RC#2

Review: Gal et al.

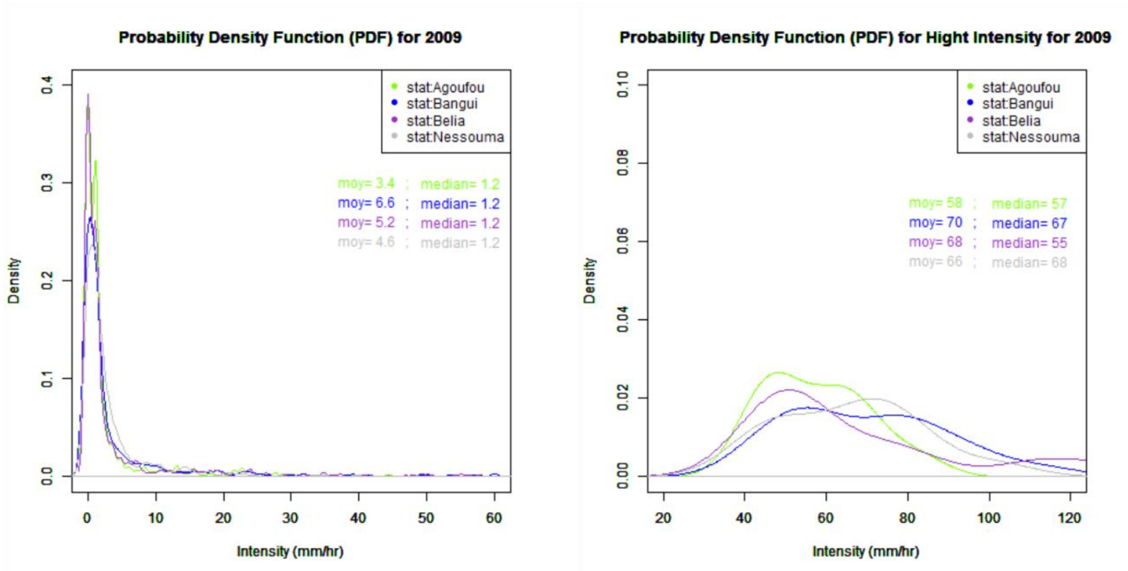
We thank reviewer 2 for reviewing our manuscript and providing valuable feedbacks. We have now addressed all of his/her comments and discuss them in the following.

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.

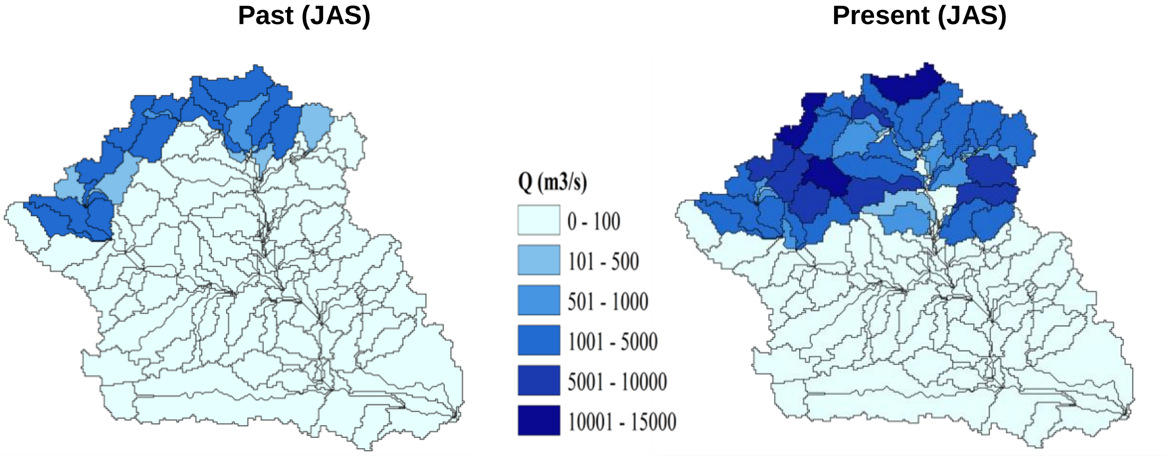
I have however main concerns of the modeling strategy, assumptions and application. They include: a large gap between the model complexity and the data used for its application; sensitivity analyses are essential to justify many of the modeling decisions made, such as which model parameters to calibrate (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.

