

RC#1

Gal – Modeling the paradoxical evolution of runoff in pastoral Sahel. The case of the Agoufou watershed, Mali

We thank reviewer 1 for reviewing our manuscript and providing valuable feedbacks. We have addressed all of his comments and discuss them below.

General observations

This paper deals with the Sahelian paradox: despite the decline of the annual precipitation, the Sahelian region is paradoxically subject to an increase in runoff associated with an increase of the runoff coefficient. The causes of this phenomenon, commonly known as the "Sahelian paradox", are not yet clear. Based on an event-based and physical-based hydrological model, Kineros2, the authors model the runoff on a small basin located in the Gourma under the Niger River loop. The model allows them to prioritize the different factors that lead to this paradox. The title of their article is moreover incomplete since this last part of prioritization does not appear in the title.

Thank you for the suggestion

We will change the title accordingly (also suggested by other reviewers).

This question of the Sahelian paradox questioned some researchers but many hypotheses have not been validated.

My first observation concerns the approach: a model is not the reality, a model is only an impoverished image of the reality, even a physical-based model: there is always a process of calibration of parameters to be launched; a model parameter have never a physical meaning. Therefore, we can not rely exclusively on a model, however excellent it may be, to determine all processes involved in rainfall-runoff transformation, and even to prioritize them. Now, this paper gives the impression that the authors seek to validate their knowledge they have of the problematic by means of a model. I preach for my part for incessant back and forth between observation and simulation ... I'm therefore a bit dissatisfied ...

We agree, a model is not the reality. We will change the manuscript to make clear that explanations are "according to the model" and that the results are subject to uncertainties. We believe that this study, even if model-based, has shed new lights on the Sahelian paradox phenomenon and that it provides an important contribution to the debate on the man-made versus natural causes.

My second observation concerns the very numerous approximations made by the authors: we do not know what are their simulation impacts, because the authors did not discuss the subject. They present mean or median results that ultimately smooth the response of the basin.

We have added 2 figures to document modeling result in more details (maps of MAN, Ks, runoff over the watershed, for the Past and Present cases, as well as Precipitation / Runoff plots for the Past and present cases) which illustrate simulation results and add spatial and temporal information (see specific observations section for the Figures).

There is one point that deserves more detailed explanations: the need to lump events and to look at runoff in a statistical way. It is directly linked to the temporal disaggregation of rainfall. We have physical reasons to use a short time step (namely the importance of Hortonian runoff). For each daily precipitation amount, we use ten events with a 5-min resolution. Since they are taken from a 5-min look-up-table, it ensures that on average, we have a good distribution of 5-min intensities (see the figure included on the response to Technical Comment p. 6, below). At the scale of a single event, though, we have no guarantees that the 5-min intensities correspond to the reality. For this reason, we look at annual means and 15 years averages (which are based on a large number of events, so that the statistical distribution makes sense). We also show the variability induced by temporal disaggregation and seasonal results, as an illustration (grey envelops in Figure 6). We will explain that in more detail in the revised manuscript.

However, I congratulate the authors for all the data that they were able to collect and process (it is not

simple in these environments) and which was the basis of this work.

Thank you for this remark. We agree that important scientific questions arise in less observed areas.

Specific observations

The study material is very simple: a single watershed, which does not make it possible to give a universal character to the results obtained.

Due to the limited data availability, no studies of this kind have been carried out up to now in pastoral areas of the Sahel, which makes our study original. The Agoufou watershed is a great case study given the unique long-term environmental monitoring, thanks to the AMMA-CATCH observatory and older programs, starting in the 1980s-90s (Boudet, 1972; Hiernaux and Turner, 1996). The watershed displays a spectacular increase in runoff, which has been quantified in a previous study (see Gal et al., 2016).

In addition, the strong evolution of surface water observed at Agoufou has also been observed in the Gourma region (91 lakes, Gardelle et al, 2010) and elsewhere in the Sahel (Niger and Mauritania, see Gal et al., 2016).

