

This study investigated the cause of the large daily flow fluctuations in the Mekong River. After reading the manuscript, I have a strong feeling that the manuscript needs to be carefully revised and reviewed. Precise and clear writing is important and sufficient to report the new findings to our scientific community. Especially for the figures, some irrelevant paragraphs and unclear descriptions would confuse the readers. The authors have done a lot of work to support their findings. But the current version still needs to be improved.

Main comments:

1) I concur with the previous reviewer's assessment that the author's literature review of this paper requires substantial supplementation with recent content, particularly the modelling and simulation of a series of hydrological and hydrodynamic models conducted around the Lancang-Mekong River Basin. Given that 2024 has already commenced, the modelling conducted around the LMRB has been refined to the day or even the hour. In light of the above, it is imperative that the author conducts a comprehensive synthesis and refinement of existing research, elucidating the pivotal contributions of this study. It should be noted that these works should not only be carried out in the discussion, but also require substantial supplementation and modification of the introduction.

2) The authors hope to estimate the time it takes for large daily changes in upstream rivers to affect downstream rivers, but with the large-scale construction of reservoirs and changes in river dynamics, the results of this study may not provide the expected reference value. Similarly, the authors' claim that "three aspects extend previous research" is difficult to achieve:

a) "Quantitative assessment of the regional contribution to abnormal downstream water level/flow changes". Given that there are about 500 reservoirs in the basin, I doubt the feasibility of this vision;

b) "Quantifying the propagation of upstream river flow changes to downstream sub-basins", as above, the presence of many reservoirs has significantly altered the river propagation process. Although the impact of reservoirs on mainstream flooding during the wet season is small, it should be noted that reservoir operations dominate mainstream water level changes during the dry season in the basin, and large-scale water conservation and diversion projects on tributaries have permanently altered river dynamics in these areas.

c) Due to the lack of consideration of the reservoir impact in the model, this study may only be applicable to the LMRB before 2009, and it is difficult to provide an in-depth understanding of climate impacts. Figures 3 and 4 confirm this view. The author can only show the time series verification results before 2000, and lacks the evaluation of the model effect on the tributaries and mainstream in the middle and upper reaches after the large-scale reservoir development after 2008.

3) This study may not be applicable to current LMRB. Given that this manuscript

submitted to HESS, I am a little unsure what new insights this paper can give us regarding the LMRB, especially considering that the basin has been undergoing large-scale dam construction for 20 years. Could the authors consider looking at other areas where dam construction has not yet begun, to increase the the validity of the study?

4) It should be pointed out that the author's model can obtain such a high NSE coefficient, which is mainly due to the input of the actual streamflow of the JH station. In fact, if the JH flow data is used directly to evaluate the CS flow data without considering the confluence runoff in the JH-CS sub-basin, its NSE will reach more than 0.85. However, I can't find any description of the JH station flow in the article. Considering that the streamflow data of JH station has been publicly released by the Chinese government, it is necessary for the author to make a detailed explanation.

5) As far as I know, THREW is not a gridded distributed model, but a model for lumped confluence in small catchments. How could this driven the Delft-3D model? I can't imagine flattening the confluence generated by the lumped model on an uneven DEM and expecting it to produce adequate confluence results.

6) I was unable to open the website <https://portal.mrcmekong.org/home> successfully, whether using the network service from German, Japan or China. Perhaps the author could consider uploading the data to such as <https://zenodo.org/> for safekeeping.

Other comments:

7) At line88, Firstly, the official name of this basin is the Lancang-Mekong River Basin, with upstream Lancang River and downstream Mekong River. Secondly, the length of the river claimed by the author is questionable. Finally, the number of Chinese reservoirs is more than 11 and needs further verification. Considering that the collaborators include a large number of senior Chinese experts in hydraulic research, it is unacceptable to make mistakes in these details and data.

8) In Figure 1, what is “the Tonle Sap Lak”? It is recommended that the author carefully checks for the spelling and grammatical errors in the paper, as similar situations occur frequently.

9) In Figure 2, I don't think it's a good idea to use both red circles and triangles for labeling. I can't distinguish the tributary station and the mainstream station at all. Besides, I think there should be a space separating the “Delft3D”.

10) Line 220, "Comparable levels of accuracy are achieved for the years 2019 and 2020, as detailed in the SM, Section 3". My understanding is that you cannot prove the overall model usability by showing only a part. ST is located downstream and has a large main stream flow, making it less affected by reservoir operation. Therefore, using flow velocity assessment at a monthly scale during the rainy season can give

better results, but this cannot prove the applicability of the model for basin-wide flow assessment after 2010.

11) The results in Figure 8 seem to be based on the comparison between the actual observed flow and the natural flow simulated by the model, or did the authors include a simulation of reservoir operation in the model? I am not sure if I missed the part about the reservoir being set up in the model. It is recommended that the authors explain how the results were obtained.

12) In Figure 9, I don't think it's a good idea to remove the year labels on the x-axis of the time series graph, as this only makes the figure harder to understand. Also, what is "recieved rainfall"? It is recommended to avoid the use of rainfall and to use precipitation uniformly. I suggest that the author consider further detailed checks on the grammar, fonts, font size, etc. of the full text and images. The current version has too many errors.

13) In Figure 10, Same as above, what is "contrinution"? I can understand that the author has a few singular and plural errors or tense problems in the manuscript. However, repeated typing errors in important figures are unacceptable.

14) In Figure 11, Please add dots of corresponding colors on the basis of the lines in the legend, which can make the image more readable.

15) In Figures 6 and 9, I am not sure how R^2 is calculated, what data are used? Please explain in detail.

16) Sources of meteorological soil and vegetation DEM data used in modelling must be listed in the main text in a clear and detailed manner. Layered citations are unacceptable.