RC#3

Interactive comment on "Modelling the paradoxical evolution of runoff in pastoral Sahel. The case of the Agoufou watershed, Mali", by L. Gal et al.

We thank reviewer 3 for reviewing our manuscript and providing valuable feedbacks. We have addressed all of his/her comments and discuss them in the following.

General comments

This manuscript presents a modelling exercise made for investigating the causes for the so- called 'Sahelian paradox' that consists of a runoff increase in the last decades after the catastrophical drought of the seventies, in spite of a decrease in annual precipitation. The subject is of interest for the HESS readers, uses a known rainfall-runoff and erosion model as well as rainfall input data derived from networks and new information on land cover, soil types and catchment runoff derived from remote sensing and photointerpretation. The paper is mostly well written and its overall formal quality is good.

Nevertheless, the manuscript handles the issues related to temporal and spatial scales as well as parameterisation in too simplistic ways for the results being sufficiently sound to 'explain' or to 'understand' the Sahelian paradox.
Using a 5-minute step event model designed for small agricultural catchments for simulating the annual discharges of a 250 Km^2 basin, assuming that the relationship between daily and 5-minute rainfall intensity did not change on time is not a conventional research approach.

The reasons why we use this kind of model with the data we have (daily precipitation, annual or infrequent runoff) were probably not well explained in the manuscript. We simulate the different hydrological processes at fine spatial and temporal scales. We believe it is necessary given that hydrological processes in the Sahel, as in some other semi-arid areas, are driven by rain events at the sub-hourly time scale (so high time resolution makes sense) and runoff is generated by shallow and impermeable soils occupying a small portion of the landscape (so high spatial resolution also makes sense) by Hortonian runoff. Even if the final objective is to investigate changes between the present and the past 15-year periods, we believe it is critical to use a model that can address this scales. Indeed results from a land surface model intercomparison (Grippa et al, in press in J. of HydroMeteorology, available as early release on line at http://journals.ametsoc.org/doi/pdf/10.1175/JHM-D-16-0170.1 and upon request to M. Grippa) have shown that global land surface models are unable to represent surface hydrology in this area.
In the literature, the size of watersheds studied with the KINEROS2 model varies widely according to the authors. Al-Qurashi et al. (2008) tested the model on a catchment whose area was 734 km² and obtained good results at the annual time scale.
Concerning the rain, we agree that assuming a distribution of rain at 5 minutes which is similar between the present and the past yields some uncertainty. Research on rainfall intensity is currently carried out (including studies by some colleagues of ours) to investigate that, but up to now, no study shows that 5-min intensity has changed between the Past and the Present period.

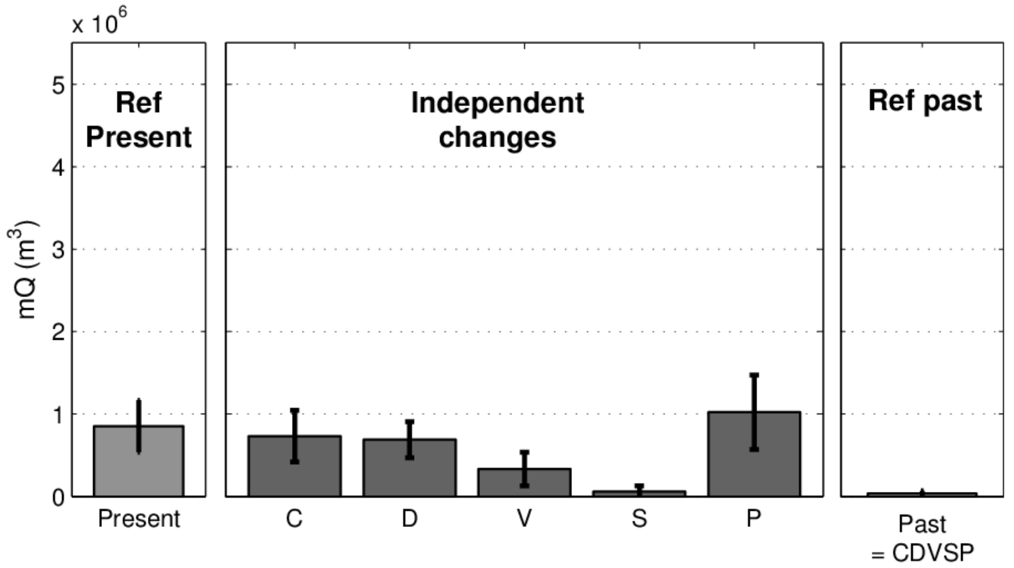
Al-Qurashi, A., McIntyre, N., Wheater, H., Unkrich, C., 2008. Application of the Kineros2 rainfall-runoff model to an arid catchment in Oman. J. Hydrol. 355, 91–105. doi:10.1016/j.jhydrol.2008.03.022

More essentially, as it is well known among the hydrological community that any hydrological model can give good results for the wrong reasons, when the purpose is not to obtain good results but to investigate the reasons, the researcher must be particularly cautious to take into account the likely equifinality of diverse possible parameterizations.

