Review of "Hydrological heterogeneity in Mediterranean reclaimed slopes: runoff and sediment yield at the patch and slope scales along a gradient of overland flow" by L. Merino-Martín, M. Moreno- de las Heras, S. Pérez-Domingo, T. Espigares and J.M. Nicolau.

First, I like to congratulate the authors for the interesting, well-written and easy to read paper. Secondly I like to point out that, in my opinion, the interest of this paper lays in achieving interesting research conclusions on the basis of creating a synergy with previous existing knowledge and giving a step forward.

The paper analyses the eco-hydrological role of the vegetation pattern established in reclaimed slopes after mining activities in a Mediterranean environment. It presents an important contribution to the to the Ecohydrology field of restored slopes in water-stressed environments. The paper is very skillful in integrating basic concepts and approaches from different disciplines and generates new applied knowledge for the specific problem of stabilization of reclaimed slopes. It is not extremely innovative in the type of data, concepts or data analysis, however it presents a creative way of applying basic research results and generating synergy between well-known concepts already individually explored in the last two decades. It brings all those concepts together and it results in an integrated analysis of eco-hydro-geomorphological processes from an ecological restoration perspective. The results are susceptible to be transferred as criteria for the design and/or managing of restored slopes.

In my opinion the paper fits perfectly with the HESS journal and could be published after discussion and clarification of some points:

1. The paper introduces in my opinion two creative terms: overland flow gradient and hydrological diversity. The use of those terms gives a new dimension to former concepts of intensity land use gradient, climatological gradient and Shannon diversity index, incorporating the hydrological dimension within them. I support the use of both concepts but both should be clearly defined from the beginning, for instance between brackets in the abstract and/or clearly stated in the methods.
2. I suggest also renaming the term "patches" to "patch-type" or "type of patches" or something similar. When you talk about different patches or seven patches sounds too general and confusing, it seems to refer to the quantity of total patches and not the seven classes of patch.
3. The paper uses updated references, including some key papers of the last two decades in vegetation patterns-hydrological response-geomorphological processes relations, however I still miss some other key papers that made an important contribution to this subject and could be inspiring for some parts of the introduction and discussion, including some reviews that synthetize the knowledge of last decades:

    a. Arnau-Rosalen, E., A. Calvo-Cases, et al. (2008). "Analysis of soil surface component patterns affecting runoff generation. An example of methods applied to Mediterranean hillslopes in Alicante (Spain)." Geomorphology **101**(4): 595-606.
    b. Bautista, S., Á. Mayor, et al. (2007). "Plant Spatial Pattern Predicts Hillslope Runoff and Erosion in a Semiarid Mediterranean Landscape." Ecosystems **10**(6): 987-998.

c. Bergkamp, G. (1998). "A hierarchical view of the interactions of runoff and infiltration with vegetation and microtopography in semiarid shrublands." CATENA **33**(3-4): 201-220.
d. Boix-Fayos, C., M. Martínez-Mena, et al. (2005). "Concise review of interrill erosion studies in SE Spain (Alicante and Murcia): erosion rates and progress of knowledge from the 1980s." Land Degradation & Development **16**(6): 517-528.
e. Boix-Fayos, C., Martínez-Mena, M., Arnau-Rosalén, E., Calvo-Cases, A., Castillo, V., Albaladejo, J., 2006. Measuring soil erosion by field plots: understanding the sources of variation. Earth Science Reviews **78**, 267-285.
f. Cammeraat, L. H. (2002). "A review of two strongly contrasting geomorphological systems within the context of scale." Earth Surface Processes and Landforms **27**: 1201-1222.
g. Cantón, Y., A. Solé-Benet, et al. (2011). "A review of runoff generation and soil erosion across scales in semiarid south-eastern Spain." Journal of Arid Environments **75**(12): 1254-1261.
h. Davenport, D.W., Breshears, D.D., Wilcox, B.P., Allen, C.D., 1998. Viewpoint: sustainability of piñon-juniper ecosystems—a unifying perspective of soil erosion thresholds. Journal of Range Management **51**, 231-240.
i. Mayor, Á. G., S. Bautista, et al. (2009). "Factors and interactions controlling infiltration, runoff, and soil loss at the microscale in a patchy Mediterranean semiarid landscape." Earth Surface Processes and Landforms **34**(12): 1702-1711.
j. Mayor, A. G., S. Bautista, et al. (2008). "Measurement of the connectivity of runoff source areas as determined by vegetation pattern and topography: A tool for assessing potential water and soil losses in drylands." Water Resources Research **44**(10).

