Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-627-RC1, 2017

Hydrology and Earth System Sciences Discussions
Open Access

Interactive comment on "A curve number approach to formulate hydrological response units within distributed hydrological modelling" *by* Eleni Savvidou et al.

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Received and published: 4 January 2017

Review Savvidou et al., A curve number approach. . .

The manuscript describes a modified curve number (CN) used to delineate hydrological response units for hydrological modeling.

The topic is of interest for the hydrological community, because although a huge amount of literature exists on HRUs, how to best subdivide the catchment for hydrological modeling remains an open question.

I am not so convinced that the results of the study show the practical utility of the method. Therefore, I recommend expanding the case study (see detailed comments).

I also would not refer to the modified CN as a "new framework". The author propose a different definition of the CN, which as a result, becomes something different from the CN, and therefore can also be given a different name.

As the authors are considering the suitability of the CN in the optic of a specific application (i.e. the subdivision of catchment in HRUs for a specific hydrological model), it is unfair to speak about limitations of the CN, and of "improvements". The CN was not developed with the authors' application in mind.

Detailed comments

1. Abstract. I did not see evidence in the results that support the claims made in the abstract.

1a. The authors state that the new approach aims at reducing the subjectivity introduced by the definition of HRUs. Yet, the paper does not provide clear support on how to select the number of HRUs. The authors propose (line 7, page 11) that "the number of HRUs equals the number of discharge monitoring stations". This is however a general recommendation given without support, and cannot be considered a general conclusion of the study.

1b. The authors state that "the CN-based parameterization allows the user to assign as many parameters as can be supported by the available hydrological information". I don't see how this would be a distinctive outcome of the study. One can always get more HRUs by overlaying different properties such as geology, topography, soils, etc.

1c. "the CN-based parameterization reduces the effort for model calibration". Surely, this depends on how many HRUs are selected, and the number of HRUs can vary.

2. Introduction. I found the introduction quite imprecise in the terminology, and incomplete concerning the coverage of the relevant literature.

2a. For example, when talking about the differences between lumped to distributed models, several imprecise statements are made. E.g. that lumped models are simple (compared to what? To all distributed models? Not necessarily true), model parameters are associated with the macroscopic properties of the watershed (not necessarily true), lumped models have limited physical background (not necessarily true), model parameters need to be inferred by calibration (not necessarily true). With respect to distributed models, the authors state that the model domain is discretized in "finely resolved" computational units (not necessarily true), that in theory parameters are estimated from field data (not necessarily true), etc.

2b. Considering the focus on HRUs, I would have expected a broader coverage of the relevant literature, e.g. [Kampf and Burges, 2007], which talk about different discretization approaches, [Fenicia et al., 2016], which deal with the problem of how to discretize the catchment into HRUs, a discussion of alternative, more common approaches to define HRUs [e.g. Scherrer and Naef, 2003].

3. Scope of research.

The first paragraph gives a definition of the CN. It is redundant with what described in paragraph 4, and creates confusion, as before defining the modified CN approach one would need to recall the original CN. Anyway, this paragraph does not belong in a scope section.

Lines 6-11 page 5 are not scope, so they don't belong here. They could have been conclusions, but they are not, because they are not supported by the analysis.

4. The motivation for the modified CN is quite weak and buried in the text.

4a. Line 21 in page 7 states that an important shortcoming of the standard CN method is that "it does not take into account the effect of slope". The authors have obviously in mind a specific application, for which slope would be important. It would be useful to reveal this application to the reader in a clear way, before talking about limitations. And as stated in my general comments, it would be useful to state whether such application motivated the development of the CN, otherwise, it is unfair to talk about limitations of the CN tout court.

4b. Line 24 in the same page states that "steep slopes cause reduction of initial abstractions, decrease of infiltration, and reduction of the recession time of overland flow". I am not sure this is true in general. For example, on sandy hillslopes, there is no lateral flow. Such statements need to be supported with some references and need to be given a perspective.

5. section 4.2 is titled "Novel GIS-based framework for CN estimation". It is unnecessary to write "novel". It goes by itself that if you write a paper, you present something novel, otherwise there is no point in writing a paper. In addition, it is unnecessary to write "framework" for something that is simply a different formula for the CN. Probably, it is also incorrect to write CN, as being a different formulation, it does not need to be called CN anymore.

6. The case study should be considerably expanded.

6a. Are the discharge stations producing different hydrograph? Can this be shown through some signatures?

6b. Is the model comparison meaningful? For example, it would be useful to see how the different HRU discretization compare to a "null hypothesis" where 1 HRU is considered, particularly since results seem to favor the discretization in the smallest number of HRUs.

6c. What is the value of the objective function? Do more complex models perform better in calibration? It should be so, otherwise there may be a problem in the calibration process, also given the huge number of parameters for the models with large number of HRUs.

6d. Are the models able to capture the spatial variability in observed responses? E.g. are they able to reproduce the different signatures?

7. In the summary and conclusion section, I had difficulty in identifying the conclusions. It would be better to skip the summary (anyway the abstract is already a summary), and

just focus clearly on the conclusions.

References

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