We agree with reviewer 3 statement. Indeed we have carefully selected the hydrological model to use in this study based on its capability of representing the hydrological processes that characterize the study region (model choice is explained in response to reviewer 1 and 2). In addition, we have only calibrated the parameters of the channels to check that the planes parameters produce reasonable runoff (= runoff that fit observation with plausible channels parameters), as explained in response to rev 1 and 2. The overall philosophy was not to calibrate and optimize the most important parameters (planes KS, MAN) and to constrain the model as less as possible. A sensitivity test which multiplies planes KS and MAN by 2.5 and 1.75 leads to the same ranking in terms of Past/Present changes, which provides robustness for our main results. Of course, further studies with different models would be interesting and the data are being put on the AMMA-CATCH database in that purpose.

The paper might be accepted for publication if both the model parameters and results were more investigated. The analysis of the contribution of the diverse factors to the change of the catchment response is a strength of the paper, but it assumes just a 'correct' parameterisation; an uncertainty analysis should be made or, at least, a sensitivity analysis of the model response to parameter variation.

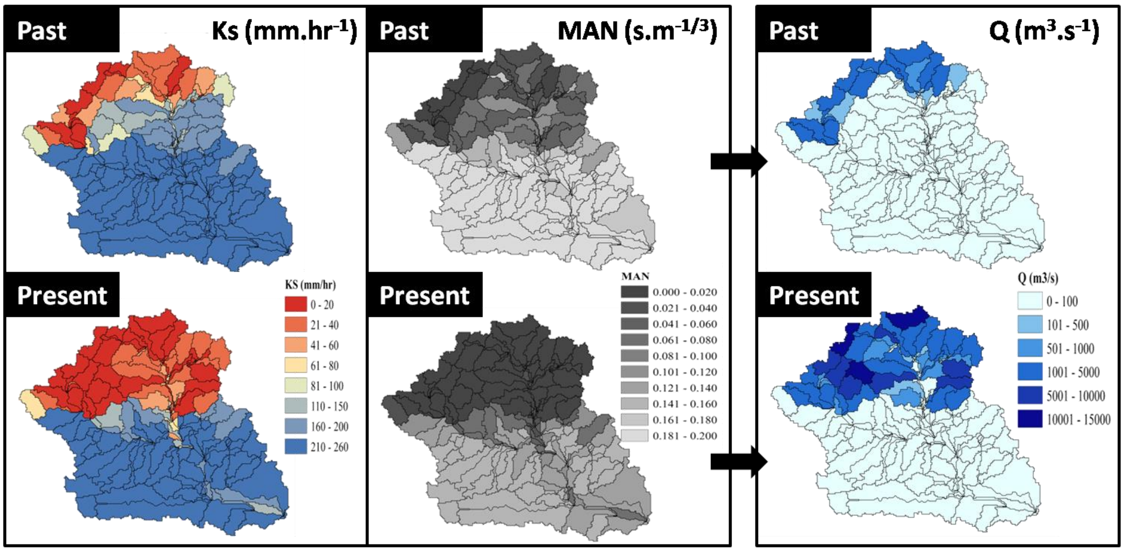
As said above, we have performed a sensitivity analysis using significantly larger Ks (x2.5) and larger MAN (x1.75) for all planes, which shows that the absolute runoff does change but the ranking of the different scenario does not change, as it is shown by the following figure. This sensitivity test is based on data compiled by Cazenave and Valentin (1989) for the Sahel and represents the variability for different types of soils. Both parameters changes decrease 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.