4. Concerning the methods section:

- Could you please add some more information on the area of Spain where the study zone is located: province, region, type of mining, geological information?
- Between 17-19 lines of page 9932, I would add somewhere close to the explanation of the scenarios selection the term an “overland flow gradient” (as appeared in the title of the paper). It is a quite intuitive concept but if you use it for the first time (I can not find if it has been used before in the literature…), is very important to clarify it from the beginning.
- In the description of the experimental setting, Table 1 provides a good description of the slopes, is not Area of Water Contribution Area redundant? Why not to use just Water Contributing Area? Please could you give in the description the soil depth?
- I miss a table explaining the distribution and characteristics of the plots. Could not be Table 2 modified to show the distribution of plots (from the description of page 9933 I count 3 plots * 5 cover types= 15, but in Table 2 I count 7 cover types *3 plots= 21 plots?) I think there are seven types but the explanation of the paragraph in the methods is confussing. Could you please clarify this? Would be possible to add to Table 2 information explaining in which slopes are present each patch type, which % of cover of patch-type is present at each slope, and how many plots were installed at each slope?
- It does not seem very accurate to determine visually the catchment areas of gerlach plots, and especially I do not understand why you did it like that if you have detailed topographical data obtained with a total station. Why did you

not produced a dtm to calculate with GIS a more accurate catchment contribution area for each plot?, any specific reason?. If you have the data I encourage you to recalculate the contribution areas and so on the hydrological and sediment yield data to obtain a much higher quality data. I do not believe that the description of visually delimitation is very adequate and if you use it you have to explain the criteria and how you measured the area in the field if this is the case (which delimitation criteria did you used, only topography or vegetation barriers as well?).

- Why did you placed the TDR sensors at 25 cm-depth? Which criteria did you follow: root depth, soil horizons, non-crusted soil surfaces so possibilities of macropore flow etc.? What is the depth of the soil?, At 25 cm is quite normal that you did not find differences in soil water content especially after 5 days of following a precipitation event, in natural slopes dominated by hortonian overland flow the soil moisture in first 10 cm are crucial for runoff generation, in saturated-runoff generation mechanisms the water soil content in deeper layers is also a crucial factor. I suggest further in points 8 to 10 some ideas to see if those concepts can be incorporated in the discussion.
- I could not find any paragraph describing the soils sampling and soil analysis, some indications are given in the foot note of Table 1, but this is not enough. Even when the results of the soil sampling are only used to characterize the slopes, I think they deserve a paragraph within the methods section. Including also the pF determinations for AWC used in Table 2.

5. I liked the synthetic description of results in general terms, but in some cases it is too short, I miss a more extended description of the sediment yield data compared at the two scale levels (plots and slopes, commenting Table 3 and Figure 2).
6. In subchapter 3.5 a more extended description of the micro-topographic forms is missing, it could be illustrated even with some photographs.
7. In general terms I find the discussion well focused and organized, however it is too long to be read without subchapters. I suggest splitting it at least in two sections following the main subjects and spatial scales: for example 4.1. Ecohydrological role of microenvironments; 4.2 Connectivity of water and sediments at slope scale and, even adding a reflection on the application of the results 4.3. Applied ecohydrological concepts for the design of reclaimed slopes.
8. After line 24 of page 9939 the discussion on the hydrological response of *Brachypodium retusum* could be completed with the work of Arnau-Rosalén et al. (2008) who found that the patches under this specie have a very high infiltration capacity (Table 2 of the mentioned paper) and have a very slow response to runoff but with a fast response in reaching runoff stability (see 4.2.1. Runoff response on the vegetated components of Arnau-Rosalén et al., 2008)
9. I think the results that you have could be further analyzed to relate the soil moisture conditions with the runoff generation mechanisms, aiming at relating the patch type with a certain runoff generation mechanism, depending on the infiltration and water-retention characteristics of the soil in such patch. I see that you calculated the AWC, so you have data on the water retention at field capacity at each patch type. It would be very interesting to check what was the water content of the soils after each event (because you do not have continuous monitoring, do you? In that case would be even better) in relation to field capacity, if they were above field capacity we probably can talk of runoff generation by saturation excess, more related with deep well-structured soils with macropore flow conditions, or if they were close to wilting point probably we can talk over runoff generation due to infiltration excess, more related to degraded soils. In this way you can take your discussion between ecohydrology and erosion

processes further, and this can give us another indication of the soil condition in the reclaimed slopes.

10. The only problem that I see to take further the analysis of your hydrological data is that the soil moisture sensors are placed quite deep, where infiltration during the pp event only would happen in case of soils under a certain type of vegetation or superficial cover that would allow macropore flow. Even like that, if you find soils with soil water content close to field capacity at that depth this can be interpreted as a positive indicator of good soil conditions of this patch-type (see discussion on soil water redistribution models and runoff generation mechanisms in Calvo-Cases et al., 2003).
11. Line 19 of page 9941, adding to the discussion of control of abiotic processes on overland flow under high overland flow volumes the results of Davenport (1998), Cammeraat et al. (2002) (which suggest that at lower scale levels biological controls are very important because are related to the finest temporal scales, and abiotic processes start to be more dominant at the higher scale levels). Control of biological factors in soil-erosive processes becomes more important when aridity decreases, while with an increase in aridity, mineral factors become more important (page 525 from Boix-Fayos et al., 2005).
12. I do not understand the last paragraph of the discussion (lines 15-30) on temporal dynamics on source-sink evolution. It is interesting but does the experimental design allow making this temporal analysis? I thought that the differences in presence of species among slopes was due to different morphological conditions of the slopes. I thought all the slopes were reclaimed at the same time and had the same stage of ecological succession. If I misunderstood, could you please make those different stages of ecological succession of the slopes clear in the description of the study area?
13. I find the conclusions well written and expressing the main findings, I wonder if you could incorporate some criteria/applied conclusion to transfer knowledge for management or design of reclaimed slopes.

Nice paper,

Carolina Boix-Fayos