Hydrology and Earth System Sciences Discussions

Interactive comment on "Land classification based on hydrological landscape units" *by* S. Gharari et al.

I. Nalbantis (Referee)

nalbant@central.ntua.gr

General comments

1. The authors present a methodology for classifying landscapes on the basis of dominant runoff generating mechanisms. Different indicators for classification are used which are exclusively based on topography. Results are compared with field observations and the well-known topographical wetness index. A headwater catchment in Luxembourg was chosen for testing purposes.

2. The paper is of significant scientific value since it provides useful information for improving hydrological models, mainly distributed ones. It concerns models of the top-

down category which can easily incorporate various kinds of information other than conventional ones (precipitation and streamflow). A proper framework for exploiting results of the paper has already been set by one of the authors (Savenije, 2010). The basic idea lies in partitioning the studied basin into areas or units that are homogeneous from the viewpoint of the dominant runoff mechanism. This can potentially allow for parsimonious and, at the same time, effective modelling. On the contrary, if one avoids differentiating model structure for various runoff mechanisms two options appear: (1) construct a very complex model which will encompass all runoff mechanisms but will break the principle of parsimony, or (2) achieve parsimony through simplifying modelling of some hydrological processes. In the latter case the model may become simplistic for some processes and too detailed for others, which corresponds to monomeric modelling as this is defined and discussed by Nalbantis et al. (2011). Those authors illustrated the problems that a bottom-up monomeric approach may encounter. The current paper can contribute to avoiding problems of the above two modelling options.

3. The existing body of knowledge on the role of topography in hydrological modelling is still inadequate to allow for substantial improvement of hydrological models. This further enhances the significance of the work presented.

4. The paper is well-written and the quality of English is good.

5. The paper is in general well structured. Yet, the large number of tests appearing in sections 3 and 4 makes it difficult to read. To improve readability it is suggested to exhaustively describe all tests within section 3 in the order that these appear in section 4, and leave results in section 4 without repeating any features of the experiments. In my view, rigorous one-to-one correspondence of the test description details and the results will help readers.

6. Some specific comments and corrections are listed in the section below; technical corrections are listed in a separate section.

Specific comments

1. Page 4382, line 7: Better say "easily obtained" instead of "readily obtained", since some non-conventional computations are required, e.g., for HAND.

2. Page 4382, line 10: The term "sample size" is confusing. What is a "sample"? Is it the dominant hydrological regime at certain points in space (field observations), or the spacing used in the digital elevation model?

3. Page 4382, lines 11-12: The phrase "The results obtained compare well with field observations and the topographical wetness index." gives the impression that "field observations" and "the topographical wetness index" form two kinds of information both of which are independent from model results; yet, in reality results include information from field observations since the latter are used in model calibration. Conversely, the topographical wetness index is not used in model calibration and can thus constitute legitimate information for validation purposes. The authors are invited to stress the above both in the abstract (in an epigrammatic way) and later in the text.

4. Page 4383, line 3: By "infer dominant runoff process" do the authors mean "infer the dominant runoff process", or "infer dominant runoff processes"? Please consider clarifying this.

5. Page 4383, line 6: I would suggest avoiding too general statements such as "for meaningful development" and be more specific, e.g., by saying "for enhancing physical meaning of model parameters".

6. Page 4383, lines 9-10: I would suggest saying "different topographical features are perceived to be linked to distinct hydrological functioning".

7. Page 4383, lines 11-12: The phrase "a catchment model ... flow generation processes." raises the question of the utility of hydrological units in hydrological modelling; please add a brief comment on this.

8. Page 4383, line 17: The phrase "their relation to various catchments" is confusing; please rephrase to clarify.

9. Page 4384, lines 23-24: The phrase "the definition of the threshold for channel initiation when deriving streams from a DEM, the seasonal fluctuations of the channel starting points" induces some doubt with regard to the difference between the terms "channel initiation" and "channel starting points"; I would suggest keeping only one of the two terms so as to eliminate doubt.

10. Page 4384, lines 26-27: Here the ambiguous term "sample size" used in the abstract becomes clear. As suggested in Specific Comment 2, it is necessary that the explanation appear earlier.

11. Page 4385, line 1: Since the paper is centred on the concept of "hydrological landscape units" I would not avoid commenting on the difference from the work of other researchers who used the same term based on another definition or philosophy (e.g., Park and van de Giesen, 2004, or Lin and Zhou, 2008).

