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Discussions

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Interactive comment on "Warm-season hydroclimate variability in Central China since 1866 AD and its relations with the East Asian Summer Monsoon: evidence from tree-ring earlywood width" *by* Yesi Zhao et al.

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Response to referee comment 1 Anonymous Referee #1 Received and published: 23 November 2018

1. Referee's comment: Dear editor and authors. Thank you for the task of reviewing the manuscript "Warm-season hydroclimate variability in Central China since 1866 AD and its relations with the East Asian Summer Monsoon: evidence from tree-ring earlywood width". The report is interesting and attempts to provide new exiting information of the

application of traditional proxy parameters derived from tree rings and at the same time attempts to provide information on the relationship between hydroclimate and the Eastern Asian Summer Monsoon (EASM). PDSI was used before in relationship to EASM at a broader scale by Cook et al. (2013), Deng et al. (2013), been applied before. The manuscript is very interesting, tidy presented, with interesting figures. The work is ambitious and reach partly the objectives. I consider that the methods are appropriate to a great extent but not determinant to fully accept the conclusions of the study. The main problem as I see, is that the authors attempted to do two papers in one, one on the quality of the signal detected by different tree ring parameters, and one on the relationship of the reconstructed regional reconstruction. These are well reflected in the objectives. As a consequence, each aim is partially achieved, but not beyond doubts. Author's Response: Thank you very much for your comments. We have strengthened the analysis for each aim, and hope you find this revision satisfactory.

2. Referee's comment: 1. For the aim n1, (1) "(To) compare the climate sensitivity of tree-ring parameters earlywood width (EWW), latewood width (LWW), and total tree-ring width (TRW) in P. tabulaeformis at BYS and LCM" (where BYS and LCM two study sites). The authors compare tree ring data with means of temperature, precipitation totals and hydroclimatic index scPDSI. This aim is partially reached by the authors. It needs to be completed with further assessment of LWW and TRW parameters have significance, but are left aside for the more sensitive EWW and not further analyzed. The probable relationships at different frequencies (interannual, to decadal) are tested only very succinctly with no exploration on the possible lags. Author's Response: Thank you very much for suggestion. We enhanced the analysis to verify that EWW can provide much stronger hydroclimatic signals than TRW and LWW from the aspects of different frequency domain, lags and leads using the wavelet coherence method. Please refer to Line 10-12 of Page 6, Line 26-33 of Page 8, and Fig. 5 in the revision.

3. Referee's comment: Moreover, only one detrending procedure was reported, a

rather conservative one, not that it is wrong, but certainly other routines should be tried when investigating aim 1. In this case, the frequency responses of each of the parameters tested should have been analyzed and tailor-made detrending options to preserve best the signal characteristics. The climate data should also be enhanced, different temperature patterns to start, min-max temperature and different precipitation indices. Author's Response: Thank you very much for suggestion. We used other two detrending methods and signal-free method to create six kinds of chronologies for comparison, and to find out the best detrending and standardization method. Please refer to Section 2.3, Section 3.1 and Figs. 3-4 in the revision. We added the maximum temperature, minimum temperature, and the SPEI of 1-month, 3-month and 12-month to enhance the climate data. Please refer to Section 2.4, Section 3.1 and Figs. 3-4 in the revision.

4. Referee's comment: Since there is no mention of the detrended interannual correlations except as in figure 8b (this is not mentioned in the methods) or the lower frequencies, the exploration of this frequency domain can be seen as incomplete. Please see through to discuss the differences of why PDSI indices are of higher relevance than precipitation alone mostly if tree rings series are irresponsive to precipitation. It is still unclear whether the partial correlation tests were run for precipitation and temperature excluding PDSI, etc please explain. Author's Response: Thank you very much for pointing out these issues. we also calculated the correlation coefficients between the prewhitened and linearly detrended chronologies and climate data to indicate that no inflation of correlation due to the autocorrelations and trends. Please refer to Line 5-8 of Page 6, Section 3.1 and Fig. 4. Test on the lower frequencies was done using wavelet coherence method, please refer to the answer for Comment 1. In fact, the May precipiation has significant impact on tree-growth (Figs. 3-4). The reasons why EWW is still restricted by PDSI but not precipitation during June-July can be referred to Line 26-29 of Page 7 in revision. Just as the comments of RC2 and SC1, the partial correlation tests for tree-ring width and precipiation, temperature, and PDSI is unreasonable, since the PDSI is calculated based on precipitation and temperature. Therefore, we

removed the partial correlation analysis.

