Dear Dr. Brunner,

We would like to thank you and the reviewers for your reviews of our manuscript "The relative importance of antecedent soil moisture and precipitation in flood generation in the middle and lower Yangtze River basin". We appreciate these insightful inputs that have helped to improve the quality of this manuscript. In response to the comments, we have made corresponding revisions. Our response to each comment is listed below in blue with the specific line numbers of the changes we have made. Again, we appreciate the time and inputs from you and the reviewers.

Best regards,
Qihua Ran

Reviewer #1
Accepted as is

Reply: Thank you.

Reviewer #2
Dear authors,
Thank you for putting great effort into addressing the reviewer comments. Several aspects of the manuscript are much clearer now, though there are still some aspects that need to be addressed before publication.

Reply: We appreciate the reviewer's comments, we have made our efforts to address the concerns and made corresponding revisions. Hopefully the reviewer finds our revision satisfactory.

I agree with reviewer #1 that the soil moisture estimates are highly uncertain. Some form of soil moisture evaluation is necessary since the water balance is rarely used for soil moisture estimates. I would like to point out that Berghuijs et al (2016, 2019) did not use the water balance to estimate soil moisture but to estimate timing of maximum precipitation excess. Additionally, they included a maximum soil moisture storage capacity value (Smax) set to 150 mm. Assuming an unlimited storage capacity as in your case is not physically sound. It furthermore does not allow you speak of "near saturated conditions" (L227), since the concept of saturation requires an upper limit. I agree that daily water balance might give an indication of catchment wetness, but I have not been able to find an evaluation how that compares to soil moisture values. Since you analysis relies heavily on the soil moisture being at least somewhat accurate, I recommend at least minimal evaluation of the values.

Reply: We have to admit that there are many uncertainties in the soil moisture estimates, and may not always represent the actual condition at event scale. However, due to the limit of observation, it is difficult for soil moisture assessment: local measurements could not provide

representative observation of soil moisture at catchment scale for our study region while remote sensing images can only provide soil moisture at the top 5cm (Babaeian et al 2019). While sophisticated models could be applied for the soil moisture estimation, there are also substantial uncertainties (Zaherpour et al., 2018). To reduce the biases that may be caused by this simplified estimation, we have replaced the normalized soil moisture with percentile soil moisture and used the percentile to indicate the catchment wetness. In this way, the water balance was used to estimate soil moisture which was then ranked to get the relative wetness in the study time period. Instead of the actual soil moisture level, it is the seasonal trend that is used in SPR calculation. This is similar with Berghuijs et al 2016, 2019 where water balance was used to estimate soil moisture which was then used for soil moisture excess / precipitation excess estimation to derive the timing of maximum events.

To validate our results, we collected the 0-200cm soil moisture from the China Land Data Assimilation System (CLDAS) provided by China Meteorological Administration (CMA) (Wang & Li 2020). 37 catchments covering a range of climate and topography were selected for comparison (Figure A1). Since this dataset only has soil moisture data from 2008, the mean percentile of antecedent soil moisture was calculated from 2008 to 2016 based on the CLDAS soil moisture. This was then compared with the mean percentile based on water balance as in the manuscript (Figure A2). As we can see from Figure A2, the scatters fall around the 1:1 line, that is, the mean percentile calculated from water balance are close to the mean percentile from re-analysis soil moisture. This is consistent with our discussion in Section 4.4 that 'averaging through long-term records would be less impacted by the simplification in estimation (Berghuijs et al 2019; Zhang et al 2019)'. Due to the length of CLDAS dataset, we only averaged within 9 years, for the at least 25 years records used in our study, it is likely to be less scatter. This is just a minimal evaluation of the values, yet, given the goal of this study, we think the averaged percentile of antecedent soil moisture based on the water balance model is acceptable for the purpose of this study at the mean annual scale.

As for the storage capacity, since we used the observed streamflow data for the water balance estimation, we didn't set an upper limit in the estimation of soil moisture. We calculated the Smax for our study catchments, they are mostly between 100mm and 300mm (Figure A3). According to the Harmonized World Soil Database (Nachtergaele, van Velthuizen, & Verelst, 2009), most of our study catchments belong to AWC (available water storage) class 1, that is 150mm/m (Figure A4). The soil depth usually varies between one and three meters; thus, the total soil water storage would be between 150mm and 450mm. Our estimated Smax is within the range. Since the study catchments are all humid ones, we assume that the maximum soil moisture level is close to saturation state. However, we agree with the reviewer that it is not rigorous to say 'near saturated condition' without explicit storage capacity. We have replaced the saturation expression with 'wetter' condition in the manuscript and added this discussion in the limitation section (please see lines174-175, 232-233, 247, 294, 308, 340, 432, 473 – 479). Hopefully the reviewer finds our explanation satisfactory.

