Response Letter to Reviewer Comments

Interactive comments by an anonymous reviewer and our response

The reviewer comments, reproduced below in italics, can be grouped into four categories: A: the scope and relevance of the paper, B: the structure of the hydrological model, C: miscellaneous specific comments, and D: figures and tables.

A. Issues related to the scope and relevance of the paper

In its current form, I cannot recommend the publication of this paper because it does to my view not make a valuable scientific contribution to "hydrology and earth system sciences" and is not within the scope of the journal. The proposed method answers two questions - i) what is the relationship between the objective function and the decision variables?, ii) are the solutions close to the global solution? – without even discussing why the answers to these questions would be relevant for hydrologic sciences. The results are likely not to be transposable to any other problem. Maybe their method to investigate the optimization problem could be interesting but only if they show how to make use of the results.
One of the contributions of the paper, with respect to previous publications of the authors, consists in addressing the question whether the identified solution is the global optimum solution. The authors do not discuss the fact that this question is probably rather irrelevant: For management decisions, knowing that a solution is the global optimum is probably not crucial, it is far more interesting to know that there is a range of solutions with almost the same impact / benefice. For environmental modeling in general, there appears to be an agreement that identifying the global optimum solution is less important than having a good idea of how uncertain this solution is (given all parameters, assumptions etc.) (e.g. Beven and Freer, 2001). For the problem at hand here, it seems not really relevant to come up with a complicated method to assess the global optimality of a solution that is the result of numerous simplifications.

The main criticism is whether the paper is appropriate for publication in HESS, and if the questions addressed by it are scientifically relevant. As suggested by the reviewer, we plan to revise the manuscript by clarifying/restating its scope and objectives. However, we respectfully disagree on the point that the paper does not provide any valuable scientific contribution and is not within the scope of HESS, as it investigates the interactions between hydrological processes and spatial land use patterns and provides optimal land patterns for sustainable water management, as outlined in the Aims and Scope of HESS.

The purpose of the paper is primarily to propose a methodology, and secondarily to illustrate it using a specific hydrological simulation model and a specific site with available data. Much discussion has been related to the specifics of the simulation model used, thus distracting from the primary focus of the paper. We recognize that the organization/presentation of the paper must be improved to make that clear.

Most natural systems (hydrological, atmospheric, etc.) are represented by simulation models of varying degrees of sophistication/complexity. The inputs to such models include exogenous variables, decision variables, and parameters. In the specific case of runoff models:

E = vector of exogenous variables, such as the geographic distributions of soil types, topography, precipitations, etc. For a given site, these variables cannot be modified, at least in the short/middle terms, and are taken as given;

X = vector of decision variables, which represent various possible human management/planning interventions, such as the allocation of land uses or the siting of ecological engineering technologies (e.g., constructed wetlands or filtration systems);

P = vector of the parameters that characterize the various equations/relationships that make up the simulation model (e.g., the CN number, Manning's coefficients).

Let **Y** be the vector of the simulation output. In the present case, there is only one output – the peak runoff. However, other models (SWMM, SWAT, etc.) would also provide pollutant loads, etc. The simulation model is essentially the following mapping:

$$(\mathbf{E}, \mathbf{X}, \mathbf{P}) \xrightarrow{F} \mathbf{Y}$$

The functional relationship **Y** = F(**E**, **X**, **P**) is implicit and cannot be expressed in closed mathematical form because of the complexity of the simulation model, which is generally run for a discrete number of scenarios pertaining to the decision vector **X**, and/or for different geographical settings (vectors **E** and **P**).

The proposed methodology is to help understand the structure of the implicit function F, and to find the vector **X** that optimizes the output **Y** subject to constraints. This is done by numerically approximating the gradient vector $\partial F/\partial X$ by repeatedly running the simulation model for all possible increments in the decision variables. This, of course, could be done for any simulation model, hence the general value of the methodology. A standard nonlinear programming algorithm is used to reach the local optimum corresponding to a given initial solution. Using a large number of different initial solutions (500 in this paper), two situations may emerge:

(1) the same local optimum is obtained in all cases (as in this paper), which indicates that the function F is convex (case of minimization); or

(2) different local optima are obtained which indicates that the function is not convex; in this case, the paper presents a probabilistic method to assess the closeness of the best local optimum to the global optimum.

