

## ***Interactive comment on “Applying PUB to the real world: rapid data assessment” by C. Jackisch et al.***

### **Anonymous Referee #2**

Received and published: 2 September 2011

Review Applying PUB to the real world: rapid data assessment C. Jackisch, E. Zehe, A.K. Singh

Jackisch et al. write in their manuscript about the pre processing in a poorly gauged basin in Northeast India. They define a complex framework and a short time schedule what and how to measure important hydrological data sets and parameters for simulations with hydrological models of different complexities and DDS. But the complete modelling part is part of a future manuscript and here they only present data of remote sensing estimation of soils and landuse as well as a poorly statistical analysis of usable meteorological stations. They are promising a PUB application but the topic is more estimation of catchment parameters in data-sparse area with the presentation of a total

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uncertain result. Validation is always missing and then there is too much unimportant text. The most figures and tables are not helpful. It is not clear why they have not observed vegetation and erosion parameters if the catchment is dominated by overlandflow and agricultural management aspects are under investigation. The English is ok but the authors have to check for unscientific language. The authors cited Fenicia et al. (2008) and wrote about a common language, but start to create their own terminology like bio-physical problem or fractals without explaining them. So it would be good if they just follow their own advice and use the typical terminology. Abbreviations have to be checked. DDS is explained several times and others like SRTM are not. The structure has to be advanced. The manuscript is in most parts too long and has to be shortened. As the first reviewer has already mentioned that manuscript can not be juggled without the second manuscript with the modelling subject or have to be completely reorganized.

Title is not appropriate, especially the rapid data assessment is not important for that manuscript. Abstract is not adequate and too short. It should be rewritten and extended. Conclusion is too short. Citation: There is no actual literature inside of that manuscript after 2008.

Specific comments are listed as page, line

7500, 10-16: Any citation would be helpful. 7500, 20, 7501, 10: The authors gave an overview about the PUB problematic but in the manuscript is the focus on estimating spatial data of soils, vegetation and meteorology. They should give an overview about the problem of data scarcity and how their methods would fix into it. 7501, 12-23: It is not clear if that is important for the understanding of the manuscript. The authors are discussing the importance of DSS which is not part of that manuscript. That framework is more a part of an outlook. Why allow the authors only ten weeks imitative of field work? It is not important how many persons have done the work but the presented quality. It sounds like the authors would like be congratulated for having done a lot of measurements within a small time frame. The modelling in the last sentence is not

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part of the presented work and confuses the reader. That's a problem of the complete manuscript. Everywhere are parts which the authors do not show. Abbreviations have to be only once explained. 7502, 15-16: That's part of the study area. 7503, 17-26: Which goals are discussed here and which will be part of their second manuscript? 7504, 9-17: move that part to the outlook and shorten it. There is still the question why the authors will use that two absolutely different types of model, WASA a semi distributed semi physically one and SWAP a physically based column model. 7504, 19-25: Here the authors should give information about the structure of that manuscript. What is part in which chapter? 7505, 23, 7506, 2-3: Pleas use scientific language. For example "task force" and "cooks striving for a common language" are not scientific. 7505, 22 - 7506, 10: The authors write what the benefit of a "one person campaign" compared to a team of scientists is. If you are going to field you have limited access to people and measurement devices but that is no reason to right a paper about it. One person has as well only a limited knowledge about different processes and measurement techniques and will bias the measurement. 7506, 13-18: The selected studies have nothing compared with the presented study area. All of them are humid catchments in an absolutely different scale. And the authors have detected that fact so why do they write about it? They should select comparable studies with similar size. 7506, 24: The authors study does not present a data source which has less uncertainty compared to Winsemius et al. (2006). 7507, 21 - 25: That soft data is always discussed but not presented. 7509, 12 - 7510, 11: There is a lot of unimportant information for the presented manuscript which can be condensed to a view sentences. Pleas explain the reader which information were used to get knowledge about which parameter. 7511, 26: Again a comparison with a catchment in a humid area, a different geology/pedology and an absolutely different scale. There are so many studies in semi arid areas, why the Weiherbach? 7512, 1-3: It is not clear what the authors mean with that sentence. 7514, 7-9: Why do the authors think that 5 samples are enough to have any representative sample size of unsaturated parameters for that size of catchment, which are even in the small scale absolutely variable? Are these parameters important to understand

