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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 are grateful for the constructive comments of the referee. Those helped us to significantly improve the manuscript. We have addressed all the comments below and we hope that our answers adequately respond to those. Please see also our responses to editor comments and comments from other referees.

General comments from the referee: This paper aims to examine the relationship between a paleoclimatological reconstruction of the Palmer Drought Severity Index for the Mekong Basin and Annual Discharge at the most downstream gauging station. Based on this relationship it then draws conclusions about the paleohydrological behaviour of the Mekong. The paper is generally well written and present topical material for HESS.

I first have a couple of overall comments to make.

Comment 1: Plots are presented that use "standardised" flow and PDSI. The standardisation presumably occurred in data pre-processing but this aspect of the methodology has not been discussed. Please specify how the data has been standardised.

Answer 1: Following sentence was added to Section 3.1 to clarify this: "The comparison of discharge and MADA PDSI was done using standard scores calculated for cumulative flows of hydrological year and PDSIST."

C2: In section 3.2.6 an extreme value analysis is described. The methodology used is not at all clear. At one point it says "Thus we fitted the GEV to the original PDSIM series to examine the extreme wet years." Extreme value distributions should be fitted to some sort of extreme value series, such as the maximum daily flow from each year. That is you need to identify extremes in some manner. It is not clear if you have done this (nor how you could do it from this data series, which seems to be just one value for each year)? This section needs to be rewritten for clarity or omitted if this is not an extreme value analysis.

A2: The extreme value analysis was omitted from the paper for the reasons mentioned by the referee, and also to reduce number of analyses (see comments of Referee #3). We believe that the remaining analyses are adequate to justify our findings.

C3: There is an aspect of the statistical analysis that the authors do not seem to have accounted for that is compromising some of their results. At 12745 line 12 and other places where p values are calculated, moving average data seems to be input into the analysis. This means the data points being analysed are not statistically independent, whereas that was presumably assumed if standard tests were used. In fact with a 21 year moving average and 95 years of data, there are few data points to assess the correlations described here (similar issue with trend tests for variance on line 19). Alternative tests or adjustments for the reduction in degrees of freedom resulting from the averaging will need to be used to gain valid results. This will also need to be

explained in the methodology.

A3: The correlation test and linear regression, to which referee refers to, were omitted from the paper for the reasons mentioned by the referee and also to reduce number of analyses (see comments of Referee #3). As stated above, we believe that the remaining analyses are adequate to justify our findings.

C4: 12734 around line 15. Please show locations that are specifically mentioned (Tibetan Plateau, Annamite Mountain Range) on Figure 1.

A4: Labels were added to Figure 1.

C5: Section 2. The introduction to the Mekong River Basin is quite broad (and interesting) but it is not quite clear why it is being given. In particular the various changes occurring in the basin will impact the hydrology but this paper is about climatological effects. Issues such as the impact of dams are not considered in the paper, although after reading this introduction I thought they might be. I would drop out the issues that are irrelevant to the paper.

A5: We agree with the referee and thus the introduction to Mekong basin was shortened. The discussion on hydropower development was removed.

C6: Section 3 paragraph 1. I found this paragraph a little unclear as I was unsure what PDSI data were being referred to and what their source was (paleo reconstruction only or a combination of climate station and paleo reconstruction). While this is explained later, a sentence alerting the reading to the fact that all PDSI data used in the paper is from MADA and that it is the paleoclimatological reconstruction from 1300-2005.

A6: This was a valid comment. We have clarified the issue in the Methodology (Section 3) of the revised paper.

C7: 12737 line 10. It would be useful to say what percentage of the entire basin area is captured by the gauge at Stung Treng?

A7: The percentage of the Stung Treng catchment area of the whole Mekong Basin is 79%. This information was added to the revised paper in Section 3.1.

C8: 12737 line 11-13. The paper does not specify anywhere what the JJA PDSI actually represents. I assume it is the mean PDSI over these three months but this should be clarified.

A8: The MADA is seasonalised gridded PDSI dataset for the summer (June-July-August). We have clarified this in Section 3.1.