This is an important point. The figure below shows an example of the rainfall PDF derived for different AMMA-CATCH stations for an average precipitation year (There are no others stations than those identified in Fig.1). Similar rainfall intensity distributions are observed for the different stations, especially for intense precipitation, which contributes to runoff.



To further investigate the question, we had also looked at the cloud top temperature derived by MSG remote sensing data, during this year. This analysis allowed us to conclude that the rainfall cells in the area are generally larger than the watershed area. In addition, the figure below (that will be added to the revised manuscript) shows that the contributing part is located to the north of the watershed. Therefore, only one third of the watershed is concerned.



The analysis of remote sensing data revealed an average time lag of 15 minutes between the east and west of the watershed (squall lines usually propagate westward in the Sahel). We agree with the reviewers that this could have a significant effect on the runoff. In our simulation setup, the impact of a time lag would only affect the timing of water flow entering channels, given that planes are too small (average 1ha) to be affected by the rainfall spatial variability. Thus, flows in channel would be impacted (in fact peak flow should be decreased and flow duration increased, leading to increased infiltration in channels). Such an effect however is compensated for by channel calibration (indeed this is also a reason to calibrate channel parameters). In addition, even if the absolute runoff values would be different in the case of heterogeneous rainfall, there is no reason that this would change the difference between the past and present case runoff, which is the main focus of the paper.

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?

It was probably not clear in the first manuscript, but changing the aspect ratio reproduces exactly the elongation of the channel network (since plane's width corresponds to channel length). Changing planes elements was considered also but there was a risk of changing plane properties in an uncontrolled way (changing planes is not straightforward in KINEROS2). That would have made more difficult interpreting changes between the past and present cases. The density factor estimated at 1.3 is not very important at the watershed scale, but when it is computed for the contributing sub-basins, it doubles between the past and the present.

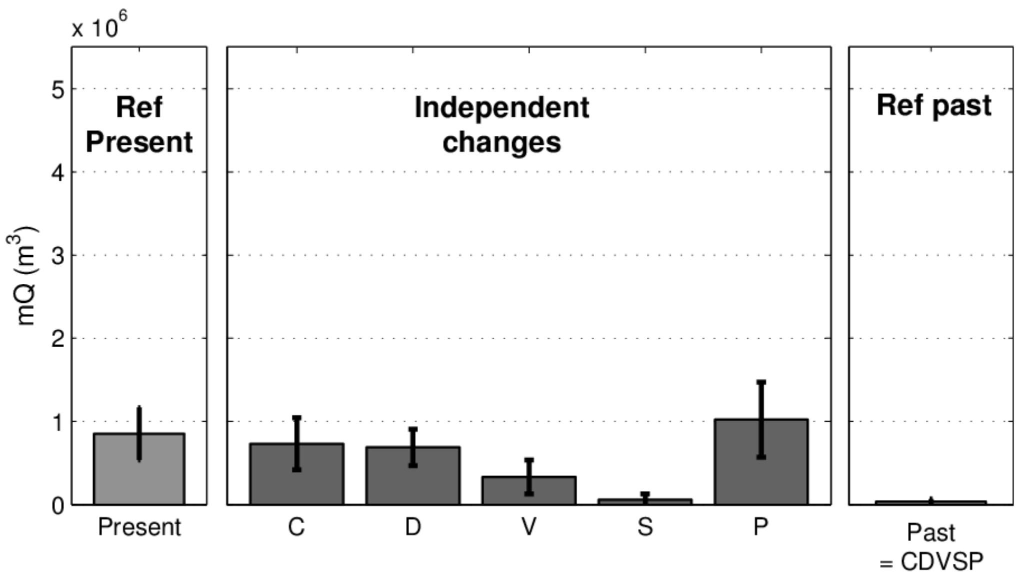
3) Kineros2 has many parameters. The authors have chosen to calibrate the channel Ks and Manning coefficient, while the other parameters were determined using from data and using different functions. This is a very problematic decision – why these parameters were selected for calibration? What do we 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.