Figure A1: Map of the 37 selected stations used for comparison.

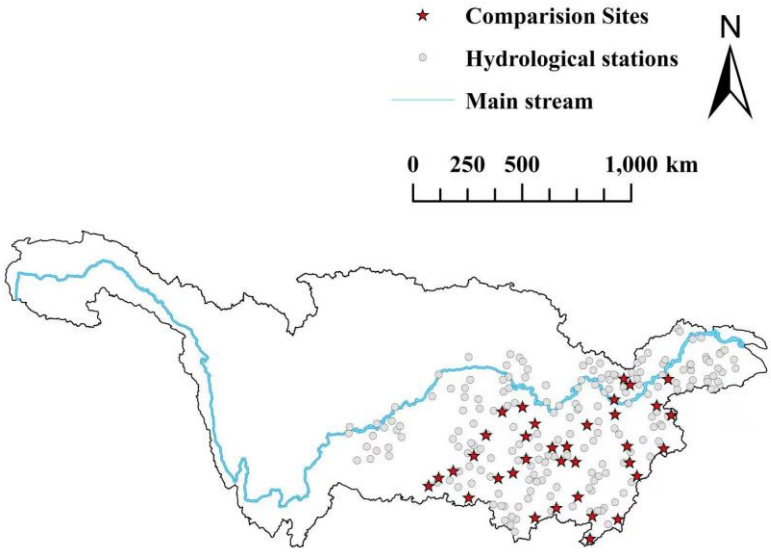


Figure A2: Comparison between the mean percentile of antecedent soil moisture in our work and the percentile of antecedent soil moisture from re-analysis dataset CLDAS. The red line is the 1:1 line.

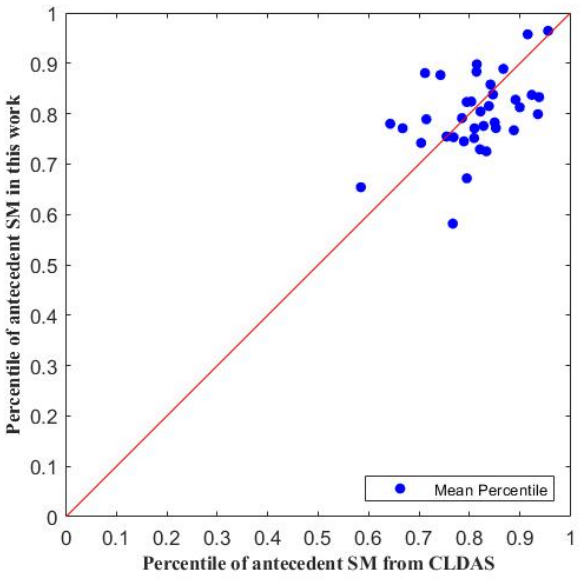


Figure A3: Histogram of Smax across the study watersheds

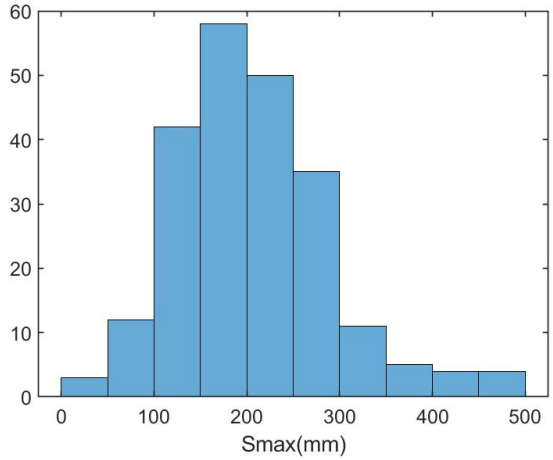
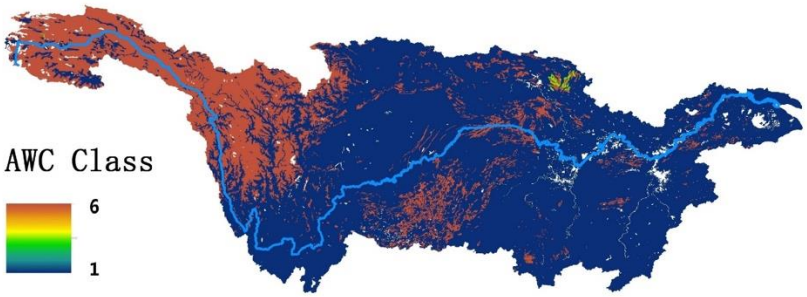


Figure A4: The available water capacity (AWC) class in the middle and lower Yangtze River basin.