5. Referee's comment: The authors indicate that MJJ (early season moisture availability) can be driver for the growing season increment of EWW. It can be considered that previous years moisture also affects the present year increment (see Fritts 1976) for example. The correlations tested start from July in the previous year. This means that April, May and June one or two years before can have importance. If this analysis is done please present the results. If it is not done yet please add it to the report. Regarding this problem, I may suggest the authors do additional tests either wavelet analysis, or evolutionary and moving intervals as those available in Dendroclim package (Biondi and Waikul, 2004) on longer temporal extension data. On the other hand, the positive correlation of LWW with PDSI indicates that there is an effect of this index on tree growth at some point in the growing season. The relationship between August temperatures on LWW with the previous year may be at least discussed. The opposite patterns of correlations found for precipitation and temperature in May (current growing season) indicates that trade off mechanisms between these two factors and photosynthesis are in action through the beginning of the growing season. This may perhaps be clarified with extending the study period to two years before the growing season as well as testing residual chronologies against residuals of the climate data. Author's Response: Thank you very much for pointing out these aspects needed to be considered. We extended the time period to the January of two years earlier. Please refer to Line 2-3 of Page 6, Line 10-17 of Page 8 and Figs. 3-4. We used wavelet coherence method to study the temporal stability. Please refer to the response for Comment 2. We discussed the possible reasons for the significant correlation between LWW and last August temperature, please refer to Line 10-17 of Page 8. Test on the residual chronologies and residuals of climate data was done. Please refer to the response to Comment 4.

6. Referee's comment: "(To) Attempt to reconstruct regional hydroclimate variability using the parameter that contains the strongest hydroclimate signals". I think this is

what the authors really had in mind when writing the report. I think it is brave to attempt to reconstruct regional features based on two sampling plots (33 trees) merged, located in the edge of the region in focus. Let alone to call it regional or local, to reach wider spatial representation more proxy data should be added. And previous to merge these datasets, more tests could have been attempted to see if both sites have same climatic signals. This comment is grounded on the small sample size, its only 33 individuals that can be deeply explored. Authors' Response: Thank you for pointing out the problems. In the revision, we firstly calculated the correlations between the chronologies of two sites. We found that the chronologies showed very high correlation, indicating they shared similar climatic signals. Therefore, we merged the tree-ring samples from the two sites to create a composite chronology. This can be referred to Line 20-23 of Page 4, and Table S1 in the Supplementary material. The reason for that our tree-ring sites located in the edge of focus may be because the meteorological stations utilized by CRU scPDSI dataset were unevenly distributed and mainly concentrated in the west side of our tree-ring sites. Please refer to Fig. S5 and Table S4 in the Supplementary material. To capture a regional scPDSI variation, we admitted that the sample depth is too small. In the revision, we selected the scPDSI over a smaller space for calibration. We would take more samples in the future to capture a regional scPDSI variation.

7. Referee's comment: "To explore the relationship between reconstructed scPDSI with EASM". I understand the need to use EASM. This exploration is also succinct. But, it can and should be explored more in detail. With that in mind, almost trivial analysis are well tested and available: e.g. evolutionary response, moving intervals, coherency and wavelet analysis among others. The aim is to find synchrony (asynchrony) between datasets and extreme episodes that can be used to link two signals. These tests can really help to clarify when and how these signals could have been related and the stability of the relationship. To achieve this aim, I consider that other environmental signals with their lags should be ruled out as well. The authors mention other circulation patterns that are expected to influence the climate in the study area. Author's Response: Thank you for suggestion. In the revision, we tentatively explore the rela-

tionship between the reconstructed hydroclimate variability and EASM. Firstly, we used the wavelet coherence method to test the temporal stability and lags of the relationship between EASMI and reconstructed scPDSI. A strong in-phase relationship between EASMI and the reconstructed scPDSI was found before the 1940s on the decadal and longer timescales. And, this significant in-phase relationship was further evidenced by the 21-year moving window correlation analysis on the decadal-filtered EASMI and scPDSI. We detailly explored the causes for the unstable relationship between EASMI and scPDSI using the precipiation data. We attributed the lack of correlation between EASMI and scPDSI partly to the change of leading mode of EASM precipitation. Please see section 3.4 in the revision. The influence of other circulation patterns on the climate in our sampling sites would be studied in the future.