We believe that the mechanisms highlighted here for the Agoufou basin may be at play in other regions of pastoral Sahel. Moreover we cannot exclude that these mechanisms may also play a role in other areas where land use change was considered the major cause for the observed hydrological changes. This of course calls for additional studies.

We will explain in the revised section "study area" the reasons for this choice, which also responds to comments by other reviewers

Boudet, G., 1972. Désertification de l'Afrique tropicale sèche. *Adansonia* 12, 505–524.

Hiernaux, P., Turner, M.D., 1996. The effect of clipping on growth and nutrient uptake of Sahelian annual rangelands. *J. Appl. Ecol.* 33, 387–399. doi:10.2307/2404760

I would appreciate that the authors use at least one other model and compare the results of these different models and compare them to their observations and their knowledge of the terrain.

A previous study, not detailed here (found in Gal L., 2016, "Modélisation de l'évolution paradoxale de l'hydrologie Sahélienne. Application au bassin 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 purpose.

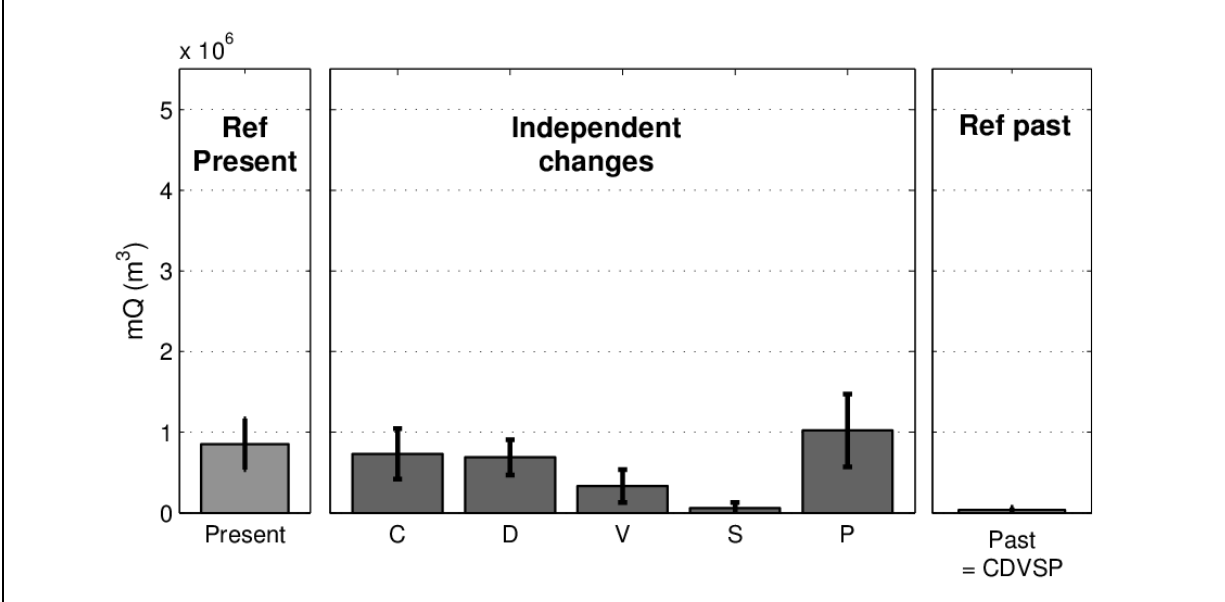
In addition a model/data intercomparison project, called ALMIP2 for AMMA Land Surface Model Intercomparison phase 2, has been carried out in this area to assess the capability of land surface models (LSMs), vegetation models and hydrological models to describe hydrological processes in this area: 20 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> or upon request to M. Grippa). The results highlight the difficulty of models to distinguish between shallow or silty soils generating the runoff ending up in ponds and no-runoff areas, like the sandy deeper soils, which infiltrate all rainfall. LSMs have been found to be too sensitive to rain and not enough to soil properties. We hope that our results will stimulate the scientific community to undertake further studies in different basins and with different models to validate or invalidate our findings. The data are being put on the AMMA-CATCH database in that purpose.

Please, give ranges of uncertainties of your treatments/process

For observational data, the uncertainties are detailed in Gal et al. (2016) and will be recalled in the revised manuscript.

