

Interactive comment on “Analyzing the relationship between peak runoff discharge and land-use pattern – a spatial optimization approach” by I.-Y. Yeo and J.-M. Guldmann

Anonymous Referee #2

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The present manuscript proposes to address an important topic, which is how to reduce nonpoint source pollution through best management practices. Assuming that this can be achieved through reduction of peak flow, the paper suggests a method to optimize land-use such as to reduce peak flow.

The well-written paper proposes a new optimization method for a model that has been presented before (Yeo et al., 2004, 2007) and studies the relationship of the objective function (minimization of peak flow for a 1-year rainfall event) and the optimization decision variables (land-use on over 1500 grid cells). Furthermore, it applies a method to show that the identified local solutions are very close to the global optimal solution.

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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.

Having said this, maybe clearly stating the scope of the paper and illustrating the relevance of the method could make this paper suitable for HESS. I have however some important concerns about the modeling methodology:

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

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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.

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).

As far as I understand, the paper presents a model that has been presented in (Yeo et 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).

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

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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.

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.
- It is not clear how exactly you complete the optimization. Giving the algorithm would help the reader to see what you have actually done.
- 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)
- What benifice 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.
- How is it possible to obtain 9 identical initial patterns in a sample of 500 containing each over 1500 cells?
- 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 !
- 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

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m³/s is not interesting

Figures tables.

- Table 1: what are these numbers? Units?
- Table 2 3: why precision up to the 6th 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
- Fig. 3: the left figure does not at all have the same scale, is it really useful to present this spike as a histogram?

References

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Beven, K., J. and Freer, J., 2001. Equifinality, data assimilation, and uncertainty estimation in mechanistic modelling of complex environmental systems using the GLUE methodology. *Journal of Hydrology*, 249(1-4): 11-29.

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