8. Referee's comment: Once these issues are solved, the authors will have material to two good papers: one on comparison between two or three tree ring parameters and one on the reconstruction of scPDSI and its subsequent comparison with the EASM and other atmospheric circulation patterns. I consider that the authors should take a decision on this issue and work on these alternatives separated. Each of these alternatives are promising contributions to the scientific community. Further, I provide detailed comments that may improve the article readability and content to rise its quality to a more publishable level. Author's Response: Thank you very much for your evaluation and advice. In the revision, we mainly focused on revealing the climatic significance of EWW and reconstructing the MJJ scPDSI. Further comparisons with the large-scale atmospheric circulation pattern are indeed an important task, but we have limited ability to dig into this issue at this stage, given that we have only one series based on two sampling sites, and the climate forcing are very complicated. Therefore, we only conducted a tentative exploration of the relationship between the reconstructed scPDSI and EASM (the most apparent influence factor) in section 3.4, indicating that this reconstruction could provide us some new understanding of the impact of EASM on local hydroclimatic condition. Please consider whether this part is acceptable.

9. Referee's comment: Page 1 lines 15-18: Please be so kind to avoid redundancy. Author's Response: Many thanks. It was modified. Please refer to Line 20-22 of Page 1.

10. Referee's comment: Page 1 line 16, MJJ scPDSI was used to denominate both the reconstruction and the scPDSI data targeted which made it rather confusing. Please use other denomination for the reconstructed data. Author's Response: Sorry for this. In the revision, we only used the MJJ scPDSI from CRU scPDSI 3.25 dataset for reconstruction. The comparison was deleted.

11. Referee's comment: Introduction. Generally, the introduction is somewhat confusing, mostly due to alternation of subjects either focusing on hydroclimatic data or the EASM. Then the real product of this article is a reconstruction hydroclimatic patterns, or an attempt to provide a predictor for the EASM, or comparisons between TRW, EWW and LWW. The authors claim that a comparison of the sensitivity to climate patters is the first objective, then the introduction should start in that way, and not focusing on EASM or scPDSI indices. WDI should be properly introduced and described. Author's Response: Thank you for pointing out this problem. We modified the introduction thoroughly with focusing on tree-ring directly rather than EASM or scPDSI. Please see Section 1 in the revision. The "WDI" in the comment may be "DWI" as we think, it was detailly described in the Line 18-23 of Page 5, as it was only used for comparison with our reconstruction.

12. Referee's comment: Page 1, lines 24-25. Please consider explain the frequency domain of these examples as well as the temporal extension. If the aim is decadal to interdecadal variability, the authors could explain these anomalous events in this frequency context. Anomalous in terms of strength of the wind? The timing in the season? The spatial extension? Please explain. Author's Response: This part was removed. In the revision, we start the introduction from tree-ring based reconstruction, and intra-annual tree-ring width directly. The EASM is not the key part of the introduction.

13. Referee's comment: Page 2, lines 4-5. The study is not focused on comparison with other proxies please reword. Author's Response: This part was removed, as we focused on tree-ring based reconstruction, and intra-annual tree-ring width parameters in the introduction.

14. Referee's comment: Page 2 lines 14-15. "and suggested the use of tree-ring stable isotopes to capture hydroclimate signals" Is this sentence relevant to the study? It suggests that the study focuses on these proxies. Author's Response: Many thanks. We removed this sentence.

15. Referee's comment: Page 2, line 20 . "These findings inspired us reconstructing hydroclimate variations..." please change to inspired us "to reconstruct..." please consider that reconstructions of past climate can not be achieve by inspiration alone. Intensive experimentation is a previous process in such an attempt. More over this sentence introduces the study aims, but later, the authors continue with introductory facts. Please consider to move this sentence further in the introduction. Author's Response: Many thanks. We removed this sentence and clarified our aims only in the end of the introduction.

16. Referee's comment: Methods The authors are too general in the description of the methods. Please be specific to guarantee reproducibility of the results. Author's Response: Thank you very much for this suggestion. We added more detail descriptions in the method section including the different detrending and standardization methods, correlation analysis, prewhitening and detrending methods, low-pass filtering methods and so on. Please refer to Section 2.3 and 2.5.

