Hydrol. Earth Syst. Sci. Discuss., author comment AC1
https://doi.org/10.5194/hess-2021-360-AC1, 2022

Reply on RC1

Dung Trung Vu et al.

Author comment on "Satellite observations reveal 13 years of reservoir filling strategies, operating rules, and hydrological alterations in the Upper Mekong River basin" by Dung Trung Vu et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-360-AC1, 2022

Vu et al. write about remotely sensing the filling strategies and operating practices of the Upper Mekong Basin cascade in China. The manuscript highly interesting for actors working in the region. Researchers, NGOs, and state actors in the Lower Mekong Basin should all benefit from understanding the practices of cascade operation in China. The manuscript is very well prepared, and I anticipate it will be highly influential in the Mekong context. I recommend publishing the article subject to some moderate revisions:

Response: Thank you for the positive feedback as well as the useful comments for improving the paper.

A. Regarding methodology

1. Figure 3 presents the workflow in estimating elevation (water level)-storage-area curves. The text tells us that surface area is estimated for every height with one-meter gaps, based on the 30m SRTM DEM. Why, then, is storage estimated with the trapezoidal approximation in Eq. 1? The DEM and elevation bands allows you to directly compute the storage volume, since you already know which pixels fall into which elevation band, and you know the elevation of each pixel. The storage volume is then easy to compute. The trapezoidal approximation is, of course, useful for Manwan Dam.

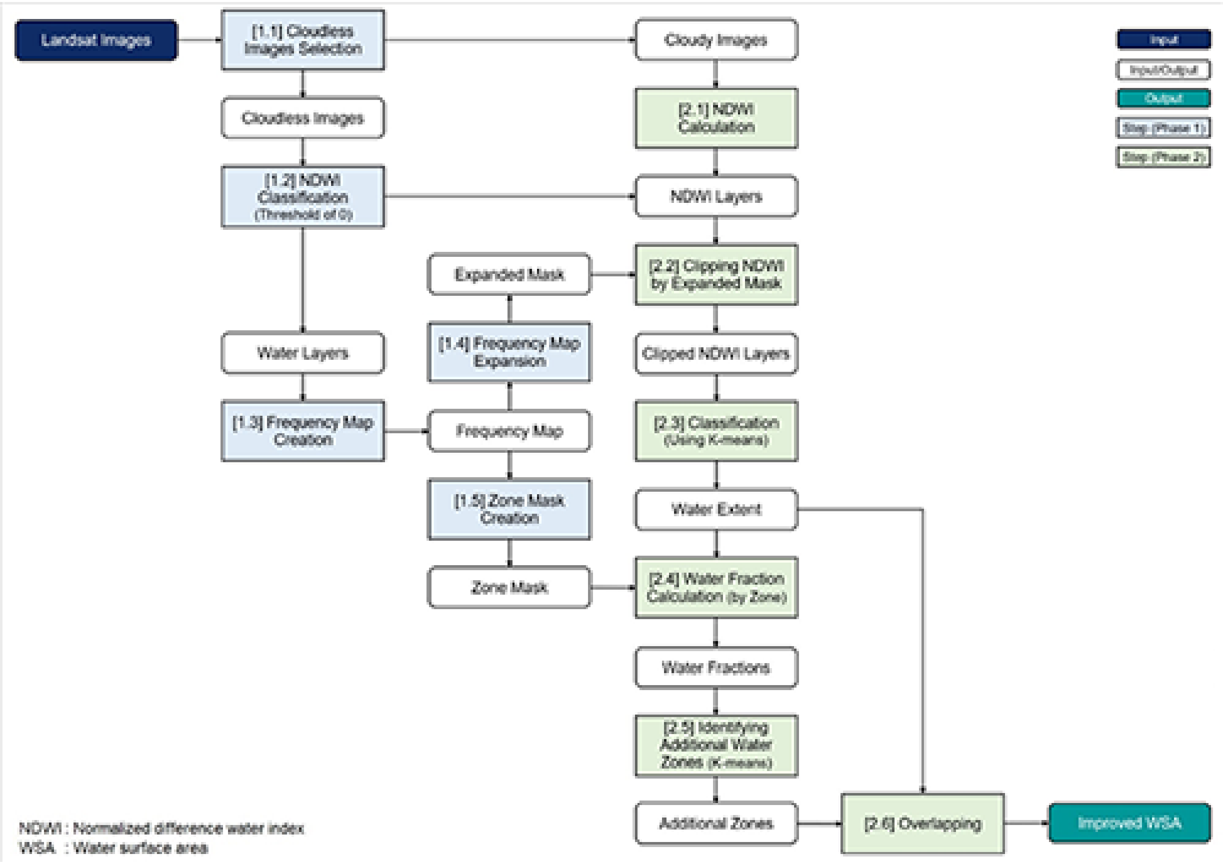
Response: This is a good point—thanks for raising it. We decided to use the trapezoidal approximation for the following reasons. First, we would like to minimize the differences in data processing for all reservoirs, including Manwan. Second, the E-A curves estimated from the DEM are well in agreement with the water level observations (from altimetry data) and water surface area (from Landsat images). In other words, these curves are validated. Therefore, we can confidently develop the E-S and A-S curves from the E-A curves using the trapezoidal approximation, which was widely used in the previous studies (e.g., Gao et al., 2012; Bonnema and Hossain, 2019; Li et al., 2019; Tortini et al., 2020). Meanwhile, we do not have observed storage data to validate the E-S and A-S curves estimated directly from the DEM.

