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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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First of all, we are very grateful of the constructive comments of the referee. The comments helped us significantly to improve the manuscript. Especially the comments on novelty and scientific contribution forced us to communicate these aspects more clearly in the revised manuscript. We have addressed all the comments below and we hope that we have responded adequately to all of them. Please see also our responses to editor comments and comments from other referees.

General comments from the referee: The paper derives a 700-year long time series of PDSI for the Mekong Basin from the Monsoon Asia Drought Atlas (Cook et al., 2010), and compares this time series with discharge data (station Stung Treng) for the 20th

century. Based on the observed correlation between discharge and PDSI time series, it is assumed that the PDSI series is indicative of the hydrometeorological characteristics of the last 700 years. Its temporal behaviour is analysed and the reported increase in discharge variability for the last few decades (Delgado et al., 2010) is compared to the changes seen during the complete 700 years.

The paper attempts to link systematic measurements with paleohydrological data – an approach which is extremely interesting, for example, to understand natural climate variability versus human-induced climate change. Hence, the overall aim of the paper is very valuable. The paper is well written and structured. However, I have major concerns and some specific comments:

Comment 51: Scientific contribution: The paper uses existing / already published data and well known statistical methods. Hence, its novelty should be sought in the insights it provides. It has essentially 2 messages: (A) The catchment-averaged PDSI, derived from the Monsoon Asia Drought Atlas, is correlated with discharge at station Stung Treng for the 20th century. (B) The increase in variability seen during the last decades has not been seen in the 700 years before. Message (A) is not new. For example, this has been published by Delgado et al. (2012). Message (B) is very interesting. However, this finding cannot be explained, and the reader is somehow left alone with this result. Hence, the scientific contribution of the paper is limited.

Answer 51: Thank you for your comment. We do agree that the novelty and scientific contribution were somewhat poorly communicated in our paper. We paid careful attention to these comments when revising our paper.

We agree on the latter point of criticism; our paper did not explain the increased variability. When revising the paper we paid special attention to this and extended our analysis to cover this part of the story as well. We found that periods with high hydrometeorological variability were associated with high ENSO activity. This is based on our Wavelet analyses and also supported findings by D'Arrigo et al (2005) who found high

ENSO activity within the periods corresponding to our findings and findings by Räsänen et al (2013) who found that ENSO-hydrology relationships have strengthened in the Mekong in the post 1980 period. Thus, the high variability can be explained at least partly by high ENSO activity.

However, we disagree with the first part of the criticism, as Delgado et al. (2012) did not use PDSI or MADA data in their analyses (those are not mentioned in their whole paper, neither in their 2010 paper). They linked the increased variability, based purely on discharge measurements, to e.g. PDO (Pacific Decadal Oscillation), to which referee might have mixed the PDSI due to vast number of acronyms. PDO is an oscillation index and is not linked to PDSI in any way. As referred to in our paper, Dai et al. (1998; 2004) used basin averaged PDSI based on instrumental data to correlate with discharges in the seven largest basins in the world (excluding the Mekong basin). But so far, to the best of our knowledge, there exists no other study which has used MADA, or other tree ring based PDSI palaeo proxy data, on river basin scale to correlate with the discharge. Thus, in the Mekong the instrumental PDSI or palaeo proxy PDSI MADA have not been correlated with discharge measurements before, our study being the first one to do so.

The referee also argues that our paper lacks novelty due to the fact that we use published data and existing methods. We argue that the novelty lies in the approach under which we apply these methods. This is also acknowledged by the Referee: "The paper attempts to link systematic measurements with paleohydrological data – an approach which is extremely interesting". We do agree with him/her, as we believe that our new approach linking the measurements with palaeo data is novel and new. See further justifications on the importance of the approach in our response to the editor comments.

The novelty and scientific contribution of the paper are clarified in the revised paper, being in summary as follows:

Novelty:

i. A new approach was developed to assess hydrometeorological variability in a river basin scale in palaeo time-scale. The method is based on Monsoon Asia Drought Atlas (Cook et al. 2010). We tested the approach using Mekong River Basin as a case study. Our findings suggest that the approach is a robust tool for detecting patterns in inter-annual variability and average conditions in the Mekong River's discharge.

ii. The approach is not case specific and it can be used in any large river basin in Monsoon Asia region.

iii. The developed approach contributes to studies on catchment hydrology and future climate change studies by linking systematic measurements with paleohydrological data.

Scientific contribution (new findings):

i. PDSI MADA was found to be a good proxy for discharge in the Mekong

ii. In the Mekong the hydrometeorological variability in the post 1950 period was significantly higher than elsewhere in the 700-year study period (1300-2005).

iii. The increased variability was associated at least partly to ENSO activity.

