Comment on hess-2021-360
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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, NGO's, 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:

Regarding methodology
1. Figure 3 presents the workflow in estimating (water level) elevation-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.

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. Fig. 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.

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 amount 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 uninundated. 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)?

4. The above leads me to the question, why such a complex procedure? A simpler alternative could be to 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, and 3) this should give you a range of elevation values at the reservoir shoreline. 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 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.

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).

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

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.

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 Jinhong to be 58km3 (1840 m3/s), while Chiang Saen annual runoff is 85.5 km3 (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.

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.

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