Wang, Y. and Li, G. (2020). Evaluation of simulated soil moisture from China Land Data Assimilation System (CLDAS) land surface models, remote Sensing Letters, 11 (12), 1060 – 1069.

Nachtergaele, F., van Velthuizen, H., & Verelst, L. (2009). Harmonized world soil database, Version 1.1. Rome: FAO/IIASA/ISRIC/ISSCAS/JRC.

Thank you for including Figure S1 in the Supplement. It is more clear what time period the data you are using is from now. Though I am still wondering what the implications for the analysis is, when the data is used from two very different time period. Have you done an analysis for each time period and compared the results to see if there are any significant differences in flow regime?

Reply: We did compare the timing and magnitude of AMFs in the two time periods (Figure A5, A6). As we can see from the two figures, there is no significant difference between the two time periods. However, this does not necessarily mean that the AMFs are similar in the two time periods. Since there are only 10 years records after 2000, it may not be long enough to provide representative information of recent years. To avoid the uncertainties that could be caused by the short records, we considered all the years we have to derive an averaged value. We agree with the reviewer that it would be interesting to see whether there are any differences between the two time periods. In the future work with longer data records and/or higher

temporal resolution (i.e., event scale), we will try to compare the results to see if there are any significant differences in flow regime. Since the goal of this work is to present a framework to evaluate the relative contribution of rainfall and soil moisture in flood generation, we would focus on the mean annual scale for now. We have included this discussion in conclusion section for future work (please see lines 517-520). We hope the reviewer finds our explanation sufficient.

Figure A5: The mean timing of annual maximum floods in the years (a) before 2000; and (b) after 2000.

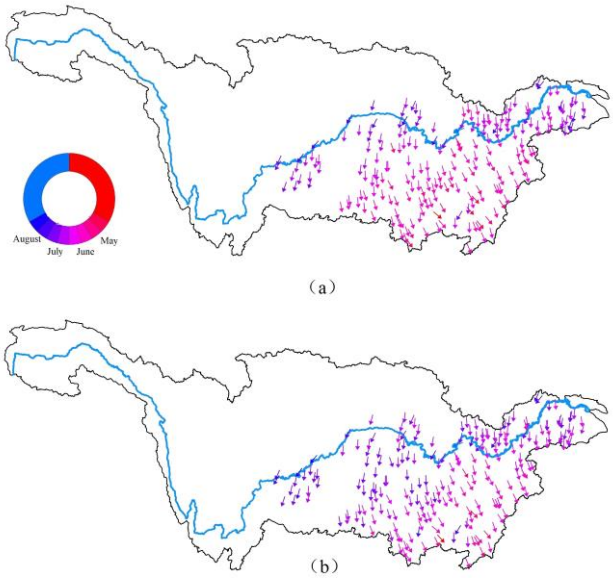
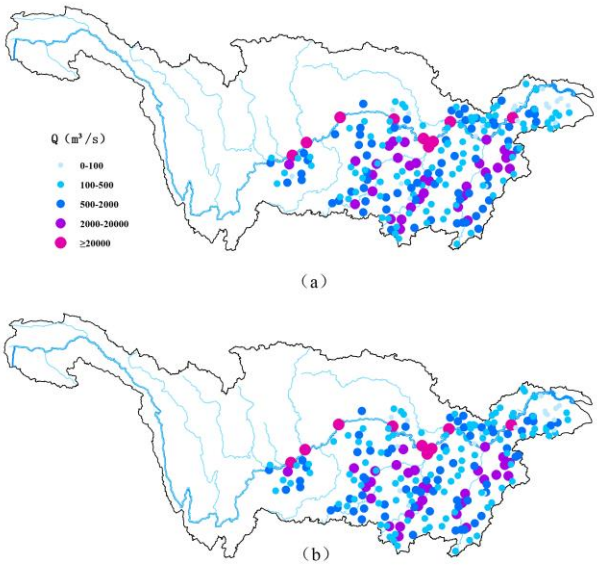


Figure A6: The mean magnitude of annual maximum floods in the years (a) before 2000; and (b) after 2000.