To corroborate the aforementioned points, we compared the results obtained with the two methods. The differences in storage corresponding to each water level in the variation range are not more than 1% (for Jinghong, Manwan, Miaowei, Huangdeng, and

Wunonglong) and 2% (for the other reservoirs). If we will have an opportunity to revise the manuscript, we will include the aforementioned explanation as well as this additional analysis.

2. The methodology regarding determining the surface area from the satellite images seem complex and I'm having trouble following the entire procedure, although the procedure is also presented in Figure 4 is difficult for me to follow because the analysis has many paths, and for the conclusion I also have to turn around and move upwards in the path. Could you reorganise the figure and leave more space between different paths? E.g., the more space between the path through 1.1 -> 1.5 and 2.1 -> 2.4, so that they are clearly separate? And for the final loop between 2.4 -> 2.6, more space between the boxes. And if it is possible, the outcome could be in the bottom. The general direction in the figure is from top to bottom, but the reversion makes it somewhat difficult to follow. Further about this, I had to read the textual explanation several times before fully comprehending. I'll leave it up to you to decide whether the text needs clarification, as I admit my miscomprehension might be on me alone.

Response: We agree with you, and we will reorganize Figure 4 following these suggestions. A revised version of the figure is reported below.



3. Uncertainty quantification. The methodology suggests that there is uncertainty in water pixel identification. In Step 2.5, all pixels within the water cluster are assumed to be water. But substantial number of pixels in the water cluster in fig 6b should not be assumed to be inundated, particularly in the first two zones with less than 80% of pixels inundated. Instead, this reflects a possibility to quantify uncertainty in your methodology.

What are the underlying elevation values in those zones which fall into water pixels (and non-water pixels)?

Response: We grouped the pixels into 50 zones by their inundation probability based on the frequency map (calculated with the cloudless images). Since the pixels in the same zone have the same (or very similar) inundation probability, there can be two scenarios at each observation time: (1) they are both non-water pixels, or, (2) they are both water pixels (even when the water fraction of that zone is less than 100%, due to cloud cover). When a zone is identified as a water zone (by k-means clustering in Step 2.5), all pixels in that zone are converted to water pixels. That is the reason for converting all pixels from Zone 14 onwards to water pixels (Figure 6b).

There can be a small error in Zone 14--which contains pixels with 26-28% inundation probability--when the threshold of inundation probability is not exactly 26% (e.g., 26.5%, 27%, ...). We can increase the performance by dividing the frequency map into a larger number of zones, but this requires a larger number of cloudless images. More important, our results in Figure 8 show that the WSA estimation algorithm (with 50 zones) works well enough.

A final comment concerning the elevation values: because of the aforementioned procedure, we do not need to calculate the underlying elevation values in the zones that fall into water pixels (and non-water pixels). We will ensure that all these aspects are clearer in the revised version of the paper.

4. The above leads me to the question: why such a complex procedure? A simpler alternative could be:

(1) Identify the water pixels as you've done up until step 2.3.

(2) With those water pixels you should be able to extract the elevation values at the boundary of the water feature, this should give you a range of elevation values at the reservoir shoreline.

(3) This is the range of possible water elevations (and thus area and volume) which would be easy to communicate in figures too.

This method would not require a cloudless image, similarly to the one you're using in the manuscript, but cloudy images would have a smaller number of values of elevation at the boundary. I admit that I've not done this and so I don't know what complications there may be, and therefore I do not require that you should do this. But I'd like to see a justification for your choice of method over this simpler alternative.

Response: We considered the method that you suggested at the beginning of our work. However, the reasons outlined below have prevented us from using it:

- *First, it is not possible to extract true elevation values from water pixels derived from Landsat images when the water level is below the level corresponding to the SRTM-DEM observation time. In our work, the matter applies to Manwan.*
- *Second, it is difficult to identify the starting point of water surface of the reservoirs in the Lancang. These reservoirs have a long and horizontally narrow shape (see the length in Figure 2 and the shape in Figure S1), so there is not a sudden opening point like in more "regular" reservoirs. Instead, the starting point of water surface moves along the longitudinal direction of the reservoirs. In low flow conditions, the part above the starting point (having higher values of elevation) behaves like a river instead of a portion of the reservoir. In sum, it is not easy to identify the starting point from Landsat images—and this in turn affects the range of elevation values at the reservoir*

shoreline. This process is even more complicated when dealing with cloud cover and reservoir branches, such as Xiaowan (see Figure S1).

