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Hydrology and Earth System Sciences
Discussions

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

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We thank Anonymous Referee #2 for the comments and valuable suggestions. We realize that the paper we propose should be more compact and structured, in order to strengthen the "interesting issues on the practical challenges involved in the implementation of hydrological research" we raise, as these are indeed "rarely discussed in the mainline peer reviewed literature". It is clear, however, that in order to clarify our proposed framework, we need a better structuring of the argument.

Although we realize that heavy additional planning instruments will be useless, we do propose that a more systematic approach would be useful when designing field

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projects and for exchanging field experiences between researchers. It may be true that planning for contingencies, asking the opinion of other experts and accounting for budget constraints are already inherent parts of the planning process, but we do not find much systematic comparison in the literature – let alone attempts to include human agency. Therefore, we are thankful that the referee acknowledges our main contribution on stressing human agencies in hydrological interventions. We realize that in order to strengthen our reasoning, we need to focus on providing a better structured set of narratives of unforeseen obstacles and the measures taken to address them. This will allow us to discuss the results.

We agree that the outcomes in terms of field realities and their obstacles and challenges encountered are highly case specific, but we do think that we can generalize about the type of issues that are shared between the field studies, including how to deal with them. We do realize that some of the issues we raise have been discussed by others. Indeed, James and Gorelinck (1994) present a framework on planning field observations taken into account budget constraints. What we aim to add is consideration of the social dimension, especially human agency. Our example of shifting small-scale interventions' locations due to local people preference suggests that even planned research is influenced much by the local community.

We will discuss in our overall comment how we propose to restructure the paper.

Specific comments

As will become clear in our general reply to the referees, some of the more hydrological details below may not be relevant anymore for a revised paper. We have provided answers nevertheless.

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

Hagen and Evju discuss using short term data for longer term goals under conditions

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of time constraint as we faced in our three research projects. Their application is much more in ecology, but their dilemmas correlate closely with our projects, in terms of intervention issues (pilot or small-scale) and possible adaptive management strategies.

- 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.

The original claim for the intervention was that contour trenching would provide long-term storage in groundwater and the unsaturated zone. Hence the project studied the impacts of contour trenching on water storage in the subsurface. We studied infiltration and recharge process. We measured surface water runoff, water in the trenches, and groundwater levels in wells. The numerical model was used link the point measurements, describe the infiltration process, and investigate the hypothesis of having possible “cloud creation” of water in the subsurface. The isotope data suggest that rainwater does infiltrate into the groundwater system. However, the modeling suggests that infiltration could not fully match the isotope data since the signal of rainwater was found in a short period of time after rainfall events. Infiltration may have been influenced by macro pores. We found no “cloud creation” of water in the subsurface after constructing contour trenches, but trenches seem to recharge the groundwater system.

Our main point on page 9512 is that the Vietnam intervention went through some negotiations influencing the hydrological research.

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

We will add a paragraph on potential evaporation.

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- 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.

Indeed, we did use Landsat data from 1984 as the starting point to compare areas WT and WOT. Landsat NDVI WO and WOT show scattered values in time before the construction of trenches, but do confirm that both WO and WOT have low NDVI values during the dry season and higher values during the rainy season. Since we found from MODIS NDVI that after contour trenching, vegetation growth does increase on the short-term, TRMM was used to indicate whether rainfall could be the cause of this. Of course we agree that runoff after rainfall events will influence much on the trench area and thus vegetation growth, but we wanted to know to what extent. Therefore we checked this by correlating shifts of rainfall (weeks to months) versus the greenness index.

- 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.

The trenches do not dampen the seasonality, but react based on seasonality with short term increase of NDVI signal during the rainy season for areas with trenches. We can see this "NDVI difference" time series of WT and WOT in fig. 8.

- 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 respectively identify the site and design the turbine?

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To identify the site and design, regardless of the economic point of view, we optimized combinations of streamflow (the catchment area) and head. Since we were much concerned on the streamflow part, DEM was used to provide more detail on the division of catchment areas. Afterward DEM was also used for searching areas with high head.

- 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).

We agree that discharge has a specific meaning in the formula.

- 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).

Indeed, daily discharge is quite important. However, we suggest that given the strong daily discharge fluctuations in this specific case, using the higher daily discharges may result in an over-design hydropower system. The low (daily) discharges during the dry months are appropriate for hydropower design.

- 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.

We do not discuss general socio-economic issues, like funding or general electricity use. Our focus is on human agency in the hydrological study. This is not totally isolated from the more general socio-economic setting, of course. In our case, the community was open to the intervention and willing to participate in construction and maintenance. However, the project itself was unable to realize the hydropower installation due to funding shortage. For the audience of HESSD, we excluded this material and focused on the hydrological research with its impact and feedback of the community.

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- 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)?

Our Kenya project included remote sensing after hydrological fieldwork (rainfall and soil moisture measurements). We used remote sensing to study our field data. We agree that the increasing availability of satellite data in relation to field studies is a key topic, and will have to include how local collaboration will be useful or not, but we do not think we can write that paper right now.

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