C9: 12737 line 15-21. The issue of the lack of a rating curve before 1950 and its impact on discharge data reliability is discussed. This is very important to the paper, given the need to establish that the PDSI is a good surrogate for flow. What is known about how quickly the rating curve for the Mekong changes? If the change is fairly slow, this brings this statement (that the "time series is not accurate for analyses at an annual resolution, but sufficient for analysis of long-term patterns") into question i.e. if it is a slow drift it will affect and 21 year moving average. If most of the change is at higher temporal frequencies then the MA will remove the noise. Do you know anything about this station that would provide insight into this such as whether this is an alluvial or bedrock section?

A9: We have revised the paper regarding this issue in results and discussion sections. We acknowledge that the lack of rating curve will mostly affect flood volumes and not the timing of flood peaks. Unfortunately, we do not have sufficient information of geological conditions of the Stung Treng discharge station. It is located in the region where the river channel is mostly alluvial and the river bank is currently partly constructed. We compared the discharge of Stung Treng (1910-2005) with discharge of upstream station Pakse (over the period of 1923-2005) and did not find any significant differences between them in terms of the timing. 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

of revised paper) and periodicities (Fig. 3). Reasons for this could not be answered within this study. All the data that were used were thoroughly checked to detect errors but none were found.

C10: 12738 line 24 to 12739 line 2. This sentence talks about 3 year dry periods in the raw and moving average data as being multi-year and decadal droughts. I am unclear how 3 years makes a decadal drought?

A10: This is again very valid comment. The definition of dry and wet periods is adjusted in the revised version of the paper and the text throughout the paper has been revised accordingly. The word decadal is not used in the revised paper. Instead we use term 'prolonged dry and wet periods'.

C11: 12740 line 3 suggested rewording of first sentence "The PDF describes the relative likelihood that a variable will take a given value."

A11: PDF method, to which this sentence referred to, was totally omitted from the paper in order to reduce number of analyses (see comment by Referee #3).

C12: Section 3.2.5. It is not obvious why you are evaluating the PDF - perhaps explain.

A12: See previous response (A11) related to PDF method

C13: Section 4. I would reword the entire first paragraph to be: "First we present the comparison of PDSIST and discharge at Stung Treng to determine how well the basin average PDSI describes the annual discharge of the Mekong over the period 1910-2005. Second we present the results from the analysis of PDSIM where we examine the characteristics of the PDSIM in the time and frequency domains."

A13: We agree that it reads better like that; the paragraph was reworded as suggested.

C14: 12741 line 2 and elsewhere. This paper deals with the annual discharge of the Mekong, which is one narrow characterisation of the hydrology of the Mekong. I think you should use the term "annual discharge" rather than "hydrology".

A14: We agree; the terminology was modified as suggested.

C15: 12741 line 4-6. Important differences exist here that seem to be glossed over. The correspondence between annual flow and PDSI for the first half of the data series is not that great (at times they are at opposite extremes) and then there is a moderate relationship. Overall 30% of the temporal variation is explained - that is 70% is error. A little more discussion of this is important as the rest of the paper depends critically on establishing this relationship. This is recognised later in the paper (12745 lines 8-10) where the comment "Thus our analyses (visual comparison and correlation analysis of annual and smoothed data) indicate that the PDSIST is a more efficient proxy for the 10 hydrological conditions on multi-annual and decadal scales than on an annual scale." is made.

A15: We have revised the paper regarding this issue in Results, Discussion and Conclusions sections. 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). We compared the discharge of Stung Treng (1910-2005) and of upstream station Pakse (1923-2005) and did not found any significant differences between them, especially what comes to timing of discharge. Reasons for the timing difference could not be found within this study. Even though the PDSIST did not have high linear correlations in annual scale and in time domain, the frequency domain analyses, continuous wavelet transform and the wavelet coherence, showed that there is strong coherence between the PDSIST and discharge. Therefore we came into conclusion that PDSI derived from MADA is a good proxy long-term average conditions and changes in inter-annual variability.

C16: 12741 line 21-22. The statement about standard deviation doubling puts quite a lot of weight on one extreme 2-3 year period around 2000. Without that event, the variance (not sigma) has increased by more like 50-100% according to Figure 2c, depending whether you look at PDSI or flow.

A16: The paper was revised and PDF analyses were omitted. This part of discussion, to which the referee refers, does thus not exist in the paper anymore.

C17: 12741 line 23-25. I would like to see the histograms rather than the fitted pdfs.

A17: The paper was revised and PDF analyses were omitted. This part of discussion, to which the referee refers, does not exist in the paper anymore.

