

Review paper submitted to HESS entitled “Reducing the hydrological connectivity of gully systems through vegetation restoration: combined field experiment and numerical modeling approach”

General comments:

In general, the topic presented in the paper is highly relevant to understand and model runoff and erosion in degraded mountain areas. Therefore, the manuscript is well placed in the journal. However, before being publishable in HESS the following general comments should be addressed:

- (1) The authors should be more precise regarding the objective of the manuscript, which is due to my opinion not to predict surface runoff within gullies, but to understand the controlling parameters and their interaction via applying a model. To use the term prediction throughout the manuscript is therefore misleading as it is not possible to predict runoff with a model that needs input parameters which must be derived during an experiment, which is later on “predicted”.
- (2) Regarding the model results I would suggest a more detailed discussion. E.g. page 2551, line 7-17, the problems during the simulation of the San Miguel 2 experiments should be discussed more extensively. The failure of the model in this case seemed to be a problem of an overestimation of infiltration after the end of the inflow. This probably results from a shallow soil layer on impermeable bedrock which leads to saturation runoff. However, it must be discussed here that the used Philips equation assumes a homogenous, deep soil layer and hence the infiltration approach is probably not suitable in this case. This could also be a problem in case of prolonged inflow and infiltration.
- (3) The sensitivity analysis is an interesting part of the manuscript. However, I suggest using not only one dry run but also a wet run to understand the system in more detail. Moreover, I have some general doubts if it is helpful to vary only one parameter at once, especially if these parameters are highly interrelated. Especially, in case of Manning’s n where the authors conclude that it has only a small effect on gully outflow I think the variation of only n leads to an underestimation of the vegetation effect, because a change in n would also change runoff width (as for a constant inflow rate a reduced runoff velocity must result in a change of runoff cross section and hence width). As the outflow is highly sensitive to runoff width I suggest also a highly sensitive behavior regarding Manning’s n . Moreover, some discussion about the variability of n in case of grass-shrub vegetation would be interesting.
- (4) My main criticism refers to chapter 4.3. I do not see any increasing potential to transfer the model to other gully systems in using an average (measured) S and estimating K from other parameters, as both are dependent on initial soil moisture content. If the authors intend to develop a more transferable approach I suggest to use existing pedotransfer functions to derive the Philips parameters S and K from soil moisture, grain size distribution etc. and to focus on a more easily transferable parameterization of the model, without using measured data (e.g. runoff width during experiment).
Moreover, I do not follow the authors regarding the interpretation of the effect of vegetation on K . I agree that there is a (strong) effect of vegetation cover on infiltration capacity due to the prevention of crusting and increasing macroporosity. However, the parameter K in the Philips equation represents the hydraulic conductivity through those pores which are filled with water at the beginning of an infiltration event (K is not equal to saturated hydraulic conductivity); hence neither the effect of crusting nor the effect of macroporosi-

ty can be accounted for with K . In general, I suggest reworking this chapter or to fully delete it and focus on results and discussion in chapter 4.1 and 4.2.

Specific comments:

p. 2542, line 18 ff: Give more detailed information regarding measuring of vegetation cover; ground vegetation relevant in case of a flow experiment cannot be determined taking woody vegetation and shrubs cover into account;

p. 2543, line 12 (and others): Give units for all variables; variables should be always in italics.

p. 2543, line 11 and p. 2545, line 2: The variable S is used for slope and sorptivity. Change this throughout the manuscript and check all variable names for unambiguity.

p. 2543, line 24: It is unclear why this equation is given here. It is not used in the following text.

p. 2546, line 1 and 24: Use consistent abbreviations and names of variables. S_0 is bed slope and S is slope of the channel, which both should be the same.

p. 2546, line 20-21: Change ...'The relation between Q and A in the continuity Eq. (5) can then be expressed by the Manning's Eq. (7).' ... in ...'The discharge Q can be expressed combining the Manning's Eq. (1) with $v=Q/A$ to yield Eq (7).' ...

p. 2546, line 22: Eq. (7) should be written as Eq. (1) or vice versa.

p. 2548, line 11-12: Where are n_{0-4} taken from?

p. 2548, line 12-15: That is confusing. How could the lateral inflow rate computed as the difference between rainfall and infiltration? Why is the lateral inflow rate equal to infiltration?

p. 2549, line 13: On page 2544, line 12 inflow is q_{in} .

p. 2549, line 21: Is +/- 23 the standard deviation?

p. 2550, line 10-15: I suggest reworking this paragraph. Describe the results of the multiple regression (give equation and partial and total R^2) and delete Tab. 3.