Based on feedback from other reviewers and reviewer 2, it appears that the objectives of our study and the methodology we employed were not well explained. Therefore, we will better explain this in the revised manuscript.

The objective of this work is to estimate the impact of observed landscape changes on surface runoff. In that purpose, calibrating the plane parameters would enforce a strong constrain on the model, which it is susceptible to mask out the impact of differences between the past and present case (it is not possible to calibrate each type of plane separately). Our approach was therefore to prescribe the plane parameters from maps of observed landscape changes, using indications on texture (field survey, soil map) and FAO classification soil types. In doing so, we accept that some uncertainty comes from approximated Ks and MAN over planes, but we do not influence the differences between Past and Present.
We fully agree with reviewer 2 that parameters on planes are important in generating runoff. Our paper provides a ranking of the different changes that impacted runoff changes. We have performed a sensitivity analysis using significantly larger Ks (x2.5) and larger MAN (x1.75) for all planes, which shows that the absolute runoff does change but the ranking of the different scenario does not change, as it is shown by the following figures. This sensitivity test is based on data compiled by Casenave and Valentin (1989) for Sahelian soil, and represents the variability for different types of soils. Both parameters changes increase the runoff, so the total effect of changing both Ks and MAN that way is a rather strong test (as it can be seen on the total runoff), which provides some robustness to our ranking results.



Ranking and impact of the different changes observed over time, with all planes Ks multiplied by 2.5 and MAN multiplied by 1.75. The results of this sensitivity test mirror what is found in the article, with a total runoff divided by more than 3.

The calibration of channels parameter plays a minor role, ensuring only that the plane description results in simulated runoff which can match observations with plausible channel parameters. The resolution of the satellite and aerial photographs used to analyze the past and the present does not allow an identification of channel properties and their possible changes over time. The philosophy adopted for this paper was therefore to calibrate the less known parameters (channels) rather than the most sensitive ones. Another reason for calibrating channel properties is to account for the rainfall heterogeneity and time lags in the precipitation events over the watershed, as discussed above
With the default values of the parameters on the planes, we have obtained, via the calibration, values of MAN and Ks in the channels that are coherent with the literature.
We have explain the calibration approach in more details in the revised manuscript.

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.

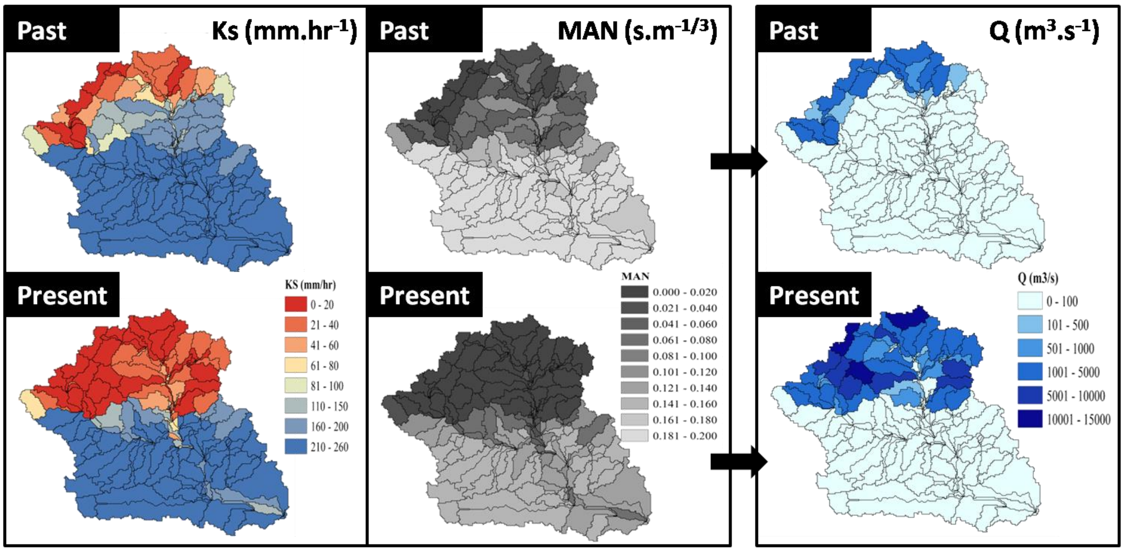
As explained above, calibration is not critical for the main conclusions of our paper. We fully agree with reviewer 2 that different criteria could have been used, and that RMSE could even ameliorate inter-annual variability. The RMSE values for the calibration simulations is specified in the results. We will specify this in the revised method section.

