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Discussions

Interactive comment on "From runoff to rainfall: inverse rainfall–runoff modelling in a high temporal resolution" *by* M. Herrnegger et al.

Anonymous Referee #1

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The article proposes a method to inverse the rainfall-runoff relationship at the catchment scale to estimate precipitation from runoff. This is an interesting study and the approach may be useful in areas where the estimation of precipitation is difficult, e.g. due to sparse raingauge networks.

I think the article could make a valuable contribution to HESS. However, I have a number of concerns: - the general organization of the paper could be improved, especially the results section

- some explanations and justifications are sometimes too short to fully understand the choices made by the authors in the study,

- the validation of the approach should be strengthened, using other test catchments,

- there are too many illustrations (24 in total), not all of them appear necessary.

Given these limitations (and other aspects detailed below), I think major revision is needed before the article can be reconsidered for publication.

Detailed comments:

1. P13263,L6: The hypothesis of a closed catchment is quite strong, since there are many catchments where underground water exchanges with deep aquifers or surrounding basins are significant. One implication is that this underground exchange term is neglected later in the study (e.g. in the water balance equation). Does it mean that the approach would not be suitable for basins where there are such underground water losses/gains? This should be further discussed somewhere in the article as a possible limitation of the proposed approach.

2. P13263,L24-26: I found this sentence unclear. Could the authors further explain why this approach has limitations that justify the introduction of the new method they purpose?

3. P13265,L19: The equation may give the impression that time steps are considered independently in the method. However, S(t-1) is a function of antecedent rainfalls R(t-1), R(t-2), etc. Maybe this should be stated more clearly.

4. P13266,L6: How the upper bound of 50 mm/h was chosen. Is that a general value that can be applied everywhere or is it specific to the study catchment?

5. P13266,L12-14: Could the authors detail this a bit more? Which disadvantages are they?

6. P13269,L3-9: I found the choice of the study catchment quite strange. The authors mention at the end of section 2.2 that the method cannot be applied in snow influenced catchment. . . and then they select a test catchment heavily influenced by snow! Consequently, the test of their method can only be done on a short part of the test period where there is no snow influence, which limits the depth of the evaluation of the pro-

posed method (e.g. can it handle seasonal variations in precipitation?). I found this choice unfortunate. Besides, I found that testing the method on a single case study is also limited, since it does not give any information on the transposability of the method elsewhere. Therefore, I think it would be useful to have at least two study catchments with contrasted climate (and possibly hydrological) characteristics and without snow influence to provide a more comprehensive evaluation. If the authors have a specific interest in the Krems catchment, then it could be kept as an additional case study to show how the method can partly be applied in snow influenced catchments.

7. P13270,L4-12: The authors should explain why such a short period is sufficient to test the efficiency of the method, given the known variability of hydroclimatic conditions.

8. Section 4: I found this section not well organized. It mixes the presentation of testing methods, criteria and results. I think that all methodological aspects should be presented before the results section, to provide a clearer overview of the testing approach, and then the results section should only detail and discuss the results.

9. P13272,L11-13: I found this is not so clear for the year 2007.

10. P13273,L17: R2 is known to be very sensitive to outlier data. Therefore is it really a well-chosen criterion? MSE is used later. Why these two criteria are necessary?

11. P13273,L19-22: How can this be interpreted? Can the low-pass filter role (smoothing effect) of the catchment be partly responsible for this?

12. P13273,L27 to P13274,L8: I found this part not really essential.

13. P13275: What can be learnt from this?

14. P13279,L29: In real time flood forecasting, the target is the future flow, not the past rainfall. So I did not understand why the application of the method would be useful for this application.

15. P13281,L10-11: I did not understand this sentence.

16. Table 1: What is the source of these values? Any reference?

17. Table 3: Should fluxes not be expressed as depth per time step?

18. Figure 1: BWI should appear in the first store

19. Figure 2: This is quite classical. Is it really useful here?

20. Figures 4 and 5: Could these two figures not be merged?

21. Figure 7. Remind in the caption that only a few months are used each year.

22. Figure 8: It is always nice to have hydrographs shown, but I found the added value of this figure is rather limited.

23. Figure A1: Is this figure useful, given it is not really commented in the text?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 13259, 2014.