Journal: HESS
Title: Reducing the hydrological connectivity of gully systems through vegetation restoration: Combined field experiment and numerical modelling approach.
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MS No.: hess-2009-77

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:

Reply to Comments of Reviewer1

(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".

REPLY: It is correct that the main aim of this study is to understand the main factors controlling surface runoff in gullies, rather than predict runoff quantities. We have developed a kinematic wave approach to understand the interaction of various controlling parameters. We agree with the comment of the reviewer, and have changed the terms 'prediction' by the terms 'simulation' throughout the manuscript.

(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.

REPLY: We now discuss our results in more detail. It is clear that the simulation for the San Miguel2 area, an active gully, was influenced by the fact that impermeable bedrock was exposed in the gully bed. The San Miguel2 gully is an active gully channel, which is subject to erosion during flow events. The reviewer is correct that the Philips equation is not suitable to assess infiltration correctly in such cases. This is now clarified in the text.

(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.

We largely expanded our analyses, and now include the results of the sensitivity analysis of a 'dry' and 'wet' run. Our results indicate that the sensitivity of the model is highest for dry hydrological conditions (see discussion in the text and Figure 7). As vegetation affects both the runoff width as the roughness of the gully bed, we also conducted a sensitivity analysis by varying Manning's n and runoff width W simultaneously. We observe that there exists some dependency among these two input parameters, which might be the main reason of the apparent marginal sensitivity of the model to changes in Manning's n. This is now discussed in more detail in the text (Chapter 4.2).

(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).

Pedotransfer functions are often used to derive e.g. hydraulic conductivity based on grain size distribution, soil moisture and soil depth. Various authors have shown that this gives reasonable results for deep, homogeneous soils. In our study, we observe large spatial variability in soil depth, grain size distribution and vegetation cover within one gully system. If we want to use pedotransfer functions to estimate e.g. infiltration rates for the various gully sections, we would need very detailed information about the sedimentation in the gully bed (grain size, thickness of sedimentation, vegetation cover, etc.) to obtain reliable results. This would not only demand a lot of additional field measurements, but would also be highly dependent on the applicability of the pedotransfer function used. Therefore, we have chosen to estimate the parameters S and K from an experimental regression model that was specifically fitted for the gully systems under study. It has been shown by various authors that this approach provides more reliable results when working on small study sites with highly variable soil characteristics.

Moreover, I do not follow the authors regarding the interpretation of the effect of végétation 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 macroporosity can be accounted for with K.

REPLY: Here we caused confusion. We see K2 as a fitting parameter: as the model is simplified and has only one parameter that will accommodate for variations in infiltration, K2 does no longer have a physical meaning but its optimized value will vary as a function of all factors affecting the infiltration rate. By looking at factors affecting variations in K we may obtain a better understanding of the controls on infiltration rates on gully beds.

We agree with the reviewer that the section as it was originally formulated could mislead readers, as we did not clearly indicate our intentions. We reduced the section in length and clearly stated that K2 should be regarded as a fitting parameter only, having no true physical meaning as it has to accommodate for all changes in infiltration.

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.

REPLY: We have reworked this chapter 4.3. thoroughly. The text has been rewritten, and new tables and figures have been added. We particularly added the results of some additional sensitivity analyses for dry and wet runs. Instead of the previous approach where we varied only one parameter at once, we now analyze the effect of several interrelated parameters on the predicted outflow.

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; We have rephrased this section. It is clear that herbs and grasses are particularly efficient in covering the gully bed. Although shrubs and trees do not provide a high ground vegetation cover, their root system is well developed and provides additional cohesion and shear stress to the gully bed. We now specifically refer to the study by Vanacker et al. (2007) that describes the methods.

p. 2543, line 12 (and others): Give unites for all variables; variables should be always in italics. Ok. This has been changed. The unit for the Manning's coefficient has been added.

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. We have checked this, and now use the variable name S_{gully} for the slope of the gully bed, and S for the sorptivity.

p. 2543, line 24: It is unclear why this equation is given here. It is not used in the following text. We have deleted this equation, as we do not use this information (Froude number) for our analyses.

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. See reply to comment above.

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).'… Ok.

p. 2546, line 22: Eq. (7) should be written as Eq. (1) or vice versa. Ok. The equation 7 was rewritten.

p. 2548, line 11-12: Where are $n_{0\text{-}4}$ taken from? These values were taken from lookup tables that were published by Cowan (1956). We now clarify this in the text.

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?

