to measure absolute drought, we argue that in our case it is not important with which method the potential evaporation is calculated as we look at variability. Furthermore, the PDSI from MADA covers the wet monsoon months when the rainfall is the main source of variability. Most important is that the same method is used for the whole dataset, which is the case here. Sheffield's

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(2012) criticism on PDSI was added to Discussion section (Section 5.1), where we also justified PDSI's use in our study.

C37: The PDSI analysis is limited to the JJA monsoon months. So, is 'drought' here defined as a year with a low annual flood peak? I would expect that dry spells that occur in the non-monsoon months are the real droughts, but they are not analysed here, or are they?

A37: This is again a very valid comment; many thanks! In our results we use the terms wet and dry to signify PDSI and flow conditions different from long term-average conditions as the analyses are carried out mainly on wet monsoon season months (as mentioned by referee) and therefore, for example, the traditional definition of drought does not apply. We have used the word drought in the paper only when it refers to actual documented drought in the literature. So in the case of floods wet refers to higher flows (cumulative of hydrological year) and dry refers to lower flows (cumulative of hydrological year) than average. We have clarified this in the Methodology section (Section 3).

C38: Sec 1. A basin averages PDSI value is used. This seems appropriate for dry spells, but I'm not so sure about floods. At least in temperate regions, floods are generated in only a part of a basin (that's why bankfull discharges or annual floods scale less than linearly with basin area). Maybe in tropical monsoon climate's that's different. Please discuss.

A38: In the Mekong the annual hydrograph is a monomodal flood pulse generated by wet monsoon season (May-October). The use the cumulative flow of hydrological year captures the magnitude of this annual flood pulse. The cumulative flow of hydrological year in the lower reaches of the river (e.g. Stun Treng) reflects very much the general intensity of the monsoon and not single short-term rainfall-flood event. The PDSI JJA (June-July-August) tells also about the monsoon intensity during the main monsoon months. The logic is that both the flow of hydrological year (i.e. the flood pulse) and

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PDSI JJA reflect the intensity of the annual monsoon. Thus both the flow of hydrological year and PDSI JJA are used as indicators of wetter and drier conditions than average. Furthermore, Dai et al (2004) has shown that PDSI can be used to reflect annual flows in many regions of the world. We added text to Methodology and Data sections 3 to clarify this issue. We also improved the terminology in Abstract, Introduction and Conclusions to highlight that the main focus of the paper is on inter-annual variability in monsoon conditions and river flow on a river basin scale.

C39: Sec 3.1 The discharge time series prior to 1952 is regarded as less reliable, because there is no rating curve for this period. The authors claim that smoothing increases the usability ("sufficient for analysis of long-term patterns"). No further explanation is given. I've got the impression that the underlying thought here is that individual (annual) errors average out when considering longer time spans. I'm not sure of that. Either the rating curve is linear, in which case water stage can be used as hydrological variable of interest, in which case the problem disappears, or – more probable – the rating curve is nonlinear, in which case the annual errors do not average out. Please elaborate some more on this topic.

A39: This is a valid comment. In the revised paper we do not claim that the smoothing increases the usability of the time series. We just state that the smoothing was used for the identification of wet and dry epochs in PDSIST and discharge and that the results show that both PDSIST and discharge showed remarkably similar patterns in long-term average conditions. It is acknowledged that the potential errors in the rating curve definitions in pre 1952 period may also affect the smoothed data. However, our results do not suggest any significant errors when the PDSIST and the discharge were compared. We also compared Stung Treng discharge to discharge of upstream station Pakse to see whether the data from Sung Treng shows signs of significant errors. We did not find any.

C40: Sec. 3.2.1 The data sets are smoothed with a window size of 21 years. I expected that the selection of this window size would be based on spectral analysis, such that

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short time 'noise' could be separated from long-time 'signal', but that is not the case. The choice of 21 seems to be a bit arbitrarily to me, especially given the detailed attention to wavelet analysis, later on.

A40: The smoothing analysis with moving average with window size of 21 years was removed from the revised manuscript. We left the smoothing with LOESS in the paper. For LOESS we used span (n) of 21 years. This selection removed adequately short-term noise to reveal multi-annual and decadal changes in average conditions. The used span was the longest that we found feasible. If the smoothing span would be longer, the wet (dry) periods would have included individual years which were not inherently wet (dry). The purpose of smoothing and definition of prolonged wet and dry epochs are meant only to be indicative. For example we state at Identification of prolonged wet and dry epochs in Section 3: "Resulting definition is somewhat arbitrary but it is expected to serve the purpose of approximating the major dry and wet epochs in paleoclimatological context"

C41: Sec. 4.1 The PDF analysis reveals that the means of the first and second periods differ. Is this difference statistically significant?