17. Referee's comment: Page 3 lines 3-9: Please indicate the extension of the datasets do they start in 1887? Please indicate correlation values of detrended data, either residuals or first differences, otherwise is a trend relationship that the authors are describing. Author's Response: Thank you very much for this suggestion. Extension of the datasets were indicated in Line 13 of Page 4 in the revision. Correlation were tested

based on both the original standard and signal-free chronologies and their prewhitened and linearly detrended series. Please refer to Line 20-23 of Page 4 and Table S1 in the Supplementary material.

18. Referee's comment: Page 3 line 19-27. With aims of reconstruct climate data, would it not be better to keep two separate chronologies and use them as independent predictors to the PDSI? Provided that there are issues on the signal strength and intercorrelation between the two datasets may expected to be higher due to the distance between sites, whereas it may be expected to have different climatic signal due to the altitude difference. Author's Response: Please refer to responses for Comment 6 and Comment 17.

19. Referee's comment: Page 3 lines 32 to Page 4 line 1: "and were quality checked before release" vague sentence and perhaps not really relevant as written here if the authors of this article have not done this quality check. What do the authors mean with quality check? Is the data homogenized in any way? Author's Response: Sorry for this. We mean the "quality check" is the homogeneity and missing values had been checked and corrected by the China Meteorological Administration before publish the data. We deleted this sentence in the revision.

20. Referee's comment: Page 4 line 4. PDSI is not described in the introduction either its application in relevant articles in the area. Please be so kind to complete or specify. Author's Response: Thank you for this suggestion. It was done. Please refer to Line 20-23 of Page 2.

21. Referee's comment: Please be specific what frequency domain is tested in the correlation test. Only data with no autocorrelation can give interannual responses without low frequency noise. If the standard versions of the chronologies were used the authors, they should indicate the possibility of inflated correlation values due to the slope effects of the curves. Author's Response: Thank you for this suggestion. We used the prewhitened and linearly detrended chronologies and climate data to calculate the

correlations. Please refer to Line 4-8 of Page 6.

22. Referee's comment: Page 4 lines 8-10. Please be more specific on what limiting factors, since the authors are performing the analysis at this stage, do they assume hydrological deficit is a limiting factor? Or temperature alone? Regional means, or extremes, etc. Author's Response: Thank you for this suggestion. It was done. Please refer to Line 13-15 of Page 6.

23. Referee's comment: Page 4 line 11: "For hydroclimate reconstruction", grammatically incorrect, please revise. Author's Response: Thank you for this suggestion. It was done.

24. Referee's comment: Page 4 line 13: Please specify the periods which were used to split the data. Author's Response: Thank you for this suggestion. It was done. Please refer to Line 16 of Page 6.

25. Referee's comment: Page 4 line 13: The authors could be so kind to add Durbin Watson test and Cox and Stuart Tests for the autocorrelations of the regression residuals. Author's Response: Thank you for this suggestion. It was done. Please refer to Line 18-23 of Page 6, Table 3, and Fig. 6b in the revision.

26. Referee's comment: Page 4 Lines 13-14. Please be specific: What spatial data were compared the reconstructed time series with? Author's Response: It's the CRU scPDSI 3.25 dataset (van der Schrier et al., 2013). It was added in the revision. Please refer to Line 29-31 of Page 6.

27. Referee's comment: Page 4 line 16. Please explain the criteria for selection of the spatial extension of the scPDSI data used in the study. Author's Response: We selected this spatial extension of the scPDSI because the scPDSI in this area has the highest correlations with our EWW, although it was in the west side of our tree-ring sites. This may be because the meteorological stations utilized by CRU scPDSI dataset were unevenly distributed and mainly concentrated in the west side of our tree-ring

sites. Please refer to Line 6-9 of page 5, Fig. S5 and Table S4 in the Supplementary material.

28. Referee's comment: Page 4 lines 21-24. This description introduces the reader to EASMI indices and should be properly described in the introduction. If you please. Author's Response: Thank you very much for this suggestion. It was done. Please refer to Line 22-25 of Page 2. Besides, we detailly described this EASMI in Line 23-31 of Page 5.

29. Referee's comment: Page 4 lines 15-20. Could you please indicate the length of the time series named here. Author's Response: It was done. Please refer to Line 15-18 of page 5.

30. Referee's comment: Page 4 line 21. Vague sentence since the term "notions" is confusing in this context, please reword. Author's Response: Thanks. It was done.

31. Referee's comment: Page 4 lines 24-25. Please describe how this index was calculated, even if it is described in Zhao et al. (2015). Author's Response: Thanks. It was done. Please refer to Line 25 of Page 5.