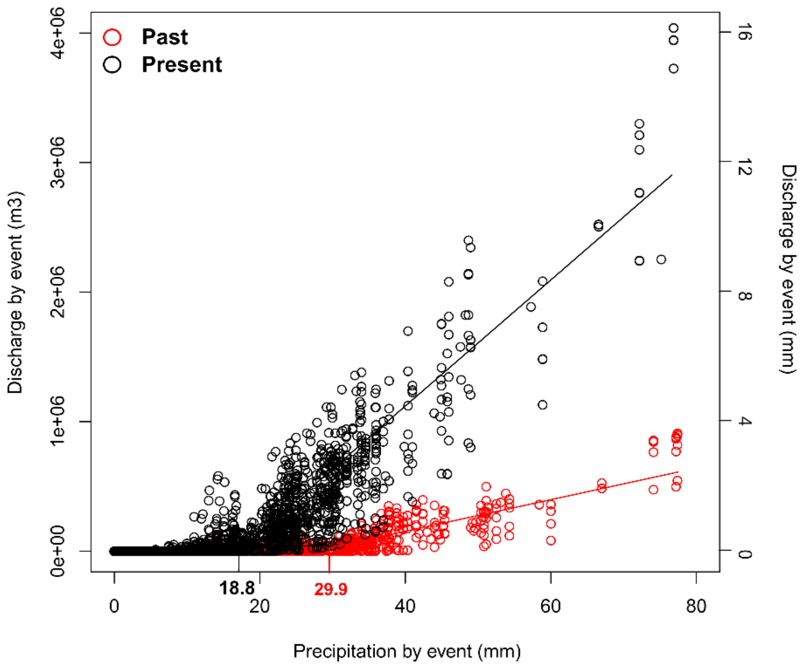
In the same vein, simulations performed with different values of Ks and MAN for the channels give similar results.

Annual catchment discharges should not be the unique focus of model output, but some model results at the event scale (extreme events, annual number of runoff producing events, rainfall threshold values...) and at the landscape unit scale (identification of contribution areas for diverse type of events...) should also be shown and discussed.

Following this suggestion, we have added 2 figures in the revised manuscript, and we agree that they provide very interesting information, thank you for this suggestion. 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 a doubling of 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 (intercepts of linear fits for non-zero discharges).



Yet, the authors must make a rigorous distinction between model and reality, simulations and observations, as well as more clearly separate drivers, processes and model parameters.

We agree, we will modify our writing in the new version of the manuscript, adding ‘according to the model’ and similar sentences.

Detailed comments:

Page 1, line 16: KINEROS is not a water balance model but a rainfall-excess runoff model. Water balance, which is the main challenge in the Sahelian paradox, is therefore indirectly simulated, as runoff water is subtracted from infiltration and subsequent evapotranspiration. This is a relevant aspect to be stated in order to assist the understanding of the paper by readers not familiar with KINEROS.

OK. Thanks for the suggestion.

Page 1, line 17: it is necessary to state the catchment area

OK. It will be done.

Page 1, line 23: "shallow soil being eroded and giving place to impervious soils" please rewrite in a less literary way

OK, will be done.

Page 1, line 24: the converse is more rigorous: "The KINEROS-2 model was parameterized in order to simulate these changes in combination or independently".

OK

Page 1, line 26: Catchment flows shown in volume (m^3) cannot be related to rainfall rate and this is not usual in the hydrological literature because volume depends on catchment area, it is more adequate to show them in runoff units (mm per year). Showing only the simulated results is not informative at all (are those simulated with KINEROS2?).

We will add the correspondence between volume in m3 and mm/year and use mm/year when possible as in the rainfall/discharge figure above. (From a water resource point of view volume in m3 is also informative).

Page 1, line 29: "Modification" refers to the action of changing something and should not be used for a natural change. This is unclear that vegetation cover was modified in the parameterisation. Please describe more precisely the modification of parameters made for simulating the landscape changes.

OK

Page 2, Line5: During or after the drought?

OK

Page 2, line 25: Reduction of vegetation cover and topsoil crusting are factors of too different nature to be cited together. Reduction of vegetation cover can (directly) decrease rainfall interception and plant transpiration, or decrease the sol protection against raindrop energy, so (indirectly) favour soil crusting. Soil crusting usually decreases infiltration rates and favours rainfall-excess overland flow. Please, describe more precisely the drivers and mechanisms that have been pointed out for explanation of the 'Sahelian paradox'.

OK, thank you for the suggestion.

Page 4, line 2: "and runoff is frequently generated over them."

OK

Page 4, line 21: KINEROS was not designed explicitly for arid and semiarid areas.

Agree, but most of K2 applications concern semi-arid zones so far. The sentence will be changed accordingly.

Page5, lines 18-22. These are not soil and land cover data but just images.

OK

Page 6, line 13 (and below): "Erosion surface" seems to refer to a geomorphic unit (land form), but here is not a good denomination for a landscape unit because it is not clear to most readers and it is equivocal with the soil erosion processes. "Pediment" is a geomorphic English term equivalent to the French "glacis" term that could be used instead.

OK. We will change the text accordingly, thank you.

