

Interactive comment on “HESS Opinions “Urgent water challenges are not sufficiently researched”” by P. van der Zaag et al.

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This opinion paper makes some very interesting observations of biases in current hydrological research. I fully agree that there is a lack of research aimed at urgent socio-economic questions, particularly in development countries. And I also concur that different forms of (preferably interdisciplinary) research cooperation involving "the South" should be stimulated, particularly those aimed at strengthening research capacity.

Still, I would be interested to hear the author's opinion about the reasons and potential solutions to the problem. Some reasons are already mentioned by the reviewers (financial, maybe not really a scientific problem) but as a "Northern" researcher actively trying to establish research connections with developing countries, I would like to share some of the major problems I see with what I would call "development relevant

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research":

- lack of funding. This is a major and very obvious contender, but also an easy culprit. Indeed, getting research funded is always difficult. On the other hand, I think there is an additional risk (as a researcher from the North) in focusing your research on overseas areas, and getting partners from developing countries on board. Those partners may be perceived as not as scientifically excellent as partners from the North. Focusing research on areas with lower data availability is also seen as a higher risk, jeopardizing the potential for world-class research. Finally, at least in the UK, the recent economic developments have encouraged several Research Councils to screen research proposals on relevance for the own country's economy. This obviously does not help overseas research. A solution (outside the hands of scientists, that is) may be specific funding calls for such research, maybe preferentially through international institutes.

- lack of data. Hydrological data are often lacking, of poor quality, or simply not accessible in many developing countries. From my own experience, even local universities in the south often have no access to state owned data (or are charged ludicrous prices, in the order of USD0.5 per data point, which may add up quickly for time series). Data collection is very costly and time consuming. Particularly for hydrological research, long time records are ideal, but the collection of these require a long term investments many individual researchers cannot commit to. One solution may be to establish large scale, concerted, global monitoring networks such as GLORIA (<<http://www.gloria.ac.at/>>).

- a strong focus on conceptually new research. From my experience, hydrological journals are very much focused on new concepts. I (subjectively) perceive an interesting difference with other disciplines such as biology. While the discovery of new plant or animal species (even if they occur only very locally) tend to be a big event, a paper with a detailed description of the hydrological processes of a catchment that may differ from all the processes we as hydrologists know, may be dismissed by a quality journal as having only local relevance. In that sense, the data scarcity mentioned before is a

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major stumble block. New techniques, ranging from model concepts to interpolation techniques, are difficult to apply and evaluate in data scarce areas. This may have even resulted in a focus on very complex models and methods that they are not applicable in say, 90% of the world, due to lack of data. In that sense it is interesting to see that in catchment hydrology papers, only a handful of experimental catchments seem to recur. Maybe as a hydrological community we should think of a new journal dedicated to the detailed description (and publishing) of hydrological data records?

This is just a quick and in no means exhaustive selection of possible reasons. I would be interested to know the opinion of others, in particular the authors of the opinion paper.

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