RC#5

Referee comment on "Modeling the paradoxical evolution of runoff in pastoral Sahel. The case of the Agoufou watershed, Mali" by Laetitia Gal et al. (Hydrol. Earth Syst. Sci. Discuss., doi: 10.5194/hess-2016-623, 2016.)

This reviewer largely agrees with many of the comments already expressed by reviewers RC2 and RC4. Given the numerous issues expressed I feel the paper should be reframed and possibility retitled along the lines expressed by reviewer RC3 whose last suggestion was "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." A new title might be something like "Exploration of the paradoxical evolution of runoff in the pastoral Sahel - Agoufou Watershed using available data and a watershed model."

We thank reviewer 5 for providing valuable comments and remarks on the first manuscript, as well as on the comments/suggestions of the other reviewers. We appreciate the suggestions based on a deep knowledge of the K2 model.

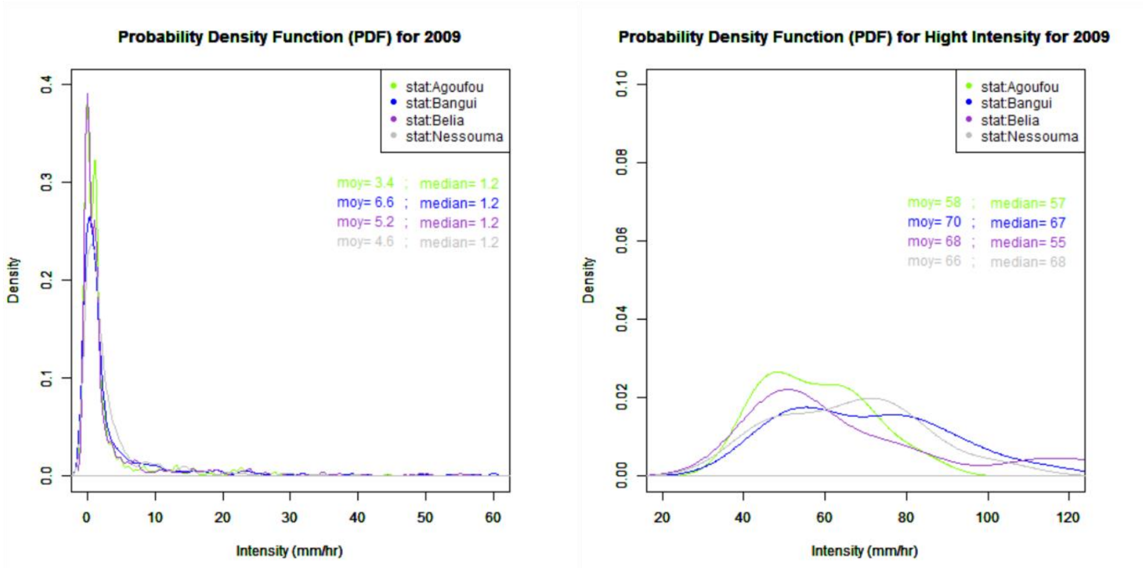
The title will be modified following your suggestions, thank you. Also, we will moderate the terms used in the conclusion and in the manuscript to emphasize we present modelling-based conclusions.

The author could then stress that the model selected could be one of many for this investigation, but K2 was selected for x, y, and z reasons as a tool to investigate possible reasons for the paradoxical evolution of runoff in the Agoufou watershed. Within the constructs of the model, its structure, and the assumptions inherent in the model it was felt it could be used to conduct a relative ranking of various factors and watershed attribute changes contributing to the paradox. Using other models one might come to different conclusions or attributions but the authors could encourage others to conduct comparable "detective" investigations to better understand factor contributing to the paradox.

A preliminary study, not detailed here (found in Gal L., 2016, "Modélisation de l'évolution paradoxale de l'hydrologie Sahélienne. Application au basin d'Agoufou", PhD thesis, Université de Toulouse) based on a literature review was carried out with 20 different hydrological models (global, distributed and semi-distributed) in order to select the model best suited to the zone and the objectives of the study. KINEROS2 was found to be suited for the study objectives. In addition a model/data intercomparison project, called ALMIP2 for AMMA Land Surface Model Intercomparison phase 2, was carried out in this area to assess the capability of land surface models (LSMs), vegetation models and hydrological models to describe hydrological processes in this area: 20 different LSMs were analyzed (Grippa et al, in press in JHM, 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). The results highlighted the difficulty of models to distinguish between shallow or silty soils generating the runoff ending up in ponds and no-runoff areas, like sandy deeper soils, which infiltrate all rainfall. LSMs have been found to be too sensitive to rain and not enough to soil properties. These various arguments explain why we have chosen the Kineros2 model.