32. Referee's comment: Page 4 line 25. "the used 200 ha..." Please remove "The used" Author's Response: Thanks. It was done.

33. Referee's comment: Page 4 line 31. First differences or trends? Please see comment on this issue above reference to the page 4 lines 7-10. Author's Response: In the revision, we tested the correlations between EASMI and our reconstruction on different frequency domain using the wavelet coherence method. When we compared the decadal filtered EASMI, scPDSI, and Precipiation, we used Pearson's correlation analysis, and the significance of correlation coefficients were tested using Monte Carlo method. The significance of correlations between tree-ring width and climate data was also tested using Monte Carlo method. Please refer to Line 14-19 of Page 7.

34. Referee's comment: Page 4 line 30-34. I Don't understand, is this only one pro-

cedure? correlation tests on FFT filtered series? And that is why the authors adjusted the degrees of freedom, right? Author's Response: Sorry for this. Since the time series were lowpass filtered by FFT, their degrees of freedom were changed. We test the significance of correlations between the filtered series according to Yan et al., (2003). In the revision, we tested the significance for all correlations using Monte Carlo method. Please refer to Line 14-19 of Page 7.

35. Referee's comment: Page 4 Line 34 to page 5 line 5: Is this the spatial correlation the authors used Climate explorer suite? Please explain how this was done, for example, lags, filtering, first differences, etc. Author's Response: Sorry for not detail explanation. The Climate Explorer suite cannot provide correlation on the decadal filtered series. We lowpass filtered all time series, and calculated the EOF, correlations using Matlab and draw the plots using Surfer 10. Please refer to Line 12-13 of Page 7.

36. Referee's comment: Page 4 line 34. These datasets can be used to represent temperature and precipitation, rather than "reproduce". Author's Response: Many thanks. It was done. Please refer to Line 33-34 of Page 5.

37. Referee's comment: On the methods section, the descriptions of the data are good, but can be favorable to present it as well in a table. Author's Response: Many thanks. It was done. Please refer to Table 1-2.

38. Referee's comment: In addition, pleas be so kind to check for repetition in lines 4-6 and 16-17 in page 4. Author's Response: Many thanks. We only used the CRU scPDSI dataset for reconstruction, and removed the comparison.

39. Referee's comment: Page 5 lines 8-9: If the extension of the chronology are not specific results of this research should be stated in the methods section. Moreover, these descriptions temporal extension of the data, EPS, Rbar, mean, etc. are better presented in a table. If the authors will keep the paragraph, please add some values, these give base to the comparison between chronologies. For example, how much stronger were the common signals of EWW? Author's Response: Thank you for your

suggestion. As this part is not our aim in the revision, we only mentioned the extension of chronology in Section 2.3. Meanwhile, the statistics of the chronologies are presented in the form of tables shown in the Supplementary material (Table S2 and Table S3.)

40. Referee's comment: Page 5 line 20: Please revise the grammar, "time stable" change to "is more stable through time than..." but then what do the authors mean with this? Could you please prove this with values? Author's Response: Sorry for this. Here we mean that the relationship of EWW and MJJ scPDSI showed more stable through time than LWW and TRW. This was done by 21-year moving correlation analysis in the original manuscript. In the revision, we used the wavelet coherence method. Please refer to Line 26-33 of Page 8, and Fig. 5.

41. Referee's comment: Page 5 line 23. "By contrast, LWW almost has no significant correlations" please add the values to make it comparable, and change "almost has no" to "has almost no..." Author's Response: Thank you for pointing out this issue. It was done. Please refer to 2-7 of Page 8.

42. Referee's comment: Page 5 line 24. A conceptual observation, LWW can not induce anything... The researchers included LWW information in TRW information. The effect is understandably a decrease of climate sensitivity for the months and frequency tested. But please consider to test the data with no trends. Author's Response: Thank you for suggestion. The sentence has been modified. Please refer to Line 8-11 of Page 9. The tests with no trend were conducted. Please refer to the response for Comment 4.