C18: 12742 line 19-21. Explain what these arrows mean. i.e. up = flow leading PDSI, down = flow lagging PDSI, left = ?, right = ?

A18: This is a good comment and the explanation of the arrows was added to each relevant figure caption. These phase arrows are standard outputs of wavelet coherency (WCO) and they tell whether two time series are phase-locked or whether the other is leading or lagging the other one. For example at wavelength of four years a phase angle of 90° would mean time lag of 1 year between two time series (see example below). Anti-phase situation should occur between ENSO and discharge because when MEIDJF is positive (El Niño) the flow anomaly should be positive, i.e. their correlation is negative. We have added more explanation on phase differences related to WCO analyses into the Methods and Results sections.

Example: wavelength=4 years; phaseangle=90*pi/180=1.571; time-lag=1.571*4/(2*pi)=1.000 years

C19: 12742 line 20. The flow lead the PDSI? - This is pretty hard to explain from a process perceptive. What does this mean?

A19: This was stated incorrectly in paper. Correct statement is the opposite: PDSIST lead 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 also responses to Comments C9 and C15.

C20: 12742 line 25-26. I think this statement needs to be tempered - a strong correla-

tion was found for smoothed data but not annual data.

A20: As some of the analyses were not included in the revised version of the article, due to reasons specified above (see response to comment C3), this statement does not exist anymore in the revised manuscript.

C21: 12744 line 17. As mentioned earlier this extreme value analysis is unclear to me as the methodology hasn't been explained properly.

A21: The extreme value analysis was omitted from the paper, see response to comment C3.

C22: 12745 line 18-20. There is a lag between runoff generation and discharge due to routing - the key rainfall period is probably more like May-September. Certainly these are the main monsoon months in Vientiane.

A22: This is naturally correct and we agree that this issue should be mentioned. The text was thus revised to emphasize the significance of May and September as the referee suggested.

C23: 12745 line 21-22. This statement is not strictly correct. The link to a "clear ENSO signal" has not been demonstrated as this wasn't analysed. The frequencies do correspond, however.

A23: We do agree on that. The paper has been revised significantly so that the ENSO connection to PDSIST and discharge is well demonstrated in Discussion section 5.2.

C24: 12746 line 3-5. It is not clear to me how the uncertainty in the rating would contribute to phase differences or phase shifts in the data.....could you elaborate please.

A24: Thanks for the good comment! We agree that this was stated imprecisely in the paper and the paper has been revised accordingly. 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

C25: Section 5.3. It might be useful to distinguish between meteorological drought (PDSI) and hydrological drought (streamflow) in this discussion.

A25: We do not differentiate between meteorological and hydrological droughts reported in literature because they are often intertwined and not always differentiated in the literature. When we report dry or wet years based on PDSIM we refer in general to years that were wetter or drier than average, although PDSI is generally used to refer to meteorological drought. This is because the analyses are carried out mainly on wet monsoon season months and therefore, for example, the traditional definition of drought does not apply. We have clarified this in the Methodology section (Section 3).

C26: Figure 5 and its discussion. At present I don't think this figure adds anything more than what is evident from Figure 2D. I would suggest to delete it.

A26: We agree on that and the whole extreme value analysis was omitted from the paper; see more in response to comment C3. Former Figure 5 does thus not exist in the revised MS.

C27: 12733 line 23. Please define the acronym MADA where first used. (and remove the later definition)

A27: The paper was revised according to this comment. The acronym is defined now in the Abstract, Introduction and in figure captions to maintain the readability of the paper.

C28: 12736 line 24. Replace "departure" with "demand".

A28: Corrected as suggested by the referee.

C29: 12741 line 22-23. The sentence "The results also indicate...." is repeating lines 8-10. Delete it.

A29: The PDF analyses were omitted from the revised manuscript and thus this repetition was also removed.

C30: 12744 line 2 "their" should be "the"

A30: Corrected as suggested by the referee.

C31: 12744 line 3 should be "closer to an AR2 process"

A31: Corrected as suggested by the referee.

C32: 12744 line 10 "observed elsewhere in the period 1300-2005" would be better

A32: Corrected as suggested by the referee.

C33: 12744 line 14 "to" should be "with"

A33: This part of the text has been omitted from the revised paper.

C34: 12750 line 8. This should be "...with the measured annual discharge..." for clarity.

A34: Corrected as suggested by the referee.

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