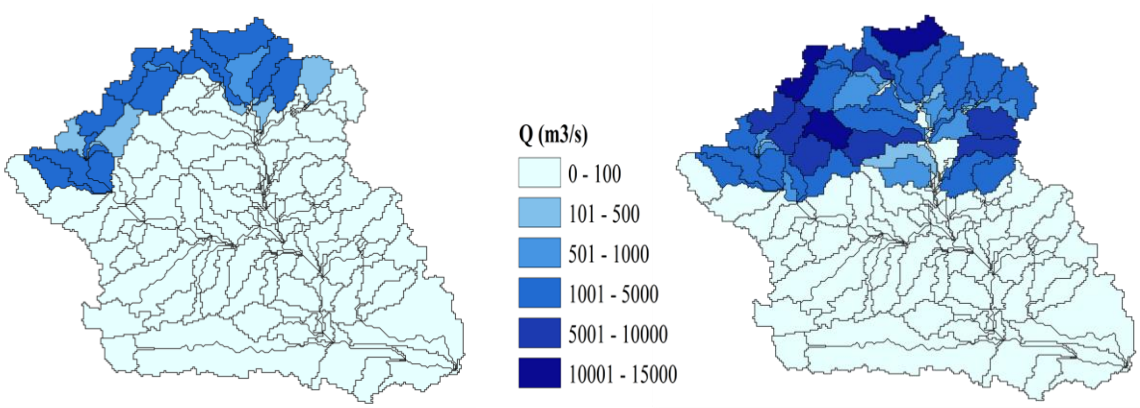
As pointed out by the other reviewers the uniform precipitation assumption for a basin this size constitutes a major simplification and calls into question the ability to carry out a defensible model calibration and validation. Al-Qurashi et al. (2008) applied K2 to a 734 km^2 arid watershed with 7 rain gauges where a "parameter set which gave best calibration performance over any combination of 26 events did not generally produce acceptable performance (defined as within 30% of observed) when used to predict the 27^{th} event". In this and similar situations, the authors noted that "data sets typically used for distributed (or semi-distributed) rainfall-runoff modeling in arid regions cannot provide an accuracy which justifies the effort and expense of this (K2) modeling approach. The limitations imposed by relatively sparse observations of rainfall are of particular concern" (Al-Qurashi et al., 2008, p. 104).

The uniform precipitation hypothesis is an important point and has required additional analyzes not detailed in the manuscript. 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, so we have no station in the north of the Agoufou watershed). 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 catchment. Therefore, only one third of the watershed is concerned.

Past (JAS) **Present (JAS)**



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). 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 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. 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.

Qurashi et al. (2008) analyzed their results at the event scale unlike we do. We analyze the results on an annual scale and 15-year average scale. The point is indeed to analyze the results in a statistical way in the light of uncertainties related to the precipitation.

To remove this major limitation and use K2 as a tool to explore causes of the runoff increase this reviewer suggests taking a relative change approach as advanced by Goodrich et al. (2012) and Sidman et al. (2015) for post-fire watershed assessment in watersheds that often do not have any rainfall-runoff data available for calibration and validation. In this approach a pre-fire land cover map is used to parameterize the watershed and conduct a simulation with a spatially uniform design storm. The burn severity map is then used to alter model parameters based on prior research and analysis of the effects of burns on cover and soil hydraulic properties. A second post-fire simulation is then conducted using the same rain storm. The results can then be spatially differenced to analyze changes. For the author's study the present and past model parameterizations based on analysis of historic and current land cover and soils data are analogous to the pre- and post-fire conditions. The authors could pick one of their most trusted rainfall data sets (perhaps when they had high temporal resolution measurements) and use that rainfall input data set for both the past and present watershed model parameterizations. Given one of their conclusions (last paragraph) was that climatic and precipitation changes from past to present appeared too little or no impact on the findings this would further justify the approach noted above. By doing so the authors would isolate the analysis on watershed changes as they would be using identical input drivers. This would still be directly in line with the stated objectives of their study.

In fact, if we understand correctly, using 5-min data from a look-up-table (i.e. well documented storms) is similar to what you propose, but done in a more systematic way since all rain events are considered. This holds true for the Present compared to C, D, V and S simulations. Only the P (precipitation) simulation uses different 5-min evens, corresponding to Past daily rainfall.

