

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

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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 palaeoclimatological data as an extra source of information is highly welcomed.

The paper is generally well written, and I would recommend publication after minor

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revisions, based on my questions below.

General remarks:

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 (PDSI) and discharge (Q) are analyzed. No attempt is made to analyse the effect of dams.

The use of the PDSI to assess long-term trends in droughts is recently being criticised (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.

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?

Specific comments:

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.

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

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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.

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 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.

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

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

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.

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 where they are defined. You could use thin black vertical lines or so.

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.

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 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.

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

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.

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

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

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