A41: The paper was revised and PDF analyses were omitted (see response to Referee #1 comment C3). This part of discussion, to which the referee refers, does thus not exist in the paper anymore.

C42. The WTC plot suggests that "discharge led the PDSI in the 1920s". This is remarkable, because you would expect the opposite. Please explain.

A42: This was stated incorrectly in the paper. Correct statement is the opposite: PDSIST leads the discharge. It seems that there is a two year timing difference between the major peaks of PDSIST and discharge in pre 1935 period. Otherwise both the PDSIST and discharge show clear resemblance in their annual patterns (Fig. 2A) and periodicities (Fig. 3). See more of the issue in responses to Comments C9 and C15 (Referee #1).

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C43: Sec 4.2 page 12742 line 27. At this point, the “clear epochal patterns” are not clear at all. It is only at page 12743, lines 17+ that it is explained how these patterns were identified.

A43: There has been a confusion of two types of epochs: prolonged wet/dry epochs and epochs dominated by particular periodicities revealed by CWT. The paper has been revised so that only the periods with prolonged wet/dry conditions are called epochs. The periods that referee point at page 12743, lines 17+ are called just periods.

C44: Page 12743 line 2 “dry and wet epochs”. These are based on thresholding the smoothed curves? Please plot the threshold lines as well. Page 12743 lines 18+ “five different epochs”. Please indicate the epochs also in panel b, because it is here were they are defined. You could use thin black vertical lines or so.

A44: We have indicated the prolonged wet and dry epochs in revised Fig. 5A with orange (dry) and blue (wet) colour bars. Page 12743 lines 18+ “five different epochs” which are called in revised manuscript simply as periods are not highlighted in the revised figures. The highlighting was not used in order not to complicate the figures too much. We considered that this approach is appropriate to maintain the clarity and readability of the paper.

C45: Page 12744 lines 11+. In the GEV analysis, you plot PDSI values that seem unrealistic to me. At least, you should include data points in Fig 5. to indicate where extrapolation starts.

A45: The extreme value analysis (GEV) was omitted from the paper for the reasons mentioned by the Referee #1 and also to reduce number of analyses (see comments by Referee #3).

C46: 5.1 Page 12746 line 1 “phase shifts”. You’re referring to the “Q leads PDSI” issue here? Please make this explicit. You’re referring to the unreliability of the data prior to 1952 here. However, according to the manuscript, there appears to be only a problem

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with absolute values here (because of the lacking rating curve) but not with the timing. It seems to me that the erroneous phase shifts could only be due to timing problems, and not due to erroneous absolute values, though. Please discuss.

A46: We agree with the referee and the paper has been revised accordingly. Correct statement is the opposite: PDSIST leads the discharge. It seems that there is a two year timing difference between the major peaks of PDSIST and discharge in the pre-1935 period. Otherwise both the PDSIST and discharge show clear resemblance in their annual patterns (Fig. 2A) and periodicities (Fig. 3). See more on the issue in responses to Comments C9 and C15 (Referee #1).

C47: Figure 1. The current way of indicating the MADA grid cells is not very clear. Please plot the grid boxes, e.g. using thin grey lines.

A47: The Fig 1. was revised so that MADA grid cells are better indicated in the map.

C48: Figure 2b. Please also plot (e.g. using thinner lines) also the annual data, to make a good comparison between annual and smoothed data possible.

A48: The Fig. 2 was fully revised and the annual data are now visible.

C49: Figure 4a. Please add horizontal lines corresponding to the dry/wet spell thresholds.

A49: We have indicated the prolonged wet and dry epochs in Fig. 5A with orange (dry) and blue (wet) colour bars in revised Fig. 5.

C50: Figure 5. Please plot the data points, such that the extrapolation range becomes explicitly visible.

A50: The extreme value analysis (GEV) was omitted from the paper for the reasons mentioned by the Referee #1 and also to reduce number of analyses (see comments by Referee #3). The figure referred by referee does thus not exist in the revised MS.

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