Page 6, line 30 and subsequent: This is one of the main weak points of the paper, as the method used assumes that there is no change in the fine temporal structure of rainfall events. In the lack of data to improve the approach, some sensitivity exercise should be made to test the role of changing this structure on runoff generation. This may be made using a range of 'ensemble' 5- minutes series with higher, average and lower 5-minute intensity within reasonable bounds.

This is certainly an important topic. However, we have little information so far to provide reasonable bounds for 5-min intensities. There seems to be a change in the frequency of high rainfall events

(Frappart et al. 2009, Panthou et al. 2014), starting around 2000. This is a rather weak signal though. To our knowledge, trends for the very scarce 5-min long times series (ex. Niamey station, Léauthaud et al. 2016) have not been evidenced. We feel like such a sensitivity test would be a little bit speculative for this paper and we prefer to stick to the conclusion that daily rainfall regime change (which has been observed with large dataset) does not contribute to increase runoff in our case.

Leauthaud, C., Cappelaere, B., Demarty, J., Guichard, F., Velluet, C., Kergoat, L., ... & Mainassara, I. (2016). A 60-year reconstructed high-resolution local meteorological data set in Central Sahel (1950–2009): evaluation, analysis and application to land surface modelling. International Journal of Climatology.

Page 7, line 19 and subsequent: In fact, there is a terminological confusion in the paper respect to the changes in the drainage network: the changes observed are really changes in the stream channel network; in the old period runoff was too slight or infrequent in the thalwegs to form distinguishable channels that were cut after the drought period (see another comment below). The subsequent paragraph describes how the DEM derived drainage network was adapted to the network observed in 2011, but not clearly how the 'old' network was parameterized.

OK, we agree that additional details are needed here. This will be explained in more details

Page 7, line 34: Thickets

Page 8, line 1: Low LAI is a reason but high winds favour the evaporation of intercepted rainfall. Check the rainfall interception literature in semiarid areas (e.g. Llorens & Domingo Journal of Hydrology, 2007)

We have been checking the literature before assuming no interception. There are few data for similar biomes and similar climate (strong convective event with strong gusts, low vegetation cover).The nearest case studies are for semi-desert sites or desert sites, for instance the sites documented by Carlyle-Moses, D. E. (2004). Throughfall, stemflow, and canopy interception loss fluxes in a semi-arid Sierra Madre Oriental matorral community. *Journal of Arid Environments*, *58*(2), 181-202. (Review by Llorens and Domingo is mainly for trees, and include 3 bush sites with Mediterranean climate).
Studies in arid/semi-arid environment point to small interception losses (e.g. less than 10%) with high throughfall and significant stemflow.
In our case, the rainfall rates that produce runoff are the highest convective rates, for which interception can be neglected. We agree that interception can occur for instance at the very end of a convective event, when 'stratiform' precipitation sometimes occur, but these do not produce runoff. Also, the area contributing to runoff has an extremely low vegetation cover. Deep soils with important grass cover and scattered trees don't produce runoff, although interception losses are set to zero.
Therefore, we believe it is reasonable to neglect interception when the objective is runoff simulation.
In terms of evaporation during convective events, the high winds (gusts) come with high relative humidity (close to 100%), cold air, and large cloud cover (and the diurnal cycle of convection and squall line produces maximum rainfall from late afternoon to early morning). Information can be found in Frappart et al. 2009, Guichard et al. 2009, Samain et al. 2008, Largeron et al. 2015, for rainfall and associated meteorological data.

Largeron, Y., Guichard, F., Bouniol, D., Couvreux, F., Kergoat, L., & Marticorena, B. (2015). Can we use surface wind fields from meteorological reanalyses for Sahelian dust emission simulations?. *Geophysical Research Letters*, *42*(7), 2490-2499.

Samain, O., Kergoat, L., Hiernaux, P., Guichard, F., Mougin, E., Timouk, F., & Lavenu, F. (2008). Analysis of the in situ and MODIS albedo variability at multiple timescales in the Sahel. *Journal of Geophysical Research: Atmospheres*, *113*(D14).

Page 9, line 22 and subsequent: In the writing of the following paragraphs there is sometimes confusion between the changes of the extension of the mapped landscape units, the changes of the properties of these units and the related hydrological processes.

OK, this will be rewritten. Thank you.

Page 10, line 9 and subsequent: Please, change the "Erosion surface" term.

OK

Page 10, line 11: “an important erosion of the underlying soil has occurred”: do you have evidences of this phenomenon? Where are the eroded soils deposited? “Impervious bare soils have replaced most of these areas”: this is not a rigorous description of a landscape change. In all this paragraph there is confusion between changes in the map units, the characteristics of these units and the processes related to these changes (as causes or consequences).