The reviewer questions why one might want to obtain the global optimum. In our view, there are two fundamental reasons. First, because a necessarily limited number of simulation runs may yield significantly inferior solutions, knowing the global optimum may help avoid this situation. Second, more importantly, the global optimum is the benchmark to be used when assessing heuristic procedures that yield good, but not necessarily optimal solutions. Heuristics are much less computationally demanding, but have no value if they cannot be evaluated. The operations research literature has offered many heuristics for difficult-to-solve optimization problems.

The reviewer brings into the discussion the concept of equifinality, whereby the same output **Y** can be obtained with different sets of the input parameters **E**, which are characterized by measurement or other uncertainties. The uncertainty of the input parameters is a very general modeling issue, that applies as well to socio-economic models (e.g., future unit prices or costs). This uncertainty can be tackled, in part, through sensitivity analyses or with stochastic programming techniques. However, the focus of the proposed methodology is not on the vector **E** but on the vector **X** – the management/planning decision variables.

Finally, we recognize that runoff and nonpoint source pollution are not the only factors to be considered in the land allocation process, and that other constraints and objectives must be brought into applicable models. This point is further discussed in our response to the first reviewer.

We propose to reorganize the paper to emphasize the above methodological discussion, and to shift the description of the hydrological model to the application section.

B. Issues related to the hydrological model

i) The curve-number method is not a process-based model (contrary to what is said in the paper p. 3547) but an empirical method which can only be used for the purpose it has been developed for. Using the method in a distributed way as suggested here, is to my view questionable for the following reason: The proposed method makes the assumption that overland flow production at each cell is independent of overland flow production at surrounding cells. This is not realistic since flow is routed from one cell to another and some flow generated at 1 cell could contribute to generate flow in another cell or simply infiltrate there.

We agree with the reviewer that the CN method is an empirical one, in the sense that the loss from infiltration is empirically derived using the curve number, which changes with soil type, land use/treatment, surface condition, and antecedent soil moisture. However, the hydrological model used in this study is process-based, as it simulates the different components of the hydrological cycle over the watershed area. Contrary to what the reviewer states about the routing process, the hydrological model does not assume that the overland flow production at each cell is independent of the overland flow production at surrounding cells. Rather, it assumes that the infiltration capacity (i.e., the initial abstraction) at the cell only depends upon on-site characteristics, such as soil, land uses, surface characteristics, and antecedent soil moisture. At the cell level, the initial abstraction is computed and compared with the depth of precipitation. If the on-site infiltration capacity exceeds the precipitation depth, runoff at the cell is generated and computed while accounting for the upstream runoffs routed through the flow path. The following scheme (Figure 1, excerpted from Yeo et al. 2004) explains how the runoff at the cell and along the path is computed. To clarify, we will add a similar diagram in a revised paper.

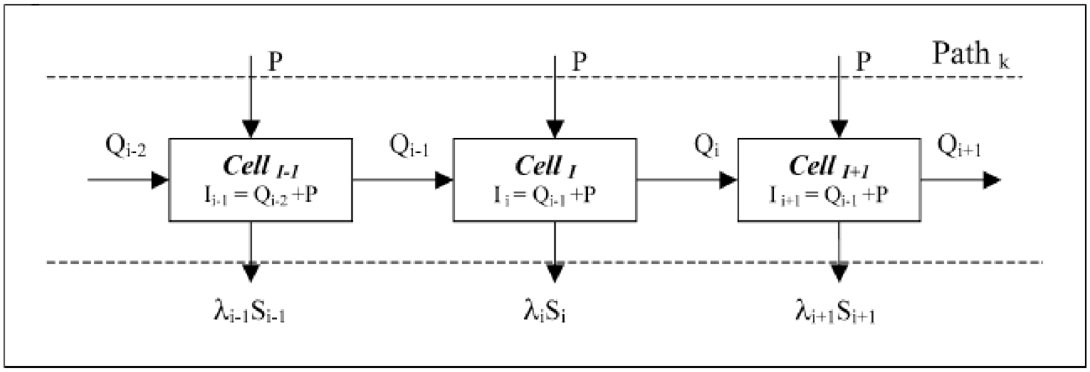

Figure 1. Runoff process and flow path.

