Hydrol. Earth Syst. Sci. Discuss., 8, C3205–C3210, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C3205/2011/

Hydrology and Earth System Sciences Discussions

Interactive comment on "Land classification based on hydrological landscape units" *by* S. Gharari et al.

S. Gharari et al.

shervangharari@yahoo.com

Received and published: 3 August 2011

Reply to Comment by E. Zehe

We would like to thank the editor for his constructive comments on our work.

- You sampled your points along transects obviously for good reasons, but you should comment and justify this design

We thank the reviewer for this suggestion and will clarify this in the paper. The idea behind the approach is to select transects where the HAND values vary continuously. HAND is zero on the river reach and has maximum gradient when moving uphill. Our

choice was to move uphill but not on the line of steepest descent, to cover larger areas.

- Maybe I missed it but which at within which ranges did you sample μ and σ

This was mentioned in page 4391, line 25 and 26, however we didn't use the notation μ which was mentioned also by I. Nalbantis in his comment on our paper. The values mentioned in section 4.1 as, for example S, should be reported as μ_S and so on. This will be corrected. The range for sigma(σ) was started from a big range and after running several times the iteration limited reasonable values.

- Eq. 14 by normalizing the sum of P with the sample size, you give equal weights to the three classes. This might be problematic when sample sizes are very different and confidence levels are different?

What the reviewer mentions is correct. The reason of this choice is to avoid giving little weight to the portion of the landscape that is occupied for a small part by a certain class. For example, if 5% of the catchment is covered by wetland, and about 5% of the observation data fall into wetland, we still want wetland to count as 1/3 in the computation of the objective function, and not 5%. Otherwise this 5% percent will be affected by bias from the points gathered from the rest of the catchment. This will be clarified in the manuscript.

- I advise you to revise the terminology and avoid statistical terms when they are not adequate, as I am not whether all these "probabilities" sum up to one.

Actually what we obtain is the probability of a certain class of the landscape, which is always adding up to unity.

- When making these expert classification did you take S, D, H into account, if

so how? Did you use other indicators for instance functional vegetation types or soil types to for instance identify wetlands (which has typical vegetation and depending whether it is at a slope or close to the river typical soils)? I think it is crucial to validate, train your scheme with landscape units that have been classified according to different indicators, otherwise this is a bit a logical circle.

For the data collection and in situ classification of the landscape we used S, we did not use D and H, and we used a richer set of criteria, that include soil and vegetation. In particular, we determined classification based on slope and visual inspection, and other indicators, such as soil and vegetation. In a wetland, we used a hand auger to determine the depth of the water table. The vegetation was used to differentiate the plateau which is mostly agricultural in the Wark Catchment and wetland which is mostly covered with grass without trees because of the high water table. Hillslopes were mostly not cultivated allocated to industrial forestry. We found relatively good agreement between land use and our defined classes. However we didn't present this in the paper because of the fact that the available land use map was not as accurate as our DEM.

- The paper suffers from making statements a) too general and b) being often imprecise. For instance not all hillslopes are dominated by capacity controlled subsurface runoff, pipe systems might be intensity controlled (Wienhöfer et al. (2009), van Schaik et al. (2008)) some hillslopes are dominated by Hortonian overland flow (the Weiherbach in Germany). Your scheme of associating typical morphological units with the proposed dominant processes is certainly a valuable concept. However these dominant processes might change when moving to other landscapes. This should be discussed as example for being imprecise: what is exactly meant if you claim that ecology, hydrology and geomorphology co-evolve, not the science fields I guess.

The reviewer is right that we may not claim that our findings for the Wark Catchment

have general applicability. We are aware of the fact that hillslopes can respond differently under different environmental boundary conditions and that this might require different landscape categories in other areas. We will make this clearer in the revised manuscript.