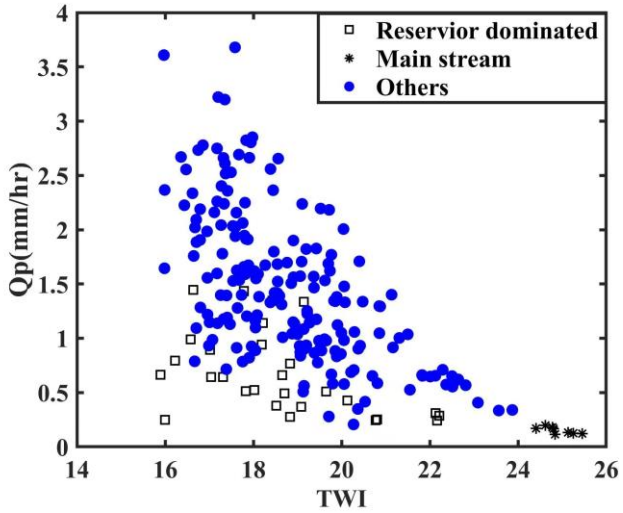


Section 4.3. Since TWI is correlated with SPR and SPR is correlated with AMF, are TWI and

AMF correlated as well? E.g. does SPR have more explanatory power for AMF than TWI? Please show these results, since you main implications (SPR can be used for predictions in ungauged basins) relies on SPR contributing to AMF estimation. An interesting implication which you only briefly mention in the conclusions is using SPR for climate change impact assessment on flood generation across the different watershed types. This is only a suggestion, but it would be great if you could elaborate on this in the implications section.

Reply: Yes, TWI and AMF are also correlated (Figure A7), the pattern is similar with Figure 7 in the manuscript. The reason we present the correlation between SPR and AMF instead of TWI and AMF is that, TWI is constant for each catchment, while SPR could vary with the study time scale. SPR could be estimated at mean annual scale as we did in this work, and could also be calculated at annual scale and event scale with high resolution data. Since the goal of this study is to present a framework to evaluate the relative contribution of rainfall and soil moisture in flood generation, the correlation between SPR and AMF is present as it could be applied to event scale in future analysis. In our first version of manuscript, we discussed a bit about using SPR for climate change impact assessment on flood generation in the implication section; however, due to the lack of assessment at event scale and other watershed types, we removed this discussion following the reviewers' suggestion. We would further investigate the correlation between SPR and flow peak at event scale in the future work with more detailed data of higher resolution. We have included the figure of TWI and AMF in supplementary (Figure S2) and this discussion in Section 4.2 and 4.3 (please see lines 377-378, 412-414). Hopefully the reviewer finds our revision satisfactory.

Figure A7: Scatterplot between topographic wetness index (TWI) and area weighted annual maximum discharge (Qp).



L61/62: In what aspect is the analysis by Yang et al 2019 and Yang et al 2020 limited? This is an important aspect to explain how your study contributes further than the detailed causative classification of Yang et al, 2020. I recommend that you discuss your findings in comparison to the flood types found by Yang et al, 2020 as well. In contrast to your findings they attribute

a much higher component of the floods in the Yangtze river basin to long and short rainfall instead of wet antecedent conditions.

Reply: Although this work analyzed the floods across catchments in China as Yang et al 2019, and Yang et al 2020, we have different focuses. Yang et al 2019 is more focused on the temporal change of flood series and used GEV distribution to quantify flood peaks. While Yang et al 2020 is more focused on the classification of dominant flood type. Instead of the causative classification, we are more interested in the relative importance of soil moisture and rainfall (SPR), furthermore, we attempted to link it with catchment characteristics as well as flood characteristics. SPR could provide us a more straightforward representation of the contribution from soil moisture and rainfall, which is more flexible and may be applied to event scale in future with more detailed data.

As for the differences in the flood attribution, according to Yang et al 2020, antecedent soil moisture also contributes to flood generation for the floods in the Yangtze River basin (Fig. 6 in Yang et al 2020), though the contributions of long and short rainfall are more dominant. Secondly, since we used different criteria to quantify the contribution of antecedent soil moisture and rainfall, it is likely, the contribution of long rain was considered as antecedent soil moisture in our study. These may be the reasons that our results are not exactly the same as theirs. We have included this discussion in the introduction and discussion section (please see lines 61-63, 439-444). We hope the reviewer find our explanation satisfactory.

