Review of “Investigating the environmental response to water harvesting structure: a field study in Tanzania”

Article by J.A. Eisma and V. M. Merwade. Review by R. W. Hut

The authors investigate three sand dams in Tanzania on macroinvertebrate habitat, vegetation, streambank erosion and local water table. Data like this is hard to gather and much needed to evaluate the impact of measures such as sand dams. This dataset is in itself a valuable addition to the literature on sand dams. However, I have major concerns with the conclusions that the authors draw based on their data that, in my opinion, need to be addressed before this work can be published in HESS. My concerns focus firstly on the statistical representativeness of the research and the claims that the authors make on sand dams in general based on the research they did on three individual dams. Secondly, I believe the modelling effort is flawed and the conclusions from the modelling (mainly on evaporation) are inaccurate.

All in all I would either suggest major revisions or ask the authors to split this paper in a data paper and an analysis paper. While the analysis paper would need considerable work, the data paper can be published almost as is and is a valuable addition to the scientific literature on sand dams, in my opinion.

I use P4L24 to point to Page 4, Line 24 of the manuscript.

Statistical representativeness

In their introduction the authors start by claiming that previous work “do not tell the whole story of sand dam impacts and this had created a false perception.” P2L18. They continue to dismiss previous work as anecdotal and not scientific by stating that “This study aims to respond to anecdotes with science” P2L20. The authors set their goal clearly: “This diversity of features ensures that the sand dams included will be representative of the sand dams found throughout the region, and this study will therefore create a holistic understanding of how a sand dam interacts with the local environment.”. Claiming that this can be done be examining only three dams out of the 1500 dams in sub-Saharan Africa [P2L5], even when chosen carefully, grossly underestimates the differences among the dams.
It is the very nature of geoscience in general and land surface studies like hydrology in particular that every locale is different. Isolated experiments in particular locations will never

draw the entire picture of the (luckily) very diverse land surface. In their conclusions the authors contribute dam failure or success on specific attributes of the dams they study. For example: the increase in vegetation was higher at the Soweto dam and the Soweto dam resides in a relatively flat area P11L27. The authors extrapolate this to the conclusion that “to maximize positive impact of a sand dam on local vegetation, sand dams should be build in flat areas.” Since there are far more factors that influence success of a dam, some of which the authors touch upon, this is a way too broad statement. The observation that for this particular dam, the local vegetation is positively affected is a valuable observation. The hypothesis that in this particular case that is caused by the relative flatness of the area is a valid hypothesis that future researchers can test if it holds in a broader context.

I want to ask the authors to skim through their manuscript for places where their conclusions and claims extend beyond the data they have gathered and adjust their manuscript to bring conclusions and data in line with each other.

Modelling efforts and conclusions

The authors use a water balance model to model how much water the dams are are losing over time. I have several issues here:

1. The model calculates Qout based on the other terms, it therefore accumulates all errors in Qout, including errors because of terms not included in the model
2. I assume from figure 7 that the authors start the dams “full”. This is not made explicit in the article.
3. The inflow term 0.038CP(t) accounts (I think, not made clear) for the amount of rain water that falls on the dam itself and is subsequently stored? I would argue that during a rain event all water from upstream would be routed over the stream-bed thus re-filling it. The 0.038 term from Aerts 2007 relates to the total amount of water a sand dam saves from annual discharge to see if dams have an impact on downstream water availability. This factor can not be used as the authors do.
4. The 0.15 factor from Kumar 2018 relates to the percentage of evap that is canopy evap in the Noah LSM, which, if I recall correctly, was not calibrated for the region that the authors use it for. I would guess that on the African regions of interest here, the amount of canopy versus other evap would be different.
5. The Qcomm term is estimated based on conversation with locals. This is understandable given the constraints of the research, but introduces a very large uncertainty. In my own research we observed that some people living close to the dam would, against the deal with the entire community, use a machine pump to irrigate their lands from the sand reservoir, draining the reservoir very fast (Hut 2008).

Based on this concerns with nearly every term of the water balance, I would argue that any conclusions based on the final term Qout, should be taken very carefully. In the conclusions the authors related the decline in water to evaporation only and claim that 400.000 L per week is “lost”. I would first ask the authors to convert this to the usual mm/day units to compare if this is

remotely realistic. If I assume that the evap only comes from the sand-reservoir behind the dam and the reservoir is 10 times as long as the dam is wide (25 m), this would mean 400.000/(7 * 0.5 * 25 * 250) is about 18 mm of evap per day which seems unrealistically high. Secondly, I think that unreported withdrawals and seepage have an influence here that the authors don't take into account.

I believe the data on water volume from the measurements are very valuable, but I would ask the authors to have a second look at their model, given all the concerns above