We have rewritten this sentence. The lateral inflow rate in gully section X equals the outflow rate of gully section (X-1), as the experiments were realized in the absence of rainfall.

p. 2549, line 13: On page 2544, line 12 inflow is q_{in}. Here, we make reference to the total volume of inflow, i.e. the total volume of water discharged to the gully head (RI). This value is different from q_{in}, which is a time-dependent inflow rate.

p. 2549, line 21: Is +/- 23 the standard deviation? As this was not clear in the text, we now added twice the annotation '(1 s.d.)' in the text.

p. 2550, line 10-15: I suggest reworking this paragraph. Describe the results of the multiple regression (give equation and partial and total R2) and delete Tab. 3.

The results of the multiple regression are given in full detail in Table 3 (including parameter estimates, partial and total R^2). We preferred to keep Table 3 instead of describing the results of the multiple regression analysis in the text.

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

The kinematic wave model was calibrated by adjusting the values of sorptivity and hydraulic conductivity. The criterion used for calibration was the goodness-of-fit between the observed and modeled hydrograph for the entire flow experiment. We have clarified this in the text, and have rewritten this section.

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 after flow 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 homogenous soil over the total soil profile.

We have reworked this paragraph completely (see also reply to comments above). It is clear that the poor result of the kinematic wave model for the San Miguel2 gully is related to the poor representation of the infiltration processes by the Philips Equation. This is now discussed in much detail in the text. Besides, we have systematically replaced the term ‘predicted’ by ‘simulated’ throughout the text (see reply to comments above).

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. The sensitivity analysis has been largely expanded. We now include a sensitivity analysis for a ‘wet’ run, and also analyzed the effects of variations in Manning’s n and runoff width simultaneously. We observe that there is some interaction between these two parameters. Vegetation in the gully beds clearly affects both the bed roughness as the runoff width. These results are now discussed in more detail in the text, and the paragraph on the results of the sensitivity analysis has partly been rewritten.

p. 2556, line 5-8: In general I agree with this statement, but it is difficult to derive this conclusion from chapter 4.3. Our data indicate that the model is highly sensitive to the input values of sorptivity, hydraulic conductivity and runoff width. In our study, we had very detailed information on these input variables available from field measurements. As this information is often hard to get, we tried to estimate these parameter input values from some general gully characteristics (see chapter 4.3). The discussion at the end of Chapter 4.3 indicates that the prediction of parameter values remains difficult : “*We therefore also investigated to what extent predictions of runoff volumes by the model described above agreed with observed values if the hydraulic conductivity predicted by Eq. (12) was used while measured values were used for all other model parameters. Figure 6c shows that predictions are poor. Generally, predicted runoff volumes are of the correct order of magnitude, but the relationship between predicted and observed runoff volumes is not statistically significant. »*

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). Correct, this is also shown by the sensitivity analyses. We clarified this in the text.

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.

During our experiments, we observed that the runoff width equals the width of the sediment deposition in the gully bed. This sedimentation area is observed as a flat area, and the flow dissipates over this flat gully bed. This can be seen clearly on Figure 2 (photo of flow experiments). We observed this phenomenon during dry and wet runs. This explains why the runoff width does not change between dry and wet runs.

The high soil moisture content of the CarmenJadan1 gully results from the fact that the gully bed was not completely dry before the flow experiment. Because of some drizzle before the flow experiment, the gully bed was slightly wetted. We still classified this experiment as a 'dry run', because the hydrological characteristics of the gully bed were far from being saturated. We now clarify this in the text.

Technical corrections:

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

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

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

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

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

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

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

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

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

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

p. 2541, line 21-23: Unclear; Rephrased.

p. 2542, line 15: I guess that the gullies which were tested were no ephemeral gullies; Correct. They were permanent gully features.

p. 2543, line 1: Use m3 instead of liters; Ok

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

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

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

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

p. 2548, line 5-6: Give a reference here. We added a reference to the work by Jaber and Mohtar (2002) on the stability of finite element schemes.

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

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)' Ok

p. 2551, line 22: Delete 'water' Ok

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

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

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.'Ok

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

p. 2558, line 3-4: Le Bissonnais et al. is not cited in the text. We now make reference to the study of Le Bissonais et al. (2004) In the Introduction.

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; *So* should be S_0; give a short description of alpha and beta; use '-' instead of 'dimensionless'; delete the term component in the first column; We have rearranged Table1. As the parameters alpha and beta are just calculated from hydrological data following the equation 10, we deleted these two parameters from the table.

p. 2561, table 2: All variables should be in italics; *So* 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; Table 2 has been re-arranged.

p. 2562, table 3: I would delete this table – see above. See above, we preferred to keep Table 3 as this table resumes well the results of the multiple regression analysis.

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

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

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. The figures have been improved taking into account the suggestions of the reviewers.