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Discussions

Interactive comment on "Modeling runoff and erosion risk in a small steep cultivated watershed using different data sources: from on-site measurements to farmers' perceptions" *by* B. Auvet et al.

Anonymous Referee #1

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The manuscript entitled "Modeling runoff and erosion risk in a small steep cultivated watershed using different data sources: from on-site measurements to farmers' perceptions" by Auvet et al. deals with setting up and applying a conceptual model of water and sediment fluxes for a small agricultural catchment in Java. It strives to give practical information for preventing soil loss by identifying erosion hotspots and driving processes. Particular stress is put on the fact that predominantly soft data is used in this data-scarce setting. As such, it is a relevant and important aim in this context. However, I have many severe concerns on the applied methodology, the drawn

conclusions and the validity of the results. Some of this may be related to the fact that I found the descriptions in some parts to lack substantial information. My major points of criticism are as follows (for details, please see the annotated PDF): 1.) The approach claims to be suited for data-scarce areas. However, in the presented study multiple data have been acquired with considerable effort: the main soil-hydrological also builds on elaborate sampling and extensive experte knowledge, numerous farmers' interviews, labour-intensive dGPS-measurements (although I'd think that 1 dGPS point / 22 m^2 is still enough to support a 0.125 m^2 DEM). Therefore, the motivation of the study lacks validity for me.

2.) The distributed model parameters were derived from decision tables, reconstructed from farmer interviews and estimated paremeters. Observations for validation were made on selected plots; runoff was roughly estimated with an unreported frequency. The modelled runoff is used to infer erosion hazard and vulnerability based on vague classification of topography, soil, cover and tillage. The model results are compared to measured runoff volumes; runoff patterns to farmer-observed patterns in an undocumented manner. All this seems very subjective and arbitrary to me. While there may be some virtue to the approach, I cannot attest this to be scientifically reproducible research. This corresponds to the study's objectives being too obscure.

3.) Essential parts of data acquisition (farmer interviews), model description and calibration are missing.

4.) The shown runoff behaviour suggests that a simple runoff coefficient with initial abstraction might mimic the event response likewise. The use of the STREAM model instead needs to be better justified.

5.) The final risks maps do not provide much surprise; high risk is assigned to areas where one would expect it in a mere qualitative way. It needs to be proofed/pointed out more clearly where exactly the benefit of the presented apporach is when compared to a simple GIS intersection analysis of maps of flow concentrations and gradient etc.

The annotated PDF contains more concrete comments and hopefully constructive suggestions. To me, 2. and 5. seem to be the most important issue. I do not see that the current manuscript can be improved within a scope of Major Revisions. I therefore recommend the rejection of the manuscript and an invitation for resubmission.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C4928/2015/hessd-12-C4928-2015-supplement.pdf

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 9701, 2015.