p. 2550, line 18-22: Was this calibration procedure carried out for steady-state runoff?

p. 2551, line 7-17: This paragraph should be substantially reworked taking the following into account: (i) I suggest not to use the term predicted because it is only tested if the model is able to reproduce measured data which are used for calibration. (ii) It should be discussed why the San Minguel2 simulation is also poor in case of dry runs. (iii) According to Fig. 4 the main simulation problem seemed to result from an underestimation of the duration of runoff. This indicates that the model allows for afterflow infiltration (infiltration after inflow ends) while this is not the case during the experiment. This indicates a shallow soil layer (which is probably saturated at the end of the inflow). In case of longer experiments this could be a general problem of the model (which should be discussed) as the Philips Equation assumes homogeneous soil over the total soil profile.

p. 2552, line 15 – p. 2553, line 3: The result that the system is only marginal sensitive to Manning's n is misleading and only true if only n is varied in an uncoupled system. Under real conditions a change in n would affect runoff width and hence has a much more pronounced effect on runoff. For a sensitivity analysis I suggest to change runoff width together with n to get a more realistic sensitivity.

p. 2556, line 5-8: In general I agree with this statement, but it is difficult to derive this conclusion from chapter 4.3.

p. 2556, line 11: The interaction of vegetation (Manning's n) with runoff width is the reason for the marginal sensitivity to changes in n (see above).

p. 2561, table 2: It is unclear why the runoff width does not change between dry and wet runs. I guess in the wet runs there should be less infiltration and hence runoff width should increase. Discuss why the soil moisture is as high as 49% m m^{-3} in case of the dry run in the Carmenjadan1 gully.

Technical corrections:

p. 2538, line 9 ff: use the term 'runoff' and not 'runoff water'

p. 2538, line 11: 'nine' instead of '9'

p. 2538, line 13: delete 'of the channel'.

p. 2538, line 19-20: do not use quotation marks for 'dry runs' etc.

p. 2538, line 24: change ...'the kinematic'... to ...'a kinematic'...

p. 2538, line 27: change ...'The sensitivity'... to ...'A sensitivity'...

p. 2538, line 27-28: Delete ...'to predictions of transfer of runoff flow in the gully channel'...

p. 2539, line 15: Change ...'In natural'... to ...'Under natural'...

p. 2540, line 29: Change ...'runoff water'... to ...'surface runoff'...

p. 2541, line 1-12: Shorten this paragraph;

p. 2541, line 21-23: Unclear;

p. 2542, line 15: I guess that the gullies which were tested were no ephemeral gullies;

p. 2543, line 1: Use m^3 instead of liters;

p. 2543, line 21-22: Delete the sentence... 'The n_4 ...of vegetation' ..., this information is already given above.

p. 2544, line 1: Delete 'Large' at the beginning of the sentence.

p.2547, line 4: Insert '(Eq. 5)' after continuity equation.

p. 2548, line 1: Delete these equations as these are already given above.

p. 2548, line 5-6: Give a reference here.

p. 2549, line 17: Variables should be always in italics.

p. 2550, line 22-26: I suggest to replace this sentences, e.g. 'To determine the quality of the modeling results three goodness-of-fit parameters were used: (i) ..., (ii)..., and (iii)'

p. 2551, line 22: Delete 'water'

p. 2552, line 8: Change ...'prediction'... to ...'simulation'...

p. 2552, line 23-24: Change ...'is unable'...to ...'do not'...

p. 2552, line 26-27: Change ...'On the other hand, a 3% increase in K both the runoff volume and the time to runoff were not predicted.'... to ...'On the other hand, a 3% increase in K results in the simulation of no runoff.'

p. 2553, line 22: The Variable E is not given.

p. 2558, line 3-4: Le Bissonnais et al. is not cited in the text.

p. 2560, table 1: Symbols should be exactly the same as used in the equations, e.g. runoff velocity is in capitals in Eq. 1; S_0 should be S_0 ; give a short description of alpha and beta; use ‘-‘ instead of ‘dimensionless’; delete the term component in the first column;

p. 2561, table 2: All variables should be in italics; S_0 should be S_0 ; the table would be much easier to read if those values which do not change between the dry and the wet run are given only once;

p. 2562, table 3: I would delete this table – see above.

p. 2563, table 4: The Headings of the columns are somewhat confusing. I suggest including an error value for all simulations.

p. 2565, table 6: Variables should be in italics.

p. 2569, figure 4: Difficult to read in its actual size;

p. 2570, figure 5: s.a.

p. 2571, figure 6: I suggest using a consistent scale for all y-axis which would make it easier to compare the sensitivity of the model to changes in input variables.