

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

## **Anonymous Referee #3**

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It is clear that the authors have a lot of experience in doing field research and it is valuable to document their perspectives. Overall field research in hydrology in the developing world remains woefully sparse. Very often reviewers and editors do not appreciate the challenges such field operations.

Funders cut budgets, and inadequate resources are allocated to field campaigns which eventually end up being unpublishable because of the issues highlighted in this paper. Researchers get very little return for years spent in the field. The paper seems to be a product of heartbreak of losing data over many years of intensive field research that I completely sympathize with having experienced these in my own field sites.

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However, there are fundamental issues with the way the paper is structured which make the arguments incoherent. As a general principle a paper should make one (or two at a stretch) main points and the whole paper should be structured to lead the reader to those conclusions. In this case the paper makes several fragmented points that do not add up to something substantive.

**Substantive Comments on the Argument** At present, the paper reads like two unrelated half papers: Paper 1 is about *planning field research campaigns* in developing world contexts to test the effectiveness of small-scale hydrologic interventions (Sections 1,3,4). Paper 2 is about the *outcomes* of small-scale hydrologic interventions (Section 2). At the moment Paper 2 is sandwiched in the middle of Paper1 in a way that feels contrived. In the end, the authors really need to decide which of the two papers they are writing. The two papers serve different purposes and make different points useful to different audiences. I do not think they should be combined into one paper.

Let's say the goal was really Paper1 (based on the title). The objective of such a paper would be to help researchers plan field campaigns systematically in the context of small-scale interventions; the intended audiences being researchers and perhaps funding agencies.

- My biggest problem with the argument as presented is that “human intervention in hydrologic research” is for most part reduced to just theft/vandalism and the main tool offered seems to be better allocation of budget. The paper does not really probe into the causes and mitigation or which research questions were the most important to ask.
- There is confusion throughout the paper in distinguishing between the interventions themselves (check dams, hydropower dams and trenches) and the hydrologic research to evaluate the effectiveness of the interventions (rain gages, water level recorders etc.). The two are treated as somewhat interchangeable (Page

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9511, Line 6). But do locals really perceive the two as the same? I.e., do locals also vandalise the trenches or just the sensors? In our field sites often sensors are tampered out of sheer curiosity by school-age kids or drunken youth. This happens even in places where there is tremendous support for the intervention itself. It's also possible that the sensor thefts are simply by poor people who want to make a quick buck and is unrelated to the public attitude to the intervention (as the authors also mention in passing at the bottom of Page 9514). Maybe they simply don't see the value in the shiny objects lying around.

- Participation theory is introduced briefly as a theoretical framework to examine why community participation may not have yielded the expected results but is then eventually dismissed as being not useful. To reach the conclusion whether and what kind of participation matters (or not) - the authors need to do more social science research on the causes of theft/ vandalism than they are probably willing to. But even so, the paper does not clearly describe what was actually attempted in terms of community participation in terms of the research itself (as distinct from participation in the interventions). For instance, I would expect that merely hiring locals as field assistants would be less effective than having locals being consulted in framing the research questions. I recommend omitting this discussion on participation and related figures altogether. Too much space is spent on a framework that is eventually dismissed and doesn't contribute to the larger argument.
- I like the effort the authors have put into trying to develop a “benefit-cost” analysis for hydrologic data. I understand that the Delphi Method was used to derive the relative benefits of different research strategies. I am wondering if “value of information” analysis might also be used? There is quite an extensive literature in hydrology to draw on here. This would involve deleting or adding parts of the data set to the models to help researchers understand how much information each data point adds. This could help quantify the uncertainty introduced because of

the data loss. This might be a way to retain some of the more technical aspects of the paper (but the details will have to be in an Appendix).

- I think the authors need to attempt a “conceptual diagram” to pull the pieces together. There are too many fragmented pieces and random unrelated lists. For instance, for each of the three case studies, the authors could illustrate a flow chart mapping the specific research questions, with the approach - how many data points would be required, to what the researchers should expect in terms of data loss.

**Substantive Comments on the Structure** At the moment the paper contains three marginally related but complete micro-papers stuck inside it (in Section 2). This is simply not a good practice.

Even if the authors decide to draw on the cases in a re-purposed Section 2, first, I do not think the section should include the details of the non-field research based components. For instance, at the moment there are sections on NDVI and TRMM analyses in the Kenya study and DEM analysis in the Indonesian study. These have no bearing on the larger arguments on systematic planning of field research which is presumably what the paper is about. There simply isn't enough detail on the research questions etc. to judge them on their own merits. Second, the cases must be tied together by a common framework in this revised Section 2.

**Minor Comments** On Page 9915, Line 8 I think the authors mean “could NOT possibly have been anticipated”. The “NOT” is missing. This obviously changes the meaning of the sentence. On Page 9523, Line 18 I suggest the authors explain what the Delphi Method is by adding the definition “a structured interactive forecasting and communication method which relies on inputs from a panel of experts.” And then add a citation.

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