We have tested K2 sensitivity to our calibration approach by running the model using other channel parameters values (Ks = 40 mm.hr^{-1} and MAN = 0.02 s.m$^{1/3}$). This yields results that are similar to those obtained using the calibration results (For present period: 3.3.10^{6} m^{3} for Ks = 30 mm.hr^{-1} and MAN = 0.03 s.m$^{1/3}$ against 3.6.10^{6} m^{3} for Ks = 40 mm.hr^{-1} and MAN = 0.02 s.m$^{1/3}$). If the RMSE criterion is chosen, the sets Ks = 40 mm.hr^{-1} and MAN = 0.03 s.m$^{1/3}$ is obtained We fully agree with reviewer 2 that different criteria could have been used, and that RMSE could even ameliorate inter-annual variability but we chose to keep the notion of bias as referents. In addition, there is not only one bias data but one per calibration year (n = 5) and for ten rainfall event (n = 10).

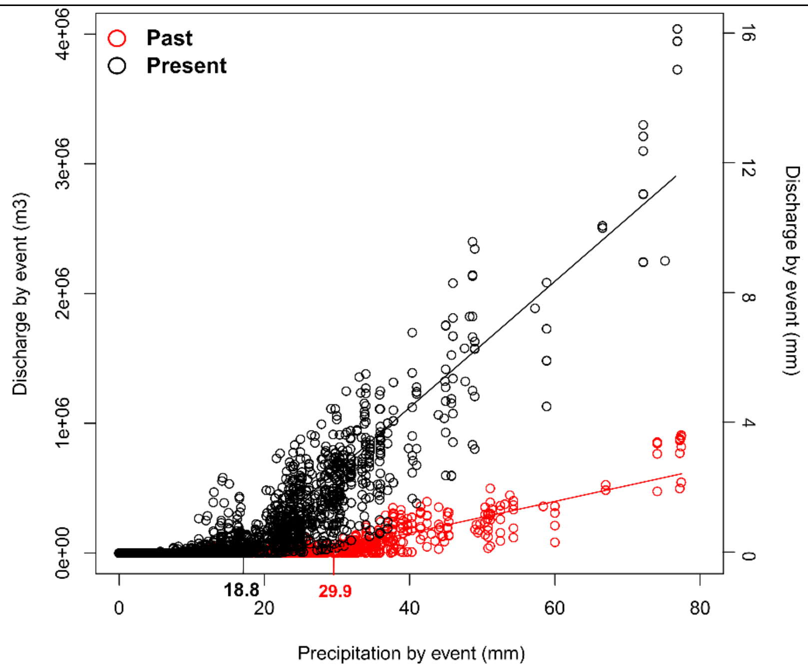
8) The authors utilize a very detailed and high resolution hydrological model (which I am not sure is the most appropriate given the very limited data they have), but they do not really take advantage of the detailed simulations. For example, they could try to understand why the change of soil and vegetation 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.

