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Hydrology and Earth System Sciences Discussions

Interactive comment on "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 et al.

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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.

We have better defined the terms "overland flow gradient" and "hydrological diversity" between brackets in the abstract (Revision, page 2, lines 8 and 21) and included a brief explanation on methods (Revision page 7, line 17).

2) I suggest also renaming the term "patches" to "patchâĂŘtype" 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.

We have included the suggested modification renaming "patches" to "patch type" (Revision, page 10, line 5; page 11, line 19; page 13, line 16; page 14, line 14; page 15, line 18; page 16, line 2; page 18, line 7; page 20, line 6).

3) The paper uses updated references, including some key papers of the last two decades in vegetation patternsâĂŘhydrological responseâĂŘgeomorphological 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.

We have included some of the suggested references (Boix-Fayos et al. 2005; Cammeraat, 2002; Mayor et al. 2008, 2009). Davenport et al. (1998) was already included (Revision page 5, line 5).

4) Concerning the methods section: a. 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?

As proposed, we have included these considerations in this section improving the description of the study area and adding a location map as a new figure 1 (Revision, page 7, lines 14-15).

b. Between 17âĂŘ19 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. We have included the proposed modification (Revision, page 8, lines 23).

c. 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?

We have included the proposed modification (Revision, table 1). Soil depth was similar in the three slopes under study. It was from 80 to 100 cm.

d. 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 patchâĂŘtype is present at each slope, and how many plots were installed at each slope?

We have included the suggested changes in table 2. We have modified the paragraph to clarify the number of patch types: “Overall, we found seven different types of patches dominated by different plant species (see Table 2 for a detailed description of vegetation cover and soil structural properties of patches). Those patches were: (1) scattered clumps of legumes (M. sativa) and (2) grasses (Dactylis glomerata) in a matrix of bare soil. Scattered dwarf shrubs dominated by (3) Santolina chamaecyparissus and by (4) Thymus vulgaris in a matrix of bare soil. Finally, we found patches densely covered by grasses dominated by (5) L. perenne and (6) Brachypodium retusum and others by (7) shrubs (Genista scorpius)” (Revision, pages 9-10).

e. 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?).

Although we have detailed topographical data, the resolution of these data is not enough to identify micro-catchments at plot scale, so we decided to delimitate the catchments by visual identification regarding to topography and vegetation barriers.

We didn’t mention the use of vegetation barriers to delimitate the micro-catchments so we have added this information in the methods (Revision page 10, line 14).

f. Why did you placed the TDR sensors at 25 cmâĂŘdepth? Which criteria did you follow: root depth, soil horizons, nonâĂŘcrusted 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 saturatedâĂŘrunoff 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. We have placed the TDR at 25cm depth because we wanted to study the interrelationships between patch cover and infiltration (mainly driven by macropores). The soil moisture sensors were placed at that depth, because the infiltration during the rainfall would only happen in case of soils under a type of patch that would allow macropore flow. Moreover, the study area presents important weathering processes and we consider that if we had placed the TDR deeper, soil moisture would have been more influenced

by factors other than infiltration caused by soil-vegetation interrelationships.

We agree with your suggestion, the first 10 cm are crucial for runoff generation and it would be very interesting to have measured soil moisture at that depth. However, we didn't have continuous soil moisture measurements, so we couldn't record the soil moisture data before the rainfalls (i.e. the antecedent soil moisture).

For those two main reasons, we decided to install the TDR sensors at 25 cm depth. Following your suggestion, we have better explained the criteria for installing the TDR sensors at that depth: "In order to test differences in soil water content between types of patches, TDR (Time Domain Reflectometry) sensors were horizontally installed at 25 cm into the soil. The TDR sensors were placed at 25 cm depth to study the interrelationships between patch cover and infiltration (mainly driven by macropores)." (Revision page 11, line 19).

g. 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.

We carefully considered this suggestion but we finally did not include the soil sampling and analysis methodology in the methods section as a paragraph because soil traits, cover features, plant traits and AWHC were sampled with different methodology and it will make the methods section too dense. In addition, these results are to characterize the slopes. However, we have improved the description of data collection on the footnote of the table 1 to facilitate the reproduction by scientists and we have added a reference of the standardized methods used for physico-chemical soil characteristics (MAPA, 1994).

We have also included the suggested reference about the pF determinations methodology: Richards, L. A.: A pressure-membrane extraction apparatus for soil solution,

Soil Sci., 51, 377-386, 10.1097/00010694-194105000-00005, 1941.

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).

We have improved the description of the sediment yield data at both scales (Revision, section 3.2.)

6) In subchapter 3.5 a more extended description of the microâĂŘtopographic forms is missing, it could be illustrated even with some photographs.

We have extended the description of the micro-topographic forms in the results (Revision, section 3.5).

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.

We have included the suggested structure.

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).

We have added the work of Arnau Rosalén et al. (2008) to complete the discussion: "These findings are consistent to those obtained by Arnau Rosalén et al. (2008) who found that the B. retusum patches have a high infiltration capacity but reach runoff

stability (i.e. steady runoff) very quickly" (Revision, page 19, lines 18-20).

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.

We found your suggestion very valuable. However, the objective of the TDR sensors placed at 25cm was to see the interrelationships between the vegetation and the infiltration and not the runoff generation mechanisms. That is the reason why the sensors were placed quite deep.

A detailed analysis of the runoff generating mechanisms would require continuous measurements of soil moisture at low depth (5-10 cm). Unfortunately, the inclusion of this type of analysis is not an option since we do not have such data. However, we will consider this type of analysis for new research in our study areas.

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).

We agree on your comment. Actually, we placed the sensors at 25 cm depth to check for important differences related to macropore infiltration. Following your recommendations, we have included some new information on the soil moisture content at field capacity in table 2, but we didn't find enough arguments to build a discussion about it.

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).

We have added the aridity gradient discussion, including the research by Boix Fayos et al. (2005) and previous research by Lavee et al. (1998) (Revision, page 22, lines 6-9). However, we did not find appropriate the inclusion of the "scale issue" on this paragraph.

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?

We agree with your suggestion. All the slopes were reclaimed at the same time so

we cannot make a temporal analysis and it was not our intention. However, we admit that in the previous paper version, the last paragraph seemed to describe temporal succession stages.

We have modified the text to clarify that the differences in presence of species (and hence patch types) among slopes are due to different overland flow volumes (Revision, page 24, lines 23). Overland flow is here influencing highly the vegetation composition and as a consequence the trajectory of succession (i.e. the observed ecosystem states).

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.

We have included a set of applied conclusions as a subchapter in the discussion (Revision, section 4.3).

Thanks for your extensive and comprehensive revision. It has been very helpful for improving the final manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 9927, 2011.