This will be rewritten more clearly. Erosion is both wind driven (particles are exported/imported) and water driven (particles are deposited in ponds and channels) and both processes interact (wind driven particles can be washed out by water erosion for instance, and dry banks can be eroded by the wind).

Page 10, line 15: The development of a drainage network (in fact this seems to mean that new channels are observed in previously unchannelled thalwegs) may be attributed to the increase of overland flow, but not necessarily to the change from sheet runoff to concentrated runoff on the hillslopes, unless new rills and shallow gullies are observed throughout. The entrenchment of channels in semiarid conditions has been attributed to increased runoff or the decay of valley bottom vegetation (e.g. Nogueras et al. Catena 2000 and cited references).

The two factors can play a role even if it is not easy to distinguish between them (an example of changes in the drainage network is shown below)

Runoff concentration was pointed out by several studies carried out in the Sahel also. A typical case is the transformation of a tiger bush (e.g. site 8 of the long term survey, Dardel et al. 2014a), with vegetation bands perpendicular to the flow, which is replaced by bare soil with scattered trees (mostly Acacia ehrenbergiana) that grow along the newly created rills, parallel to the slope. Field survey provide many example of conversion of sheet to concentrated runoff in this area. Of course there is an interplay between increased runoff, concentration, vegetation decay.

Page 11 line 5 and subsequent. This sub-section is not well written. The parameterisation of the channels is not a result. Please, describe changes in precipitation before changes in discharge and use a chronological order of the periods when possible. Reporting discharges in volume is really difficult to follow, please use runoff units (mm/year). Please, state observations before simulations throughout.

This will be done.

Page 11, lines 16-18: this paragraph is unnecessary unless the behaviour of the catchment is better described, as proposed above.

OK

Page 11, line 24: Please, include a sentence recalling that the reference run is the recent period and that changes are evaluated using equation (4).

OK

Page 11, line 26 and subsequent: “... present characteristics except dune crusting ...”. “... has two effects on the parameterisation of the land surface ...”. “... dune crust on the simulated annual discharge...”. Reporting volumes for the sub-basins is confusing, please report percentages of the total runoff and clearly state that these are simulated values for the recent/older periods.

OK

Page 12, lines 3 and subsequent: Please, state the (indirect) effect of the vegetation changes on model parameters, this is to help understanding the runoff slowing and infiltration increase. Please, report discharge in mm.

OK

Page 12, lines 14 and subsequent: “Modification” and “erosion surfaces” are not appropriate terms here, as discussed above. The “increase in erosion surfaces” is contradictory respect to the small changes in these units as described in Page 10 line 9. Here there is confusion between soil properties and landscape units, please be more explicit.

OK thank you for the suggestion.

Page 12, lines 20 and subsequent: “ the result is an increase of xxx mm/year of the discharge for the past period...”

OK

Page 12, line 28: mind the confusion between soil and landscape unit

OK

Page 14, line 36: “... and soil properties may largely explain...”

OK

Page 15, lines 3-4: “The lack...concentrated runoff”: There is a melange of causes and consequences, yet, the change from sheet to concentrated runoff must be demonstrated.

OK. Runoff concentration is best observed during field survey (new rills, sometimes cutting through sand / silt bars, and vegetation changes from tiger bush tickets perpendicular to the slope to scattered trees like Acacia ehrenbergiana growing along rills parallel the slope. Hiernaux et al. 2009, Dardel et al. 2014a)

Page 15, lines 13-14: See the note above on channel entrenchment.

OK

Page 15, line 14 and subsequent: “Our work has shown that enhanced and concentrated runoff results in an increase in both the number and the length of channels, therefore increasing the drainage density and diminishing the travel time for water to reach the drainage network” : This is not shown in the results above.

See comment above about this. We will rephrase and stick to drainage network development.

Page 15, line 19: “Our results suggest that...”

OK

Page 15, line 28: “are simulated as part of vegetation...”

OK

Page 15, line 33: “surface runoff is observed and simulated to decrease...”

OK

Page 15, lines 36-27: a preliminary test should be made changing the fine temporal structure of rainfall, as suggested above.

See comment above.

Conclusions: this section should be rewritten after the revision of the manuscript, but it is important to bear in mind that in this case the model approach may be useful to “investigate” or to “shed some light on” the paradoxical evolution in the Sahel, but not to “understand” it.

OK, we agree with this statement. This will be done. Thank you for the suggestion.