12. Page 4385, line 4: The wide hydrological audience may not know what "effective smoothing window sizes" mean; please provide an explanation or reference.

13. Page 4385, lines 7-8: Is the topographical wetness index the only result that is linked to the specific test basin? I believe that results from all tests 1, 2 and 3 depend to a certain degree on the specific test basin. So, adding "in a mesoscale catchment in a temperate climate." would further clarify what the outcome of this work is.

14. Page 4385, lines 9 to 11: The use of terms "site", "area" and "catchment" induces some confusion; in my view, using "catchment" in all instances would be sufficient.

15. Page 4385, line 24: The phrase "...distinguishes three hydrologically, ecologically and morphologically different landscape units" fails to accurately reflect the aim and scope of paper. First, the main classification criterion is hydrological, which allows for ignoring ecological and morphological criteria, at least at a first level. The differences in the origin of the terms used for landscape classes constitute an issue which needs not to be related to this phrase. Second, the undefined term "units" naturally

implies contiguous areas within the studied basin, while such requirement is absent in the proposed methodology. So, a clarification is needed to avoid any confusion or misunderstanding.

16. Page 4386, line 6: The authors speak about "units" but, as mentioned in Specific Comment 14, this term is not that clear. Here, using "landscape categories" would be more accurate.

17. Page 4387, line 2: A brief comment is required to stress that saturation overland flow is expected also in climatic zones where extremely high rainfall intensities may lead to saturation from above.

18. Page 4387, line 5: After "mainly fulfil storage" I would suggest adding "(both soil and surface)" to specify the kind of storage.

19. Page 4387, line 8: Information is required regarding the method and raw data used for producing the DEM together with an indication on the uncertainty in the horizontal and vertical direction.

20. P 4387, line 26: A few words are needed with regard to the general plan of the campaign (e.g., full coverage of the basin or selection of representative areas, or personnel requirements).

21. Page 4388, line 11: I suggest modifying "The transition from one landscape to another" into "The transition from one landscape category to another" which is, in my view, more accurate.

22. Page 4388, line 12: In the phrase "These reflect" the word "These" is ambiguous; to what does this refer?

23. Page 4388, line 18, Eq. 1: Avoiding using classical symbols for the cumulative distribution function such as $F(x)$ inevitably induces some difficulty to readers.

24. Page 4389: Using X or Y to denote probability is likely to lead to confusing proba-

bility with random variables; it is suggested to change these symbols.

25. Page 4388, line 24: To help readers I would tend to suggest: (1) defining two generalised topographical variables (e.g., X subscript (1) and X subscript (2)); (2) using these variables in classification rules; and (3) substantiating the generalized variables later, when presenting the actual topographical variables that are used in models.

26. Page 4390, line 7: I think that the word "homogenize" does not reflect what the authors actually do; I suggest changing it into "normalize".

27. Page 4390, line 11, Eq. 11: What does the new variable HD* mean? Is it an indicator of the "total flow path length"? Naming this would greatly help readers.

28. Page 4391, line 13: What is "functionality" here?

29. Page 4391, lines 14-16: The phrase "The objective matrix is designed in a way that the number of sample points in each class does not affect the calibration process" is ambiguous; please be more specific.

30. Page 4391, line 25: I think that saying "within predefined ranges" would be sufficient.

31. Page 4392, lines 8-9: The statement "The 5m×5m resolution of the DEM allowed a relatively accurate representation of landscape units." holds only in the case of a high quality DEM, since the effect of DEM resolution has not yet been discussed. This should be clarified.

32. Page 4392, line 18: The example "i.e. $2\times3\sigma$SM," is confusing; what do the indicated numbers represent? Please consider explaining or removing the example.

33. Page 4393, line 1: I suggest changing the title of sub-section 3.6 into "Sensitivity to calibration point sample size".

34. Page 4393, line 24: I suggest changing the title of sub-section 3.7 into "Sensitivity to calibration point location".

35. Page 4394, line 11: Adding a sub-section numbered 3.8 and titled "Other sensitivity tests" (or similar) is necessary, which will describe all tests that are related to results (section 4) but are introduced in section 4 for the first time. For example, this may encompass material from sub-section 4.6.

36. Page 4394, line 20: I think that by "S = 0.129" the authors denote the value of parameter "μS". The same holds for H. For other models the same inconsistency is observed. Please explain.