43. Referee's comment: One more observation: the positive correlations between tree ring data and temperature and PDSI in months other than growing season can be seen as an alarm. Is it possible that there is an artefact rising the correlation values? Following the same reasoning, the spread of the correlation values is quite low both before and after the growing season, and I do not think that the trees continue

photosynthesizing in December? This issue needs to be explored and analyzed more deeply before publication. Author's Response: According to the previous studies relevant to the seasonal dynamics of cambial activities in P. tabulaeformis (Line 10-17 of Page 3 in the revision), the tree would could not photosynthesize in December, and the earlywood growth could terminate in the mid-July. The significant correlations between EWW and scPDSI after the growing season may be ascribed to the characteristic of scPDSI which has a strong autocorrelation with previous months. This has been clarified in Line 31-32 of Page 7 and Line 1 of Page 8. The significant correlation between EWW and temperature in November seemed caused by the low-frequency, as there is no significant correlation was found between their first-order difference (Fig. 1). Since the Referee 2 argued that the correlations analysis between tree-ring width the and climatic factors in November and December is unreasonable, we deleted the correlation analysis in the revision.

44. Referee's comment: Page 6 line 5, did the authors consider two chronologies for predictors of scPDSI? Author's Response: Thank you for pointing out this issue. The number of our tree-rings samples were limited, especially in the LCM, where only 11 trees were only obtained. We found the tree-growth at the two sites shared very similar variations, manifesting the similar climate forcing. Therefore, we merged the tree-ring samples from LCM and BYS to get a chronology and used for calibration with scPDSI. We would take more samples in the future to capture a regional scPDSI signals. Please refer to the response to Comment 6.

45. Referee's comment: Page 6 lines 12-13. "We restored the variance of reconstruction... " Do the authors mean scaled? Also consider pleas to add "the" before "reconstruction". Author's Response: Yes, it is. Please refer to the equation (1) in the revision (Line 25 of Page 6). "the" was added.

46. Referee's comment: Page 6 lines 15-16. Please indicate the frequency domain the correlation is tested on. Author's Response: In the revision, we tested the correlation between the reconstruction and other hydroclimatic series on the interannual, and

decadal and longer timescales, respectively. Please refer to Section 3.3 in the revision.

47. Referee's comment: Page 6 lines 19-21. Please consider the number of datasets used in Cook et al, 2010 (>300) in relation to this study where the authors used two chronologies, it could be argued that the spatiotemporal signal strength in this study is restricted to the area shown in the figure 1. But also, be so kind to consider the different target seasons of these datasets (MJJ and JJA). In relation to the figure 1(b): Do the authors refer to NADA dataset only to the grid point indicated in the map with the red triangle? If so, it is not clear in the text, or in the figure. I also consider that a suggestion that NADA is biased and the results presented here are more correct is premature (just on regard of the sample size). Author's Response: Many thanks for pointing out these issues. The MADA grid is labeled using a red triangle in Fig. 1. Its coordinate is included in the text. Please refer to Line 15 of Page 5. In the revision, we only discussed the mismatches between our reconstruction and the MADA and the possible reasons, and removed the argument that MADA is biased in recent decades. Please refer to Section 3.3.

48. Referee's comment: Page 6 lines 21-22. As mentioned before, please report the frequency domain of the test. Author's Response: Please refer to answer for Comment 46.

49. Referee's comment: Page 6 lines 25-26. Please notice that Van der Schrier et al. (2013) explains values between 2 and 3 (-2- -3) as moderated wet (moderately dry). Since the authors are using their data is worth to be consistent with their definition. Page 6 lines 26-32. It could be valuable if the authors could show some statistics (significance) of these coincident events and if possible, described events shown in different sources that are not detected by the reconstruction. Author's Response: Many thanks for this suggestion. It was done. Please refer to Line 12-18 of Page 10 and Table 4 in the revision.

50. Referee's comment: Page 7 lines 5-6. Very interesting! Please consider explain

in the methods how this breaking point (1956) was established. Author's Response: Sorry for this. The breaking point was roughly determined visually. In the revision, we used the wavelet coherence method, and it was showed the breaking point was located around the 1940s. Please refer to Fig. 8.

51. Referee's comment: Page 7 lines 12-13. Please explain this claim, what is the importance of a dipole pattern? Is it meaningful? Is a dipole pattern contrasting to conditions previous 1950s decade? Page 7 line 13. Please demonstrate this claim with some tests. Author's Response: The dipole pattern means the contrast precipitation anomalies over south and north part of East China, this pattern receives much attention in China because it concerns the allocation of water resources. However, this issue is beyond the scope of this paper, so we did not explain it in details and only use the phrase "dipole pattern" to describe the distribution feature of precipitation. As shown in Figs. 10b, the dipole pattern was mainly occurred since the late-1970s. In contrast, the variation of precipiation anomalies before the 1970s were similar in the south and north of the Yangtze River (Figs. 10a). We attributed the unstable relationship between EASMI and scPDSI partly to the changed leading mode of EASM precipiation. Please refer to Paragraph 2 of Section 3.4.

