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Hydrology and Earth System Sciences Discussions

Interactive comment on "Anomaly in the rainfall-runoff behaviour of the Meuse catchment. Climate, land use, or land use management?" *by* F. Fenicia et al.

F. Fenicia et al.

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We thank the reviewer for his critical comments on our work, which we shall take into account in the preparation of the revised version of this manuscript. In the following reply, we shall express our opinion on the reviewer's criticisms.

Reviewer comment: The authors show that the model can only predict the observations when some model parameters are varied in time. From this some conclusions can be drawn. The authors imply that these results prove that the anomaly is due to land use management. However, land use management effects are not explicitly

taken into account in the model. The authors term this a fully top-down approach. Hence, the conclusions remain speculative and it would be better to address this as a hypothesis. Given the fact that the anomaly consists of a different behavior of the catchment with respect to generation of runoff in the period 1930-1960 (less runoff for the same amount of rain), that the climate did not change and that the overall land use remained the same, one could obviously attribute this to a difference in land use management (more evaporation because of forest rotation) without running a model. Hence, this remains speculative as nothing is really proven with or without a model.

Authors reply: This point represents the main criticism to our paper, and it has been mentioned also by other reviewers. We agree with the reviewer, that our work does not prove that land use management has had a major control on the hydrologic behaviour of the Meuse catchment, and we also agree that our hypothesis remains a hypothesis also after the outcomes of our work. We mention this in our paper (page 1803 lines 23 - 26) but we shall emphasize it even further in the revised version. However, if our work does not provide conclusive evidence that confirms our hypothesis, we believe that the approach taken reveals some interesting aspects of catchment behaviour. Our approach would provide a meaningful answer under the assumptions that (i) the data are correct, (ii) the model structure is realistic, and (iii) the model parameters are physically meaningful and identifiable from the data. If these conditions are met, the method that we have chosen would help to identify variations in model parameters that could then be related to variations in catchment characteristics. Although we may have confidence in our assumptions, we cannot prove that they are correct. Hence, it is necessary to dispose of additional sources of information to support our hypothesis. These sources of information were not directly available, but we can use information from historic records that indicates that wood production has changed considerably during the last decades.

The use of a model combined with the evaluation approach that we have selected

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has helped to identify trends in the model parameters, that could then be associated to changes of catchment properties (a clear example is the lag time of the system, which decreased substantially). Moreover, it has helped to understand whether the nature of the anomaly in the water balance, which predictably could be resolved by varying model parameters, could be explained by varying parameters within physically admissible limits.

Reviewer comment: the title does not reflect the real contents of the work, because climate, land use, or land use management are not explicitly taken into account

Previous studies on this topic have focused on climate change, and on land use change. Our suggestion is to also consider the way land was managed. We do not consider climate change explicitly, because we assume that model parameters are not climate dependent, which implies that the model should be able to cope with change in external forcing. With respect to land use change, we remain with the same assumptions postulated by Ashagrie et al., (2006), so as to make our model results comparable. The effect of land management is taken into consideration conceptually, increasing model complexity through the inclusion of specific parameters. The title reflects our aim to put this work in the perspective of what has been done before, using essentially the same data and the same type of information. We add an additional perspective to the problem (the hypothesis that land use management may have contributed significantly to the rainfall runoff behaviour of the catchment). The question mark indicates that we do not have a conclusive answer on this problem, which is perhaps impossible to give with the data that we have used.

Reviewer comment: technical suggestions on how to improve abstract and introduction

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We thank the referee for the suggestions, which we shall take into account in the revision.

Reviewer comment: Study area and data description: Page 1794, line 15: the location of De Bilt should be indicated on the map given in figure 1. This is one of the very weak points of the study. The Bilt is way out of the catchment, especially the part upstream of Borgharen. As stated in line 9, the catchment has three distinct zones. Hence, I am very doubtful that the meteorological data of De Bilt is representative for the study area. Page 1795, line 4-6: real prove or references should be given that the data of de Bilt provide an acceptable agreement.

Authors answer: We want to stress that the data from de Bilt are used in the estimate of the potential evaporation, while the rainfall is available from different stations within the catchment. For this study we used the same forcing as in Ashagrie et al. (2005), who have selected de Bilt because its records covered the entire period of analysis. Other stations within the catchment could only cover most recent years. However, we shall integrate the paper with a comparison of these data with other stations available.

Reviewer comments: Model description: Pages 1797 and 1798: the description of the model is not very clear, especially the five land use types that are considered and their parameters. For instance, interception is simulated differently for each land use, but is this so for other parameters like the shape parameter 946;? What is the time step of the model? How did the authors deal with the difference in deciduous and coniferous forest if overall land use is constant but in the Meuse basin deciduous forest was gradually replaced by coniferous forest and their interception capacity is different in winter. Were different values of the stomatal resistance taken into account for deciduous and coniferous forest? Page 1797, line 22: why was scaling parameter Ci applied to all land use types. It would have been more appropriate to use this

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only for forest, because a significant difference in interception related to stand age is expected as indicated in the literature. Maybe the authors also expected a large difference in interception due to urbanisation? Possibly the mixing of all land use types neutralised the effect of this parameter. Why not use 2 scaling parameters for interception, one for build up area and one for forest? Page 1797, lines 23 and 24: for pasture interception can vary, but in Table 1 the min and max values are the same?

Author comments: we thank the reviewer for evidencing the limitations of the model description. We here answer his comments but we shall make these points more clear in the revised version of the paper. Interception is simulated differently for different land uses, while other parameters are uniform over the catchment area. The time step of the model is daily. The fractions of coniferous and deciduous forest have been considered continuously varying. Interception is calculated separately for different land uses. Potential evaporation is also calculated separately for different land uses. The evaporation amounts calculated for different land uses are then weighted according to the time varying area of the catchment, and the model proceeds in a lumped mode.

The function of the scaling parameter is to allow the interception threshold to vary in the calibration process, while maintaining the proportions given to different land uses constant. We avoided to give different scaling parameters to different land uses to avoid unrealistic parameter combinations (e.g. interception of urban area higher than that of forest). Page 1797, lines 23 and 24: the text was wrong and we shall correct it. We assumed a constant interception threshold for pasture.

Model evaluation: The time period of the simulation is not clearly stated.

The time period of the simulation is from year 1911 to year 2000

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Results: Page 1801, lines 25 to 27: this outlier proves that the values of the model parameters are sensitive to other effects as for instance the amount rainfall. Hence, the conclusion that the changes in model parameters can be attributed to changes in forest stand should be taken with precaution.

Authors answer: point taken. Indeed this is a possibility, and we shall stress this more clearly in the revised version of the paper.

Discussion: The most clear result of the model is that there is a significant change in the concentration time in the basin (actually this is already clear from the data because floods have become more severe). This can be clearly attributed to land use change (urbanization, drainage). Probably this also will have a strong effect on evapo-transpiration which is not considered in the model. The authors assume that changes in parameter alfa and parameter Nlagf are unrelated and are respectively due to land use change and changed land use management. However, urbanisation and drainage have also important effect on evapotranspiration.

Authors answer: In the model the parameters are uncorrelated because they serve different functions. However, the reviewer is right in saying that in nature the processes may be correlated. Our model follows the standard of the HBV model, where evaporation is assumed to take place from the unsaturated soil reservoir and not from the cascading reservoirs.

Conclusion: The conclusion should be more modest in line with the real results of the work. Page 1804, line 17: "the purpose of ... century" this was not really done in this study.

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Interactive Comment

Authors answer: Point taken, we shall be more modest in our conclusions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1787, 2008.

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Interactive Comment