Review: Gal et al.

The paper focuses on a very important topic of runoff generation processes and their changes through time and in identifying the main causes of such changes in an area of sparse data. The objectives and the general approach that was taken in the study to achieve these objectives are scientifically sound and a good and insightful data analysis is presented.

I have however main concerns of the modeling strategy, assumptions and application. They include: a large gap between the model complexity and the data used for its application; sensitivity analyses are essential to justify many of the modeling decisions made, such as which model parameters to calibrate (leaving out probably the most sensitive parameters); the uniform rainfall assumption is very problematic and should be justified; and, model calibration was made against a single number of mean annual runoff volume (although annual volumes estimation do available). See more details below:

1) The authors rightly present the need of high temporal resolution rainfall and apply a disaggregation procedure. However, in space they assume a uniform distribution of rainfall over the catchment. This assumption is very problematic. Even if the storm cell is more or less at the same size of the basin or somewhat larger, the cell location in space will be often thus that only a partial coverage is achieved. Given the very high sensitivity of runoff to the partial coverage the exclusion of this factor from analysis might add a large uncertainty. Sensitivity analysis of this of catchment runoff to partial storm coverage should at least be examined.

2) The change in the channel network density between the two periods was presented by a change in planes aspect ratio rather than the CSA elements. Altering the aspect ratio generates a more or a less elongated catchment shape but the drainage density is changed only a little. Why not to utilize the derived channel network maps and identify for each period its own CSA configuration?

3) Kineros2 has many parameters. The authors have chosen to calibrate the channel Ks and Manning coefficient, while the other parameters were determined using from data and using different functions. This is a very problematic decision – why these parameters were selected for calibration? What do we know about the accuracy of the other model parameters that are not calibrated? There are two necessary steps that are essential to justify the authors decision: 1) sensitivity analysis that will show the parameters that are most important for calibration (i.e., that model output most sensitive to them), 2) for the pre-defined parameters, assess their uncertainty and examine how this is translated into small uncertainty in model output.

4) I believe that total runoff is much more sensitive parameters associated with the infiltration process in the plans, e.g., to plans Ks rather than to channel Manning coefficient and probably to channel Ks. The decision to calibrate the two channel parameters, MAN and Ks is not clear and must be supported.

5) Furthermore, a main impact on annual runoff was found to be the modification of soil properties and vegetation cover, but the model parameters associated with the hydraulic properties of these units were not determined in such a way we have a high certainty in those parameters. Obviously, modeled runoff is very sensitive to these parameters, but they were not calibrated or even examined for their sensitivity.

6) The calibration strategy seems to me not appropriate. The authors use the bias of the annual runoff as the objective function for calibration; however, Bias does not account for the year by year variations

but integrates all year data into a single value, so possibly a large overestimation of modeled runoff in one year can be compensated by a large underestimation in another year. Instead, an objective function that accounts for the yearly residuals, such as the most popular RMSD objective function is much preferred. It should be emphasized that using the Bias ignores the annual runoff values estimated in the authors previous work and just uses their integration.

7) Furthermore, a calibration strategy that is based on Bias of runoff volumes, implies that a single number is used for calibration (one data!). This seems not reasonable given the very complex model used and the hard work done to produce very high resolution data.

8) The authors utilize a very detailed and high resolution hydrological model (which I am not sure is the most appropriate given the very limited data they have), but they do not really take advantage of the detailed simulations. For example, they could try to understand why the change of soil and vegetation properties increased runoff, for which type of rain events it is more pronounced? Are the change manifested in higher peak discharge or in more streamflow events, etc.

9) As rainfall is so highly variable, conclusions about the effect of its possible change should be done with a caution. For example, the authors state that “The results show that changes in daily precipitation regime do not explain runoff changes between the past and the present.” (P. 15, L. 31), but even if such changes do occur in reality it is most likely that they are not statistically detectable due to the high natural variability.

Specific comments

1) A climatic description of the area is lacking: mean annual rainfall, potential/actual ET, etc.

2) Only one station is used for rainfall data (and few others are used for the temporal disaggregation); clearly a poor coverage of the catchment, as seen in Figure 1. Have the authors examined the option of remotely sensed precipitation? At least to examine the storm coverage area (which is assumed here to fully cover the catchment.

3) Assumption of soil recovers its initial conditions in two days – this assumption can be reasonable for arid regions. What about the deep soils in the south?

4) Add the “absolute value” sign to Eq. 3

5) The optimization of MAN and KS should be at a higher resolution in my opinion

6) Please clarify how did you identify “Isolated dunes (S1) are found at the same location for both periods, but have been eroded and partially encrusted” (P. 10).

7) Please represent the RMSE value also in percent from the mean (P. 10 L. 22).

8) I recommend to present runoff ratios for each year and to show an example of flood hydrograph.

9) As rainfall is so highly variable, conclusions about the effect of its possible change should be done with a caution. For example, the authors state that “The results show that changes in daily precipitation regime do not explain runoff changes between the past and the present.” (P. 15, L. 31), but even if such changes do occur in reality it is most likely that they are not statistically detectable due to the high natural variability.