37. Page 4398, lines 5-6: What is "head water convergences"?

38. Page 4398, line 6: The term "bias" is used in a somewhat loose manner; I think that a term such as "error" would be more suitable.

39. Page 4398, lines 13-15: The phrase "This phenomenon ... separation" fails to fully clarify how the authors treated cells with multiple observed classes.

40. Page 4399, line 20: The statement "For very robust models only small deviations would be expected." is not informative since reducing the size of the calibration data set naturally leads to loosing model capacity for generalization. So, it is the rate of such loss that is of interest here and not the model behaviour in general. Although this becomes clear later I would suggest stressing it in this phrase also.

41. Page 4400, lines 2-3: The phrase "fewer parameter combinations were able to produce equally good model results" causes some confusion. I think that a phrase such as "fewer parameter combinations were found to correspond to models with performance above a certain threshold" or similar would be much more accurate.

42. Page 4400, line 21: The verb "constrain" induces ambiguity; what was effectively done?

43. Page 4400, lines 23-24: The phrase "The results of the four analyzed sub-sets of calibration points taken from four parts of the catchment have been compared for MSHopt." is a typical example of material that should be found in section 3 (see also

C1973

general comment 6).

44. Page 4401, line 6: The justification through the phrase "due to the limited variability in slope angles." is probably not sufficient since HAND plays also a significant role. Perhaps adding a phrase such as "which reduces a two-criterion classification to a single-criterion one" would clarify things to some extent.

45. Page 4401, lines 8-9 and 10-11: The authors are invited to verify qualifiers in "the northern and eastern parts" and "like southern and eastern parts", since readers would normally expect to find the word "western" in place of one of the two instances of "eastern".

46. Page 4403, line 16: I presume that by the phrase "it is expected that most of the cells ..." the authors meant "it is expected that the wetness of most of the cells ...", 47. Page 4403, line 23: What do the authors mean by "classified map with aerial picture"? Is this a map with the same four classes (plateau, hillslope, flat wetland, sloped wetland) but obtained through using other sources of information such as photogrammetric products?

48. Page 4404, line 4: I think that the term "parameters" is confusing here; I suggest replacing it with one of the following terms: "topographical variables", "topography-based variables" or "topography-based measures".

49. Page 4404, line 14: I suggest changing "the overall landscape class" into "the overall landscape classification".

50. Page 4405, line 2: Since runoff generation mechanisms were not quantitatively identified in the test basin, I would suggest saying "presumably associated" instead of "associated".

51. Page 4425, line 3 of caption of Fig. 13: The phrase "The location of identical points are indicated by a star, triangle and square." is incomprehensible; what do the authors mean by "identical points"?

Technical corrections

1. Page 4388, line 18, Eq. 1: Please modify "efr" into "erf".

2. Page 4388, line 18, Eq. 1: Please avoid using "×" for algebraic expressions.

3. Page 4388, line 19: Please avoid capitalizing the first letter in "Where".

4. Page 4389, line 14: Please avoid capitalizing the first letter in "Where".

5. Page 4391, line 5: Please avoid capitalizing the first letter in "Where".

6. Page 4391, line 7: Better avoid starting the sentence with a symbol such as "NH".

7. Page 4394, line 5: I presume that "Eastern parts" should be "Eastern part".

8. Page 4395, line 3: Please modify "are to be classed" into "are to be classified".

9. Page 4396, line 1: Please consider changing "were several areas" into "where several areas".

10. Page 4396, line 27: I can presume that by "rang" the authors meant "range".

11. Page 4400, lines 19-20: Expressions with five qualifiers of a noun such as "higher, landscape classification relevant information content" lead to reducing readability.

12. Page 4401, lines 22-23: Please change "were classification" into "where classification".

13. Page 4418, Fig. 6: Too small characters.

14. Page 4418, Fig. 6c,d: Please change "Rang" into "Range".

15. Page 4421, Fig. 9: The same corrections as in Fig. 6.

16. Page 4419, Fig. 7: Here and in figures 10 and 13 I would suggest enhancing the contrast between colours for flat and sloped wetlands.

17. Page 4425, line 2 of caption of Fig. 13: Change "(a) categorized landscapes" into

"(c) categorized landscapes".

References

Gharari, S., Fenicia, F., Hrachowitz, M., and Savenije, H. H. G.: Land classification based on hydrological landscape units, Hydrol. Earth Syst. Sci. Discuss., 8, 4381–4425, 2011.

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