C52: Compared to its content/scientific contribution, the paper is very long. I feel it should be substantially shortened: - To make the point that discharge at station Stung Treng is related to the catchment-averaged PDSI, eight methods are used (P12735, L24-27.) Obviously, there are many methods to show that two time series are related. I recommend to reduce the number of methods by choosing the most adequate one(s). For example, what is the benefit of the spectral analysis when wavelet analyses have been performed? Do you really need two smoothing methods (LOESS, moving average)? Similarly, to analyse the PDSI time series for 1300-2005, several methods are used. -There are many other locations where I feel that parts can be deleted, e.g. the introduction to the results section (P12740, L20-25) or the discussion about what cycles have been found by other studies (P12746, L23-28).

A52: The number of methods has been reduced in the revised paper. We selected the most relevant methods, which resulted six main methods under two main themes:

i. Correlating the PDSI with discharge: linear correlations, smoothing, continuous wavelet transform, wavelet coherency

ii. Assessing the short- and long-term variability: Brown-Forsythe type test for variances, continuous wavelet transform, and wavelet coherency.

These methods were carefully chosen so that they all reveal new aspects of the data, do not overlap each other and make our findings and conclusions as robust as possible.

To shorten the text, we removed text from several places by reducing the number of tests and related results, and by removing parts of text, for example from the introduction of the results and the introduction of the study area. However, some referee comments required us to add more discussion in some places, for example in the Discussion section.

C53: Whereas the manuscript extensively uses different statistical methods and extensively describes the results, the interpretation of the findings is rather weak. There is almost no attempt to interpret the many cycles that are found in the analysis. It is well known that hydrometeorological time series show cycles and fluctuations at different frequencies. It is also a widespread phenomenon that cycles come and go (eg Burroughs, WJ, Weather cycles. Real or imaginary? Cambridge University Press, 2003) – so I would argue that the results (eg as shown in Fig. 4 and described on P12743) have to be expected. More interesting questions (How can these cycles be explained? What can we learn from these results? How do you interpret the striking increase of catchment-averaged PDSI variance in the post 1950 period? etc.), are not discussed in detail.

A53: This is very valid comment and we agree that our paper did not attempt to explain the increased variability in post-1950 period. In the revised paper, we addressed this

comment by including a detailed analysis of ENSO activity to the analyses. Based on our findings, much of the variation found in discharge and PDSIST were found to be related to ENSO activity. We also conclude the increase in variability in the most recent decades is caused, at least partly, by increased ENSO activity (revised Fig. 4 in Results section 4.1 and discussion section 5.1).

C54: The proposal that paleoclimatological data could provide valuable information about how future climate change may impact the region's hydroclimatology (P12733, L10-22) is extremely interesting, and I would like to learn more about how paleoclimatological insights could be used for this purpose. That topic could be elaborated later in the paper, e.g. in the section 'Future research directions' (beyond the few hints to published studies).

A54: Our main goal in linking paleoclimatological and future climate projection studies is simple: to consider climate change in a broader time perspective and to emphasize more the issue of discharge variability. For example, when projecting data into the future it would be valuable to know what kind of data is initially projected. But no practical suggestions have been made. However, we elaborated this point further in the revised Future Research section (Section 5.3).

C55: I propose to delete Fig. 6 and the comparison of dry and wet epochs with other studies in Asia. I do not see the benefit of this comparison, given the difference in spatial scale, in definition of epochs, in regions. What can be learned from this comparison?

A55: As requested by the Referee, we removed Fig. 6 from the revised paper.

C56: In summary: I think that, on the one hand, the interpretation needs to be strengthened substantially. On the other hand, the paper should concentrate on the key aspects. Redundant information and pure description of results should be shortened. It should be possible to show that there is a relation between discharge and PDSI with one or two methods (and not with eight).

A56: The interpretation of results and conclusions is strengthened in the revised manuscript. We have considered more carefully the usability of basin-wide approach based on MADA and the findings related to this approach in the Conclusions section 6. We also removed text from several places by reducing the number of tests, as suggested, and the related results. At the same time, some referee comments required us to add more discussion in other places, for example in the introduction to better justify our research, and in the discussion section to discuss how the findings of Sheffield et al (2012) affect our findings.

Specific comments:

C57: P12731, L9: Please elaborate on the statement ". . .general perception is that the flood variability has increased. . .dam construction". One sentence earlier, you say that severe droughts and floods have been observed recently. Please explain how severe droughts and floods are associated with increasing flood variability. Why should dam construction increase flood variability? I would expect a dampening effect.

A57: We agree that this part of text was not written very clearly in the first version of the paper. In the revised paper this part of the text, to which referee refers to, was removed, and the introduction and study area descriptions were written more clearly.

C58: P12731, L26: The 2 sentences ". . .Delgado et al. (2010) . . . 20th century..." are not very clear. The expression "they also found. . ." might imply that the increase in variability is another effect compared to the increase of likelihood of extreme floods. But the latter is a direct consequence of the increasing variability.