Technical Comments:

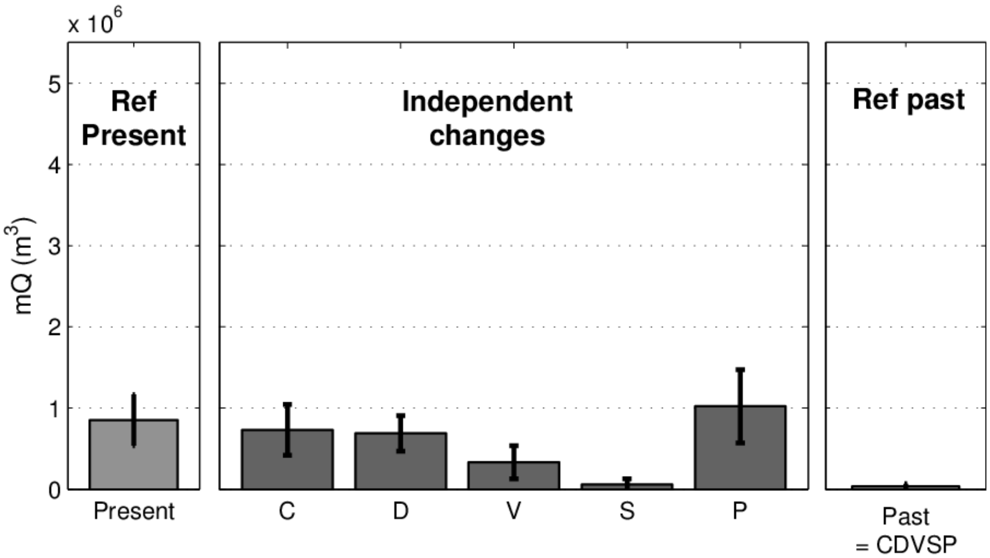
The authors have confused the meaning of the CSA or contributing source area. This is the drainage area required it initiate the head of a first order channel and effectively defines the level of geometric complexity of the watershed with a smaller CSA (percent of drainage area or absolute area) resulting in more watershed modeling elements. The channel source area modeling elements are those that drain to the head of a first order element. The remaining upland or hillslope modeling elements (planes – they can be curvilinear as well) contribute laterally to channel modeling elements.

That's right, CSA was used for all planes. This has been corrected.

Regarding the questions by other reviewers of K2 model sensitivity the author's should review and cite Yatheendradas et al. (2008) who conducted a thorough analysis of variance. In their analysis, model prediction uncertainties are dominated by precipitation input uncertainties (another reason in suggesting the approach noted above). For K2 model parameters a multiplier on the Ksat of overland flow model elements and the Manning's roughness multiplier on overland flow model elements were the most sensitive parameters while the channel roughness multiplier was also quite important. Given this information it is odd that the authors selected the Ksat of the channels and not of the overland flow planes for calibration. Note that Ksat of channels and Ksat of hillslope elements do interact. The relatively low calibrated Ksat channel value that the authors found could easily be the result of a higher Ksat on the hillslope elements resulting in lower lateral runoff inflow into the channels.

We agree with the high sensitivity or runoff to planes parameters. Indeed, it is one conclusion of our study (Soils and Vegetation changes on planes ranking first and second).

We will explain the rationales of our approach in a more precise way in the revised manuscript: Not to calibrate the planes parameters (which are constrained by our land surface maps). Calibrate channels only, which are less known, with the only objective to check than plane runoff produces a total flow consistent with observation, with plausible values of the channels parameters.



To make it clearer, we have performed a sensitivity test to planes parameters (see figure above), multiplying planes Ks by 2.5 and planes MAN by 1.75 (based on literature review by Casenave and Valentin, 1989). The absolute values of total runoff changes as expected, but the ranking of the factors is the same. This gives robustness to our findings.

(Technically, we did not use the multipliers because they modify the parameters of the planes AND the channels. Adding this feature, either adjust planes or channels, is in discussion for implementation in KINEROS2, Shea Burns, pers. com).

The calibration of channels parameter plays a minor role, ensuring only that the planes 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.

Given the author's finding of the importance in the change drainage density and channel characteristics two items are suggested:

1. Did the author's use the default values for channel cross-sectional geometry? If so these value were derived from regressions relating X-S measurements to easily derived variables from GIS operation on watershed data as obtained at the Walnut Gulch Experimental Watershed in SE Arizona, USA (Miller et al., 2000). The Walnut Gulch relationships are likely to be a poor representation of the channel cross sectional characteristics of the Agoufou watershed. It is suggested that the authors gather some field measurements from the Agoufou watershed. At least from several stream orders so they might be scaled across all the channels in the study watershed.

We only changed channels width (10 ans 11 m) to fit observations and checked that channel depth was correct (.4 to .7m).

2. Instead of altering the aspect ratio of the overland flow (plane) hillslope elements a watershed discretization can be derived from for each (past and present) channel network (contact Shea Burns for details).

It was probably not clear in the first manuscript, but changing the planes aspect ratio reproduces exactly the elongation of the channel network (since lateral plane's width corresponds to channel length). Changing CSA was considered also but there was a risk of changing plane properties in an uncontrolled way (changing

CSA with DEM doesn't seem straightforward in K2). That would have complicated the interpretion changes between the past and present cases.

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