L165: "would not" instead of "wouldn't"

Reply: We have made the suggested change, thank you (please see line 168).

L193/194: This might just be a misunderstanding, but does that mean you calculate the mean annual maximum flood date and then use the day before the mean date for soil moisture? That would not be a very informative soil moisture value. If that is not the case, what is the difference between "soil moisture at the day before AMF" and [soil moisture at the] "day before the event of AMF"? This confusion might be due to the previous sentence ("Since almost all…") which seems to be incomplete.

Reply: We are sorry about the confusion. We calculated the soil moisture before each AMF and averaged across years. As for the difference between the 'soil moisture at the day before AMF' and 'soil moisture at the day before the event of AMF' is that, the day of AMF is the day of the flood peak while the event of AMF is the start date of the event whose flow peak is AMF. Since an event could last for several days, 'the day before the event of AMF' could be several days before the 'day before AMF'. We have clarified this in the manuscript (please see lines 193-194, 196-197). Hopefully the reviewer finds our revision clear now.

L200/201: The rainfall percentile does not represent "the relative magnitude of rainfall in flood generation" but the relative magnitude of the event across time.

Reply: Thank you for the suggestion, we have changed it into 'the relative magnitude of the rainfall events across time' (please see line 206).

L427: I do not see the relevance of the reference Zaherpour et al, 2018. They discuss model-based uncertainties for mean and extreme runoff, not soil moisture.

Reply: We have deleted the citation now (please see line 435). Thank you.

L460: Capital letter needed at the start of the sentence.

Reply: Sorry about the typo, we have corrected it now, thank you (please see line 475).

Figure 6: The caption needs to be updated to reflect the changed x-axis of plot b ("(b) topographic wetness index").

Reply: Thank you for pointing out this, we have changed the caption of plot b, thank you (please see line 871).

Yang, W., Yang, H., and Yang, D. (2020). Classifying floods by quantifying driver contributions in the Eastern Monsoon Region of China. Journal of Hydrology, 585:124767.

Reviewer #3

I was previously the third reviewer. The authors have thoroughly addressed all my review comments and I wish to express my gratitude to the authors for doing so. I think the rewording and recalculation of the SPR metric is most useful. I wish to also reiterate that the paper reads well, and I feel the figures convey the narrative well and I will be using the way the authors presenting their results as a template for my own research. I have some minor comments and typos which I hope will assist.

Reply: We are grateful for the constructive comments from the reviewer and we have made further revisions corresponding to the comments. Hopefully the reviewer finds our revision satisfactory.

Line 136: I think a future reader might wonder why the data from 1990-2007 is missing, a sentence explaining this here might be nice.

Reply: Thank you for the reminding, we didn't get the data from 1990 to 2007 from public online repository, we have included a sentence for explanation (please see lines 138-139), thank you.

Figure 5: The circle size needs a scale on the figure.

Reply: We have added the circle size to Figure 5 (please see Figure 5), thank you.

Line 310: I think you can also make the point that flood is less influenced by soil moisture when it is more severe (larger). This is a point made in Brunner et al (2021) and Wasko et al (2022) – no need to cite these papers here but just thought it is a very useful observation from Figure 7 that should be mentioned here. You could also mention it in the conclusions.

Reply: Thank you so much for the suggestion, we have included this point here (please see lines 318-319), thank you.

Some typos and suggestions:

Line 43: “projection” -> “projections”

Reply: We have corrected it now (please see line 43), thank you.

Line 61: “Some research was conducted in China recently, though is still limited”

Reply: We have made the suggested change (please see lines 61 – 63), thank you.

Line 68: “even little” -> “with few”

Reply: We have made the suggested change (please see line 69), thank you.

Line 90: “Section 3”

Reply: Sorry about the typo, we have corrected it now (please see line 91), thank you.

Line 130: “using the Theissen polygon method”

Reply: We have made the suggested change (please see line 131), thank you.

Line 197: “to seven days before”

Reply: We have added “before” in the sentence (please see line 202), thank you.

Line 330: “straightforwardly”

Reply: We have changed it to ‘straightforwardly’ now (please see line 336), thank you.

Line 390: “along the main stream”

Reply: We have added “the” in the sentence (please see line 396), thank you.

Line 432: “before the event”

Reply: We have made the suggested change (please see line 445), thank you.