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the catchment? Why do they not observe erosion parameters? Erosion should be one of the important processes and there are easy methods available to observe them and are easy to measure (Hudson, 1993, Cammeraat, 1997, Herick, 2001). By their focus on crop estimation vegetation parameters especially by the use of SWAP are as well important. One of the authors has already published a paper to that problematic. 7514, 21: "model suit": Pleas use scientific language. 7515, 4-16: that block recurs a view times in the manuscript. The recurrences should be cancelled. Why they don't discuss the usage of remote sensed meteorological data like Winsemius et al. (2006) presented? The authors should give information about why they find it "quite adequate" to use data 140 km away from the study side. 7515, 14: If there are studies in neighbouring areas that is the right place to cite them. 7516, 21: A table could be more helpful than figure 5 with a characterisation of the main characters of the 5 transects (slope, dominating geology, vegetation, sample points at the transect, etc.). Please explain why there is no transect in the South-eastern part of the catchment? 7518, 11: Please present the statistics of the double ring measurements (max, min, std, median) and at which locations they where conducted. In table 4 the authors present Ks in mm/h. They should use the same unit here. If 870 mm/d is the mean value observed it is quite high to observe overland flow. They should discuss that. 7518, 13 - 18: Why were only 31 samples selected from the complete set? What was the selection criterion? What do the authors mean with "some 16 of this"? It is not important where the samples were analyzed only why and how they were selected. 7519, 19: Why have the authors chosen LANDSAT data and not other cheaper and less drawback data? They should discuss their choice. Please write out the first time used abbreviations like ETM+ and define which LANDSAT was used, which is on page 7525, 15. 7519, 19: How do they have transformed mineral spectra to soil classes? How have they ground truthed and where are the uncertainties? How can they be sure that there is no influence of plant remains in the LANDSAT signal? Actual publications are missing. 7520, 4: Montgomery (1976) is missing in the references. Again ground truthing is missing. 7520, 15: NDVI should be presented with equation and the expected ranges.

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7523-7525: That looks like a genetic soil classification. Please shorten that paragraph to the important aspects. 7525: The authors should present a value how good their cross validation was and how have they linked soil classes to mineral spectra? That is not clear here. 7527-7528: Land use classes should be shortened and validation measures should be presented. 7529-7531: For what are these data sets used? 7531: The meteorological data is already described in the former chapters. Table 1: What do they mean with topographic mapping with GPS? Have they used a differential GPS to advance the DEM? They should as well include the sample size of their methods. Table 4: Add the sample depth and the uncertainty of the samples. How are these values been created from how many samples? Figure 1: Please use a top view. Where are gauging stations? Write in the caption what kind of samples has been taken. Figure 2: Skip that figure, it is not helpful. Figure 4: The figure is too complex and not described in the text. For the presented manuscript it looks as well not important. Figure 5: Again that figure reflects only two transects of the five and is too complex. Figure 6: It is not easy to read without a colour legend and it is not clear what the reader should learn from it. Figure 9: By the strong non linear relationship the Spearman rank correlation would be a better measure to quantify the relationship.

References: Cammeraat, L.H. (1997): MEDALUS Mediterranean Desertification and Land Use Field Manual. Amsterdam, 113 p. Fenicia, F., McDonnell, J.J., Savenije, H.H.G. (2008): Learning from model improvement: On the contribution of complementary data to process understanding. *Water Resour. Res.*, 44, 5 W06419, doi:10.1029/2007WR006386. Herick, J.E., Whitford, W.G., de Soyza, A.G., van Zee, J.W., Havstad, Seybold, K.M., Walton, M. (2001): Field soil aggregate stability kit for soil quality and rangeland health evaluations. *Catena* 44, 27-35. Hudson, N.W. (1993): Field measurement of soil erosion and runoff. *FAO Soils bulletin* 68. Winsemius, H.C., Savenije, H.H.G., Gerrits, M.J., Zapreeva, E.A., Klees, R. (2006): Comparison of two model approaches in the Zambezi river basin with regard to model reliability and identifiability. *Hydrol. Earth Syst. Sci.*, 10, 339–352.

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Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 8, 7499, 2011.

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