Thank you for the suggestion. We have added figures in the revised manuscript. Two of them show the spatial patterns of parameters and results and their changes over time, taking advantage of the distributed nature of the model. The other figure is a runoff/precipitation, which takes advantage of the event-based simulations.

The first figure shows the impact of the landscape changes between present and past on the Manning roughness coefficient and the saturated hydraulic conductivity for the whole watershed. These modifications have led to doubling the contributing part of the watershed.



The second figure represents discharge vs precipitation for all events in the 15 years period in the past and the present. Two main conclusions can be derived from this figure: 1) for the same precipitation intensity, we have twice as much discharge for the Present case. 2) For the present period, rainfall events of 18.8 mm on average, contribute to the discharge whereas in the past rain events of 30 mm are required.



As far as the model choice is concerned, a preliminary study based on a literature review, (not detailed here but found in Gal 2016, PhD thesis), was carried out on 20 models (global, distributed and semi-distributed) in order to choose the hydrological model best suited to the zone and the objectives of the study. KINEROS2 was found to be the well suited (more details on this can be found in the response to reviewer 1)

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.

We agree with that comment. We will rewrite this sentence, thanks for the suggestion.

Specific comments

Thank you for the specific comments and suggestions, they will be taken into account in the revised version of the manuscript

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

OK

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.

Yes, we have investigated this issue, which we agree is important. We have also looked at the cloud top temperature, derived by MSG remote sensing data, during this year. This analysis allowed us to conclude that the rainfall cells in the area are generally larger than the watershed area (and especially the contributing part, which is located in the northernmost part of the watershed).

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?

We have calculated the time required for the soil to return to an initial state (dry soil over the first few centimeters, which controls Hortonian runoff) using soil moisture data available via the AMMA-CATCH observatory described by de Rosnay et al. (2009). This time is rather short (48 h depending on soil type).This justified to reset the soil moisture to its initial condition after each event. In Sandy (deep) soils do not contribute to surface runoff.

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

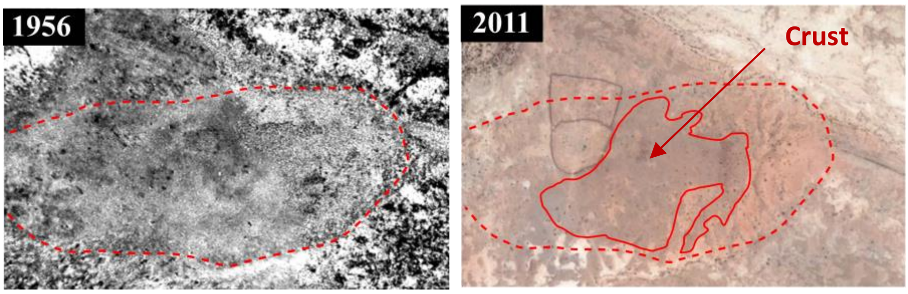
OK. Thank you.

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

We do not want to overemphasize the calibration of channels parameters, as it is detailed in response to comment 3, 4 and 5. In addition, assessment of the channels parameters is not fully automated and requires a large number of simulations and post-processing. This is why we choose to sample a reasonable interval with a limited number of parameter values. The accuracy obtained appears to be sufficient for our objectives. Indeed given to the compensation of MAN and Ks, different combination of these two parameters (in the neighborhood of the retained solution) give close values of runoff at the outlet. So we do not think that high precision would be meaningful.

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

The images below show that isolated dunes were partially crusted, modifying the hydrodynamic properties of the soil and the growth of the vegetation. This evolution was confirmed by field work (Pierre Hiernaux).



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

OK.

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

We have added rainfall/runoff for all events (as well as maps of runoff per plane), which brings information on how the watershed behaves in the Past and Present periods. The observed runoff coefficient have been presented and discussed in Gal et al. 2016, so that we would prefer not to duplicate.