A58: This is a valid comment. These two sentences were revised to be clearer in the revised manuscript. This part of text was also moved into the past research section (Section 2.1). The revised text is now the following: "Recent research concludes that the variability of Mekong discharge has increased along the 20th century. Delgado et al. (2010) found that the variability in flows and likelihood of extreme floods increased during the last half of 20th century, whilst the probability of average floods decreased.

The increase in variability is linked to changes in the Western North Pacific Monsoon (WNPM) (Delgado et al (2012)."

C59: P12737, L21: The hydroclimatology of the Mekong basin is represented by cumulative flows of hydrological years. The Mekong River has a very pronounced hydrological regime, consisting of a dry and wet season. How can you differentiate between an average year with average flow in dry and wet season, and another year in which a high flow in the wet season is "canceled out" by a low flow in the dry season? Wouldn't it be helpful to derive two discharge time series, one as proxy for the dry season and the other for the flood season?

A59: In this paper we focus on interannual variability. The MADA is already on an annual basis and therefore we need to define an annual parameter for measuring interannual variability in flow. The flow regime in the Mekong is very pronounced and is often called a monomodal floodpulse. For example, on average the monthly high flow in at Stung Treng is 41,000 m3s-1 and monthly low flow is 1800 m3s-1. The interannual variation in terms of flow volumes in dry season flows is relatively small compared to wet season. The flow variation in the dry season originates often from the early or late end of the wet season, because the dry season receives very little rainfall. For example, this was the case in 2010 when the wet season ended early and parts of the Mekong experienced record low flows. Therefore, the majority of the inter-annual variability comes from the wet season. A large part of the flow in the dry season is recession flow from the wet season, especially in the lower reaches of the Mekong, e.g. at Stung Treng. The use of cumulative flow of the hydrological year treats the hydrological regime as a single flood pulse, which captures the inter-annual variability. This approach is been used before, for example by Kummu and Sarkkula (2008) and Räsänen et al. (2013).

C60: P12740, L12: What is the motivation to divide the long time period into sub-periods of 100 years? Why 100 years?

A60: The GEV analysis, to which this comment refers to, was removed from the revised paper (see more in response to comment C2 by Referee #1). The decision to remove GEV analyses was also part of the reaction to Referee's earlier comment (C52).

C61: P12740, Section 3.26: I am not sure if I understand correctly what has been done here. How do you filter the PDSI time series (1 value per year) to obtain extreme values? Is the selection of extreme wet and extreme dry years related to the definition given in section 3.2.2?

A61: As stated above (A60), the GEV analysis to which this comment refers to was removed from the revised paper.

C62: P12741, L5ff: The correlation coefficients for the smoothed time series might lead to misinterpretation. Smoothing increases the correlation coefficient from 0.55 to 0.9. I propose to delete this information and Fig. 2B.

A62: The correlation analyses on smoothed data, to which this comments refers to, were removed from the revised paper. The decision to remove correlation analyses on smoothed data was part of the reaction to Referee's earlier comment (C52).

C63: P12741, L20: Pdfs are calculated for 2 sub-periods. Why 2 sub-periods? Why the split in the year 1961?

A63: The PDF analyses, to which this comment refers to, were removed from the revised paper. The decision to remove PDF analyses was part of the reaction to Referee's earlier comment (C52).

C64: P12742, L20: What do you mean by ". . .discharge led the PDSI in the 1920s. . ." ?

A64: There was an error in the first version of 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). 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 in timing (Fig. S3 in the supplement).

C65: P12745, L9-10: What do you mean with this sentence (". . .indicate that the PDSI is a more efficient. . .")?

A 65: We changed this part of the text in the revised paper. Now the paper states: "The comparison of PDSIST and discharge at Stung Treng suggested that the basin averaged PDSI derived from MADA can be used as a proxy for the inter-annual variability and long-term average conditions of the Mekong main stem discharges. On annual scale the PDSIST was found to be less robust proxy due to lower correlation and timing difference between PDSIST and discharge in the pre 1935 period." This refers to our findings that on an annual scale there was a timing issues in pre 1935 period between PDSIST and discharge and therefore on annual scale the developed approach is not robust. Despite this timing difference the PDSIST and discharge shared similar pattern in annual, (Fig. 2A), smoothed (Fig. 2B) and in frequency domain (Fig. 3). We have paid more attention and clarified this in the Results, Discussion and Conclusions sections of the revised paper.

C66: P12761, Fig.3: - Use same axis length for vertical axes, in order to better compare the different wavelet results. - Again, I think there is no need to have 3 sub-figures, in order to show the relation between 2 time series. Please decide which method is the most adequate one, and delete the other redundant information.

A66: Fig 3. was substantially revised and is now labelled as Fig 2. The number of analyses was reduced.

C67: P12762, Fig.4: On which basis has the period 1300-2005 been divided into 5 sub-periods?

A67: In the revised paper there is no more clear division into these five sub-periods.

The revised paper rather discusses different periods dominated by different kinds of variability characteristics. The variability characteristics were defined from CWT of PDSIM in revised Fig. 5B

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