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Discussions

Interactive comment on "Towards systematic planning of small-scale hydrological intervention-based research" *by* K. E. R. Pramana et al.

Anonymous Referee #2

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General comments

Based on three specific case studies, the paper discussed some of the contingencies and constraints involved when implementing simple hydrologic engineering designs in the context of rural communities in developing countries. The paper raises interesting issues on the practical challenges involved in the implementation of hydrological research, which are rarely discussed in the mainline peer reviewed literature. However, I do not see how the proposed framework meets its goal of addressing these challenges in a systematic and proactive manner. In particular, I remain unconvinced that the approach can be unambiguously transferred to other types of interventions. This can

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possibly be solved by better structuring of the argument to make this point more clear.

The obstacles and challenges encountered in the field are highly case specific and, in my view, the general framework proposed by the authors to integrate them in the planning seems artificial and frankly appears to be a bit of an overkill. In my opinion it adds additional layers of complexity (and costs) to the planning process, while not being quantitative enough to be actionable in a practical way. Indeed, planning for contingencies, asking the opinion of other experts and accounting for budget constraints are typically inherent parts of the planning process, and I don't see how the framework proposed by the authors is much of a contribution in that regard. More rigorously quantitative approaches exist to assign value to hydrologic information and decide whether/which observations to collect, given budget constraints and what we know (i.e. collected data) and don't know (i.e. uncertainties and contingency) about the system at any given time (see e.g., the Bayesian framework in James and Gorelick 1994 for groundwater).

The main contribution of this paper, in my opinion, is a set of case studies on the occurrence, effect and integration of unforeseen human agencies in hydrological interventions. The specifics of the case studies described in great details in Section 2 lack rigor (see below) and seem a bit disconnected from the larger discussion raised by the author: I would suggest adding them as supplementary material instead. In my opinion, the paper would be more useful to the HESS readership if it would rather focus on providing a well structured narrative of the unforeseen obstacles encountered on the field, and on the measures taken to address them. The paper could then discuss, whether the adaptation strategies were successful in these specific cases and comment on the external validity of these conclusions.

To conclude, the issues raised by the authors are important and should be discussed in the academic literature. Lessons learned from the cases presented in the paper can probably contribute to more effective and locally adapted approaches to small - scale hydrologic interventions, but that would require a substantial major revision of

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the manuscript. The paper should be more focused (shorter), the arguments better structured and the language needs to be substantially improved.

Specific comments

- p 9493 l19: Please elaborate, what did Hagen and Evju (2013) do and why is it particularly useful in this context.

- Vietnam case (p 9495-9501): Please mention explicitly what the research question is and how it might be satisfactorily addressed by the conducted analyses. As such, I fail to see the rationale for most of the described analyses . For instance, why was a numerical model necessary, and how does its outcome address the research question? Clarifying the methodology is particularly important because you make the point on p 9512 that the Vietnam intervention is the result of a particularly complex and well thought planning process.

- p 9498: Potential evaporation does not seem to be addressed in the paragraph.

- Kenya Case (p9501-9507): A counterfactual is missing in the analysis to convincingly measure the effect of trenches on vegetation. You fail to make the case that the areas with trenches are comparable to areas without trenches to begin with. What tells me that the WOT's were not systematically different from the WT's before the installation of trenches. This can be solved by including earlier NDVI data to show that the two groups were not significantly different before 2002. Also, I agree with the other reviewer that TRMM does not seem to provide any useful information and that Landsat would probably be a better source of NDVI data.

- p 9505 l.11: The wording is a bit unclear: do trenches dampen the seasonality of the NDVI signal without affecting its trend? If so, a graph showing NDVI time series of WT's and WOT would be useful to illustrate that point.

- Indonesia Case (p9507-9510): important details on the analysis seem to be missing here: what did you optimize when using the DEM and observed streamflow to respec-

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tively identify the site and design the turbine?

- p9508 l14: River discharge in itself is a random variable. Here Q refers to the discharge level that can be exploited to generate electricity (which, of course, is an inherent assumption of the potential estimation exercise).

- p9510 l1: The wording is a bit unclear: do you mean that daily discharge is irrelevant for hydropower design. If so, I respectfully disagree. Daily streamflow distribution (expressed as a flow duration curve) are commonly used to size run-of-river hydropower plants (e.g., Vogel and Fennessey 1994, Basso and Botter 2012).

- p9510 l 13: It seems a bit odd to include a case that neglects local socio-economic factors in a paper that is specifically dedicated to including these issues in hydrologic interventions.

- p9513: The Kenya case study seems exclusively based on remote sensing data, so (although very entertaining) I don't see how the particular anecdote of the elephant destroying the gauge is useful to make your point. It would be interesting, however, to discuss local involvement in hydrologic research in the context of the increasing availability of satellite data: to what extent does this new information source does or does not affect the researcher's reliance on local collaboration (and extensive field research in general)?

References:

James, B.R. and Gorelick, S.M (1994): When enough is enough: The worth of monitoring data in aquifer remediation design, Water Resources Research

Vogel, R.M. and Fennesey, N.N (1995): Flow Duration Curves II: A Review of Applications in Water Resource Planning, AWRA Water Resources Bulletin

Basso, S. and Botter, G, (2012) Streamflow Variability and Optimal Capacity of Run-of-River Hydropower Plants, Water Resources Research

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9489, 2015.