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Hydrology and Earth System Sciences Discussions

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 tackles an important and in my opinion concerning issue. Although perhaps rudimentary researched with the ISI database, the manuscript gives a relevant insight in the general (dis)focus on research themes and geographical distribution of research. I agree with the authors and referees that we should be concerned about this disfocus. I would like to add a few comments in view of my own experiences with special focus on the performing of hydrological studies in data scarce regions, that may be relevant to the discussion. I reply especially on the questions raised by prof. Sivapalan and Dr. Buytaert.

Siva asks 'Why are researchers prevented or discouraged to do research in regions where it is actually needed?' I think I agree with Dr. Buytaert that it has at least partly

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to do with data scarcity and that the remainder, such as funding and publication is a result of this. The risk of failure or too simple-minded computer-based research is too large to face the challenge of data scarcity, from the view point of both the researcher and referees/funders. Let me try to explain how this problem may be breached from both view points.

First of all, the view point of the researcher: researchers should, when studying ungauged basins, shift their focus from applying classical methods in the ungauged case towards the very question how to deal with water related problems in a scientific way under data scarce conditions. They should realise that these classical methods may not be fully applicable and should focus on developing new methods. To a certain extent, this is already done quite frequently in hypothetical case studies, where ungauged conditions are mimiced in a well-gauged basin (or multiple basins if the study deals with regionalisation techniques) by jack-kniving a certain amount of data (see e.g. Seibert and Beven, 2009, Yadav et al. 2008, Oudin et al. 2008). But it is really strange that in the era of 'Predictions in Ungauged Basins', hardly any papers really focus on the development of predictive hydrological models and methods to construct them in *real* ungauged catchments. The former studies of course give insight in the value of data and the relations between catchment properties and hydrology, and may sometimes be directly applicable in certain poorly gauged cases, but there's a large risk that the mimiced conditions are ill-posed with respect to real ungauged conditions. In real ungauged basins, there is no risk of overlooking assumptions, which inherently were taken in the gauged case (e.g. known model structure, similarity by proximity, known uncertainty in input data, etc.), but are simply not valid or not known in the ungauged case. When real circumstances are used, the scientist cannot 'cheat' and has to go for real discovery (which is what science used to be all about) for instance search for new data, for non conventional information, e.g. in satellite observations or field experience, and focus on new modelling techniques. Following such an approach to hydrological science should also allow the researcher to use less rigid information for our discoveries (for instance, impose constraints on models based on soft knowledge

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on the response of a tree species or local water levels, also suggested by Seibert and McDonnell, 2002). It would furthermore 'strengthen the research capacity in the global South' as the authors state, because researchers are mostly forced to work together with local water people.

But are referees and funders willing to embrace this approach, despite the risks? This connects to Siva's last question: 'what should we do to encourage and support more research in these areas?' To encourage more research in regions with extremes and lack of data, the community, in the form of referees and funders, should be willing to let the researcher take this risk of performing studies in data scarce regions and accept that the results will look non-conventional. In the case of hydrology, they should not always expect the classical hydrograph with probability intervals. In real ungauged conditions, the quality of results is not known a priori, which is usually the case in many other scientific research areas, and reviewers and funders should be willing to accept that the type of results are therefore also not fixed and not known a priori. To concede to referees and funders, the researcher needs proof that his/her method actually works. He/She can try to find some validation material. The scientist should go into the field and make his/her hands dirty, for instance by collecting a concise set of measurements that can be used as proof of concept, but the reviewer should then accept that such a dataset is maybe not very long. Recently, our group did something like this in the Luangwa catchment, a true ungauged basin in Zambia with little measurements, having all sorts of data quality problems. A model was constructed and calibrated in a fully indirect way by extracting in an objective way as much information from the little data as possible (without regionalisation). 'Soft' satellite information was also used, while taking into account that the uncertainties in these data were poorly known. Proof of concept was given by a relatively good dataset of rainfall and discharges collected in the field. I certainly hope that the community will let us succeed in publishing. We are keeping our fingers crossed here.

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