On the other hand, like for any other model we have limitations and boundary conditions. The aim of the model is not to say that hillslopes work in similar way everywhere but is rather to extrapolate our point expert knowledge form 0D (point) to a 2D (map). In the case that our knowledge indicates the functional behavior of landscapes is different; this model is not valid anymore and should be adjusted accordingly. For example if bed rock changes from impermeable volcanic rocks to sandstone then this has to be considered.

We agree that the manuscript is a bit too imprecise at places and we will correct this. Concerning the final question by the reviewer, what was meant is that the prevalent ecological, hydrological and geomorphological conditions coevolved as the landscape was formed.

- I miss a proper referencing to approaches to assess functional units in the landscape, I am no expert but I know work from Pelletier and Rasmussen (2009), Behrens et al. (2010), James and Roulet (2009), Schmocker-Fackel et al. (2007), Boogart et al.

This is what other referees (I. Nalbantis) also suggested and we also feel it is essential; we will refer to other related papers. We will add another sub-section for section 4 (discussion) and describe the differences . As one of the referee commented, we also would like to add this section to compare the methods developed for this paper to Nobre et al. (2011).

- What is meant with essential hillslope functions (drainage and storage) essential for what? Or do you mean generic? - Preferential flow paths can also

origin from biotic processes, cracking.

In this context essential means: essential for the hydro-ecological system to maintain itself, requiring both drainage and moisture retention. As formulated in the manuscript this may not be entirely clear. We shall reformulate this part.

- I would assign equal scales to panels c and d in Figure 11 and add the sample size at least to the figure caption

This is a good suggestion we surely will implement it in Figure 11.

- There might be something to learn by analyzing the patterns of wrong classifications, in space and with respect to systematic errors?

This is also a good point which was explicitly mentioned in our paper in section 4.5. The problem comes from the fact that the threshold cannot be generalized for the entire catchment, particularly in cases where geology is different. So in this paper the limitation for calibration for all the points was not met and the catchment were treated as a uniform body, however the different results obtained for the northern and southern parts show that the threshold for the HAND (μ_H) is deferent in both cases and the general HAND threshold cannot very well predict the gently sloping landscapes giving an overestimation of wetland in gently sloping areas. We will extend this analysis a bit further in the revised manuscript.

Once again we would like to kindly thank the editor for his constructive comments on our work.

References

Behrens, T., Zhu, A.-X., Schmidt, K., and Scholten, T.: Multi-scale digital terrain analysis and feature selection for digital soil mapping, Geoderma, 155(3-4), 175–185, 10.1016/j.geoderma.2009.07.010, 2010.

James, A. and Roulet, N.: Antecedent moisture conditions and catchment morphology as controls on spatial patterns of runoff generation in small forest catchments, Journal of Hydrology, 377(3-4), 351–366, 10.1016/j.jhydrol.2009.08.039, 2009.

Nobre, A. D., Cuartas, L. A., Hodnett, M., Rennó, C. D., Rodrigues, G., Silveira, A., Waterloo, M., and Saleska, S.: Height above the Nearest Drainage, a hydrologically relevant new terrain model, Journal of Hydrology, 404(1-2), 13–29, 2011.

Pelletier, J. D. and Rasmussen, C.: Geomorphically based predictive mapping of soil thickness in upland watersheds, Water Resource Research, 45, W09 417, 2009.

Schmocker-Fackel, P., Naef, F., and Scherrer, S.: Identifying runoff processes on the plot and catchment scale, Hydrology and Earth System Sciences, 11, 891–906, 2007.

van Schaik, N. L. M. B., Schnabel, S., and Jetten, V. G.: The influence of preferential flow on hillslope hydrology in a semi-arid watershed (in the Spanish Dehesas), Hydrological Processes, 22(18), 3844–3855, 10.1002/hyp.6998, 2008.

Wienhöfer, J., Germer, K., Lindenmaier, F., Färber, A., and Zehe, E.: Applied tracers for the observation of subsurface stormflow at the hillslope scale, Hydrology and Earth System Sciences, 6, 2961–3006, 10.5194/hessd-6-2961-2009, 2009.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4381, 2011.