ii) The paper considers a single event and identifies an optimal land-use pattern for reduction of peak-flow. This pattern is only valid for this particular event. How would this pattern react to other events? How can you know that this pattern does not increase peakflow for some other storm event? In (Yeo et al., 2004), presenting a very similar methodology, there are indeed different optimal patterns for different storm durations (beside this, the assumption that simply reducing the event storm flow from the entire catchment at some random moment in the year also reduces the mean annual or peak load of nonpoint source pollutants, is a questionable simplification; for mean annual load, questions of timing of the rainfall event would need to be addressed. For peak load, again, the event timing with respect to the growth season would be critical).

We agree that different optimal land patterns are obtained with different size storms, and Yeo et al. (2004) fully discuss how these spatial patterns reduce the on-site generation of surface runoff and its delivery to surrounding areas and watercourses, and why different patterns were delineated for different storms. This earlier study shows that land management as a BMP is most effective with a small size storm. As the focus is here on the effectiveness of land use as a BMP to reduce the peak runoff, it is reasonable to choose a small design storm, such as 1-year storm. However, we agree that optimizing under 1-year design storm is clearly different from optimizing for the annual load. The proposed methodology can be easily extended to deal with multiple storms and to delineate an optimal land-use pattern for multiple storms, by employing a continuous watershed model, instead of an event-based model to simulate the annual load. Consider a representative year subdivided into T ($t=1 \rightarrow T$) precipitation periods. For a given, time-independent, land-use pattern subsumed by vector **X**, the peak runoff for period t would be $F_t(\mathbf{X})$, as computed by the simulation model under the conditions of period t. Minimizing, for instance, the aggregate annual runoff, $\sum_t F_t(\mathbf{X})$, could be implemented with the same optimization procedure. It simply would be lengthier and more computationally demanding because gradients would have to be calculated for each period. Such possible extension will be outlined in a revision.

(iii) As far as I understand, the paper presents a model that has been presented in (Yeo at al., 2004) and applied in (Yeo et al., 2007). The current paper adds to these two the investigation of the behavior of the objective function and an assessment of the global optimality. Both aspects are completely case-specific (rain type, size, catchment size, structure etc) and I do not see in how far this is interesting. Especially since given all the assumptions in the whole approach, what do you learn from knowing that a solution generating 0.254123 m3/s of peak flow is the global optimum solution within an interval of solutions covering [0.254073, 0.254298]? (these are the interval numbers given in the text).

Results from any optimization and/or simulation model are site- and case- specific, as the modeling is done using parameters that describe the physical/social characteristics of the study site. However, the modeling approach is transferable to other sites/geographical areas. This has been discussed earlier.

(iv)The authors state in the conclusion "This paper has investigated and characterized the relationship between land-use patterns and watershed hydrology." In fact, this should be rephrased into "this paper has investigated and characterized the relationship between land-use patterns and the peak-runoff generated with the curve number method". This illustrates that the paper studied in detail the behavior of the model for a small catchment but not of the natural system. Whether the results are relevant for modeling / understanding / managing the given natural system is not discussed. In addition, the paper does also not discuss whether the findings are relevant for transposing the method to much bigger or otherwise different catchments. This last question could be addressed by studying the behavior of the model for higher concentration times, other curve-number distributions etc. Since for bigger catchments, the rainfall spatial structure certainly becomes relevant, I guess that the method will not be applicable in this simple form.

We agree that the relationship between the land use pattern and watershed hydrology is investigated using the framework of the CN method. This is clearly stated in the manuscript, and will be articulated again in the conclusion, as suggested by the reviewer. The other points raised by the reviewer are related to the scope/relevance of the approach, which we have discussed earlier.

C. Detailed comments

- The method for assessing the closeness of a local optimum to the global optimum is not clear in the paper. I do not understand it.