52. Referee's comment: Page 7 line 14. Please change "and there are no significant spatial pattern changes" for "and there are no significant changes on the spatial patterns". Author's Response: Many thanks. We have modified the discussion in this part. Please refer to Section 3.4.

53. Referee's comment: Figure 1(a) units or information on the color bar are missing. Figure 1(b) Please add code or name of the stations, altitude can be also relevant. Since the EASM is relevant to the article can be good to indicate the spatial influence of this phenomena in the map. Figure 1 caption Page 17 line 3. Please change "Cycle" for "circles". "Monsoon atlas..." "...grid point triangle" please reword this sentence, since it is not altogether clear what the authors mean. The last sentence "and the range ..." please clarify that is a selection taken from Van der Schrier et al., (2013)

larger dataset. Author's Response: Many thanks. It was done.

54. Referee's comment: Figure 2. Please list the stations if possible, with the temporal extension. Author's Response: Many thanks. It was done.

55. Referee's comment: Figure 3. Please change "piece" for "section". These examples usually list the sample ID. Author's Response: Many thanks. It was done. Besides, we have moved this figure to the Supplementary material. Please refer to Fig. S1.

56. Referee's comment: Figure 4. Figure caption line 4. Please change "size" for "depth". Author's Response: Many thanks. It was done. Besides, we have moved this figure to the Supplementary material. Please refer to Fig. S4.

57. Referee's comment: Figure 5. Is this figure really relevant? Please write the names of the datasets. Author's Response: Thank you for your suggestion. We deleted this figure.

58. Referee's comment: Figure 6. This is a key figure for the study. It must be complete. Please add at least from April in the previous growing season, and I wish to suggest the authors to add 2 years before the current growth year. I assume these correlations are run with the standard chronologies which probably contain a significant amount of trends. A figure similar to this could be added with prewhitened tree ring and station data for each chronology. Author's Response: Thank you for your suggestion. Please refer to the new Fig. 4 in the revision.

59. Referee's comment: Figure 7. It is a very interesting figure. This can be completed with LWW and TRW information, to rule out the possibility that there has been loss of signal for LWW. The figure itself is good and illustrative but highlights that no lags were tested. Consider that this figure is made on time span after 1956, a date that the authors claim there is a change in the relationship of hydroclimatic variability and EASM. Thus this figure is restricted to "actual" conditions and not useful to illustrate past relationships. This is a subtle problem that challenges the temporal stability of the

relationship between the datasets tested (tree-rings and climate). Author's Response: Thank you for your suggestion. Here we used the Fig. 5 to replace the original Fig. 7. The Fig. 5 can display both the temporal stability and lags of the relationship between tree-ring parameters and MJJ scPDSI. The original Figure 7 was used to tested the temporal stability of the relationship between EWW and scPDSI, precipitation, and temperature, but not EASMI. This figure only indicated that EWW had much stable relationship with MJJ scPDSI than with precipitation and temperature. In addition, the reconstructed MJJ scPDSI can be validated by other hydroclimatic reconstructions and historical document records (Please refer to the Section 3.3 in the new revision). So, we think there is no problem in the reconstruction.

60. Referee's comment: Figure 8. Caption: a, "Raw time series" is this the raw chronology? or standard chronology "raw"?. b, 1st order difference over raw time series or standard chronologies? Please add some statistics on the figure, and analysis of residuals. Author's Response: Many thanks. It was done. Please refer to Fig. 6.

61. Referee's comment: Figure 9. It is demanding for the reader to guess all the time whether is scPDSI reconstruction or original data. Please make a denomination of the reconstructed index. Please add the authors in the corresponding axis of the charts. There is a lag between precipitation and scPDSI (d). This is not discussed at all, as the information in this figure is hardly integrated in the manuscript. Author's Response: Many thanks. The comparison with CRU scPDSI dataset were removed and the data was only used for calibration and reconstruction. Authors for corresponding reconstruction were added. The lag and mismatches were detailly discussed in revision. Please refer to section 3.3 and Fig. 7.

62. Referee's comment: Figure 10, caption. Please indicate what type of filter. Pearson correlation? Author's Response: Many thanks. It was done. Please refer to Fig. 9.