Because of these reasons, we developed the idea of identifying the misclassified water pixels (due to cloud cover and other disturbances) based on their inundation probability, which was used before by Gao et al. (2012) and Zhang et al. (2014). The method we used can work well with reservoirs like Manwan. Figure 8 shows that water surface area estimated by our algorithm is well in agreement with the one calculated through the altimetry data.

5. Your overall methodology is similar to that of the Mekong Dam Monitor. I'd like to see a short comparison of how yours differ from theirs but in more detail than just their choice of using Sentinel and thus having a shorter timeseries (line 54).

Response: Yes, the overall methodology is similar, in the sense that both our methodology and the one used in the Mekong Dam Monitor (MDM) are based on the idea of extracting the water extent of the reservoirs from satellite images and then converting it into water level and storage by using the information from DEM data. However, there are a few differences:

- *First, we use the image improvement algorithm, which is important and necessary because it enables us to extract the information on reservoir storage from Landsat images for a long period (2008-now). Meanwhile, to avoid the cloud contamination in satellite images, MDM looks to other remote sensing product, Sentinel-SAR (Synthetic Aperture Radar), which can "pierce" through clouds. However, Sentinels were launched recently (in April 2014), so the information before that time (including the construction and filling periods of five reservoirs on the mainstream of the Lancang) cannot be revealed.*
- *Second, with the water extent estimation provided by our algorithm, we directly infer water level and storage through the E-A-S curves estimated from the DEM. Meanwhile, MDM calculates the average elevation at the reservoir shoreline, then converts it into storage. This way may not work well for all Landsat images (please refer to our previous response).*
- *Finally, to strengthen our results, we make use of water level from Altimetry data (where available) to validate the results obtained by processing the Landsat images.*

We will make sure that all these points are clearly stated in the revised version of the paper.

B. Regarding Results

6. Despite my remarks of the methods, the results section is impressive and very useful.

Response: Thank you.

7. Section 4.3.2 gives additional theory and methodology with the storage equation, computing evaporation, VIC-Res related methodology etc. I would find it clearer if this methodology would be explained before the results section. The same applies also for indicator of hydrological alteration in section 4.4.

Response: When writing the manuscript, we were a little hesitant to do that, because Section 3 ("Methodology") is already rather long. However, it is true that some elements presented in Section 4.3.2 (and 4.4) should be presented earlier, so we plan to do that in the revised version of the manuscript.

8. I find the indicator of hydrological alteration very clever, as it does not require

estimating inflow to the reservoir. However, it does require estimating the streamflow originating from below the cascade. Räsänen et al 2017 estimate the annual inflow to Jinghong to be 58km3 (1840 m3/s), while Chiang Saen annual runoff is 85.5km3 (from observation timeseries). This is a substantial difference, and needs to be taken into account in computing I. With the VIC-Res already set up, it should not be a big deal. It will be interesting to see how index I changes after accounting for this.

I ask you this because your study deals with a highly political issue, and it is necessary to have good evidence for the statement that China did not change their operating practices despite a severe drought downstream. It would therefore be important to provide validation for the performance of VIC-Res e.g., in the supplementary materials. You point to Dang et al 2020, which gives some validation but does not include the period with Xiaowan and Nuozhadu, and it isn't easy to say how is the performance during the wet season, the time when reservoirs are filled.

Response: At this stage, the only validation of VIC-Res we provided is a comparison of the simulated and observed storage of Nuozhadu and Xiaowan (Figure S5). We agree that such validation is not very comprehensive (especially when seen in light of the geo-political implications of our findings), so we will proceed by extending it. In particular, we plan to report a quantitative comparison of the observed and simulated discharge for the study period as well as a comparison of the observed and simulated storage for the other reservoirs. Considering the length of the manuscript, we will likely add this part to the Supplement.

As for your first point, our understanding is that the recommendation is to calculate the index I not only at Chiang Saen, but also downstream of Jinghong. Is that correct? If yes, we will proceed by calculating the indicator at this location (and report the result either in the manuscript or SI).

C. Regarding Data

9. As my last point, I'd like to invite the authors to deposit their results in some open repository (e.g., Zenodo?). The methodology is explained in detail which allows for replication - but since you've already done the work, it would be a great service to the Mekong community to have access to the data - i.e., the water level-storage-area timeseries, maximum and minimum reservoir shapes etc. This would improve the usefulness of the work even further.

Response: Our results (E-A-S curves and storage time series) and code are already available online, as stated in the "Code and data availability" section (https://github.com/dtvu2205/210520). This said, we reckon it is better to archive the data and get a corresponding doi, so we will deposit them on Zenodo or Hydroshare.

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