This closeness is assessed by constructing a 95% confidence interval using the Weibull distribution. The detailed mathematical formula and derivation for the confidence interval are presented in Section 2.3.

- It is not clear how exactly you complete the optimization. Giving the algorithm would help the reader to see what you have actually done.

A discussion of the optimization procedure is provided in Section 2.2 (with references to the three previous papers) and the stopping condition for completing the optimization is discussed in Pg 3555-3556 (ln 27-28 & ln 1-5). The gradient method is applied to find the optimal solution (as discussed in p 3550-3551) approximating the implicit nonlinear runoff function using a first-order Taylor's series expansion (P 3550, eq 7)

- Why is the fact that the Weibull distribution is independent of the parent distribution relevant here? What do you mean by that (don't forget that the readers of HESS are not statisticians)

Assumptions on the parent distribution are critical in constructing a confidence interval (CI) for the global optimum, as they are used to derive the statistical parameters that determine the lower or upper bound of a CI. The optimization procedure only provides the extreme value (the maximum or minimum) for a give problem, and their probability distribution remains obscure. The Weibull distribution does not require such assumption to derive the probability of the extreme values, as long as there are sufficient data available (Roberts, 1971). We will further emphasize this point in a revision of the paper.

- What benefice do you draw from assessing how close the local optima are to the global optimum? A possible application would be to later on use a few local optima to derive the global optimum. But you have no idea whether your analysis holds for other storm types, storm sizes, catchment configurations, catchment sizes etc.

Indeed, the optimal land distribution is specific to the study site, and it is not possible to extend the specific results to another site. However, as discussed earlier, the methodology can be applied to any other site, and also to any other simulation model.

- How is it possible to obtain 9 identical initial patterns in a sample of 500 containing each over 1500 cells?

The paper does not state that there were 9 identical initial patterns, but rather that nine identical local optimal solutions were obtained (P 3455, ln 1-6). All the initial patterns were different from each other and we will clarify this point in a revision.

All the results are reported up to a precision of 0.000001 m3/s. Given the catchment size this is a precision of 0.00006 mm/ day !

The high precision is due to the fact that the simulation code is developed using double precision (i.e., floating point format with 15 digits) and we rounded up to the decimal point needed to show differences in the peak runoffs after optimization. Since the model provides local optima so close to each other, the 6^{th} decimal point was needed to show the differences.

If you wanted to illustrate the range of solutions, you should do this in the "decision variable" space since the objective function space (peak flow) shows virtually zero variation. Showing maps corresponding to percentiles of an interval covering 0.0002 m3/s is not interesting

We show both the initial maps (i.e., the decision variable space) and the optimal maps to show the variations in the solution. As noted in the paper, the initial maps had runoffs varying from 0.25 to 0.5 m^3/s

D. <u>Figures - tables.</u>

- Table 1: what are these numbers? Units?

Unit is given in the title (30 m cell). But we will clarify this by adding "the numbers indicate the amount of total cells assigned for different land use types"

- Table 2 3: why precision up to the 6th digit?

The computation is run with a high precision (with 15 digits of floating point), and the stopping criterion for convergence is when the difference in the decision variables between two iterations is less than 10^{-8}. Since the optimal solutions are very close, their difference must be pointed out at the 6^{th} digit.

- Fig 1: what is hydrologic soil distribution? What is A, B, C, what is the unit of the slope? There is no soil type D even if it is mentioned in the text

The hydrologic soil distribution (soil type A, B ,C, D) is the soil grouping used in the SCS-CN number method, and is related to the soil infiltration capacity. The USDA has tested more than 500 different soil types and classified them into four different hydrologic soil groups. The unit of the slope is given in figure (1.C). There is no soil type D in the study site.

- Fig. 3: the left figure does not at all have the same scale, is it really useful to present this spike as a histogram?

The figure shows the distribution of the peak runoffs before and after optimization, and naturally they have different scales, because the local optima are very close to each other.

Reference:

Roberts, K.L., 1971, A search model for evaluating combinatorial explosive problems, Operations Research, 19(6), 1331-1349.