63. Referee's comment: Figure 11, please indicate what represent the color bar in the figure. Caption: Please indicate what type of filter, reconstructed scPDSI? source of the

datasets: "author et al (year)", Mean (?) temperature. This figure answers the question "is any change of atmospheric regime in the EASM area" not altogether relevant with the objectives, since the time domain is marginal within the reconstruction period. Are the rivers set as geographical reference? Author's Response: Many thanks. Since we found a decreased correlation between the reconstructed scPDSI and EASMI, we want to use this figure illustrate that decreased correlation may be associated with the change of leading EASM mode. The information of color bar, filter, rivers for reference were added. Please refer to Fig. 10.

64. Referee's comment: Missing in the manuscript: The more urgent motivation, local reconstruction, should have more local facts. More accurate description of the data, what was originally used for and why was it relevant to this one study. Often reports do not include such information, but since the dataset is small, it is worth to convince the reader of the robustness of the data. An overview table and figures with the chronology information: this because different tree ring datasets are compared. This comparison must be done in deep. An overview table with the climate data used, An overview of the data used for comparison (discussion) Better descriptions of the methods used (more accurate) Better descriptions of some of the datasets e.g. DWI. The relevance of the findings. Why are these results valuable? Please be so kind to explain. Axis information in the figures (color bars information) Text information within the figures. Acronyms to the specific datasets, two datasets can not be called in the same way. Please fix this detail. Author's Response: Thank you very much for pointing out these issues. We modified the introduction to clarify our motivation. The overview table and figures of the chronology information were added. Please refer to Fig. S4, Table S2 and Table S3. The table of climate data and reconstructions used for comparison can be referred to Table 1 and Table 2. Descriptions about DWI were added in the Line 18-22 of Page 5. The findings from the comparison between our reconstruction and other hydroclimatic reconstruction were detail discussed. Please refer to Section 3.3. Information of the axis, color bars were also added. The CRU scPDSI 3.25 data was only used for calibration and reconstruction, and the comparison was deleted.

Please also note the supplement to this comment:
https://www.clim-past-discuss.net/cp-2018-141/cp-2018-141-AC3-supplement.zip

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Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2018-141, 2018.
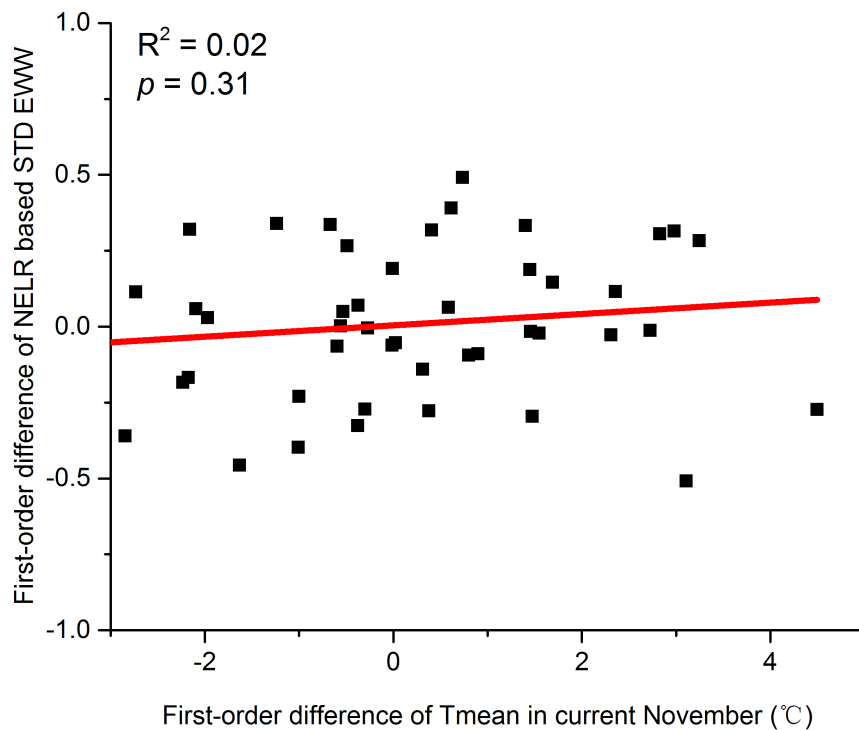
Discussion paper

Fig. 1. Linear regression between the first-order difference of the NELR based EWW STD chronology and the Tmean in November of the growth year