For the uncertainties on the planes parameters, we will provide additional results in the revised

manuscript. A sensitivity study has been carried out to highlight the robustness of the model in ranking the factors responsible for the increase of surface runoff. To that end, K_s of all planes was multiplied by 2.5, and MAN by 1.75. This corresponds to the interval given by Casenave and Valentin (1989) for many Sahelian soils. Both changes (K_s and MAN) tend to decrease runoff, therefore the combination of K_s and MAN decrease total runoff by a factor of 3. The ranking of the different factors is however the same as with the original planes parameter. This test illustrates the robustness of the results.



I'm not native-english, so I cannot evaluate the quality of the English.

Technical observations

Page 3, lines 30 and after : It also means that as a result of important rainfall events, these ponds may be temporarily interconnected for a more or less long period. Is this type of interconnections possible at Agoufou pond?

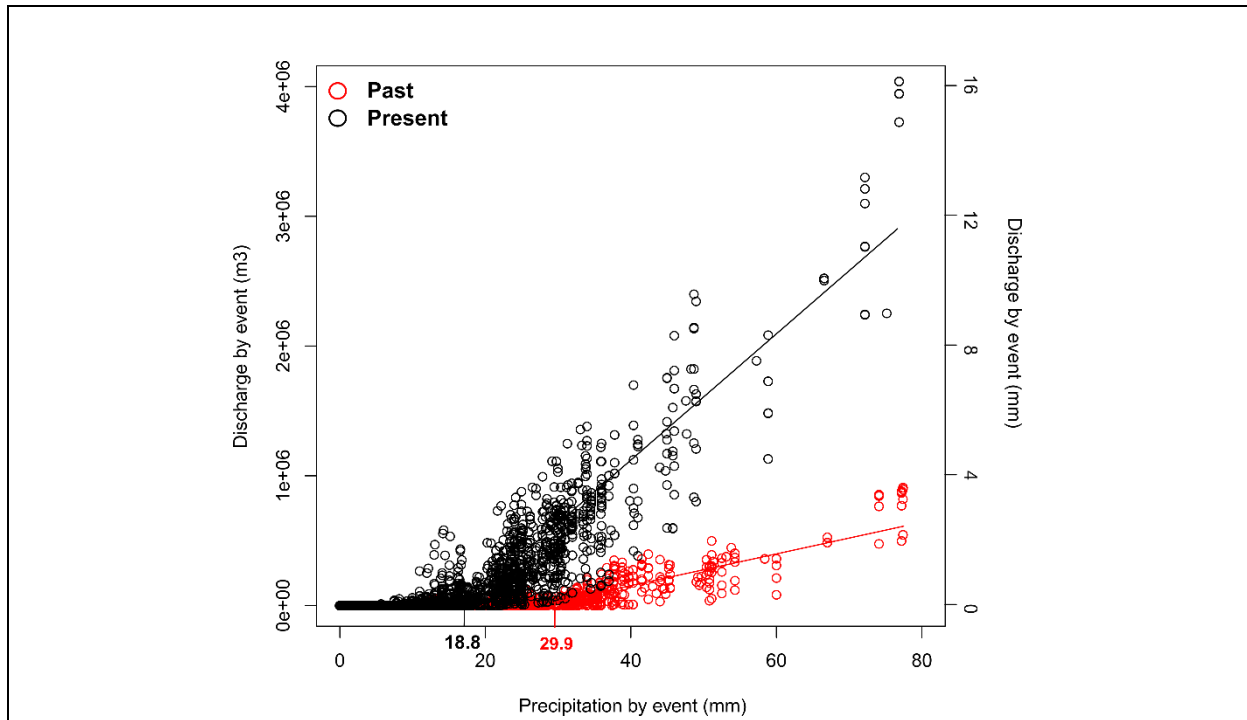
According to the satellite images and regular field survey, no visible connection between the Agoufou lake and the eastern pond has been observed during the whole study period. However, with the dramatic increase in the surface water and precipitation recovery, it is possible that in the future these two lakes will be connected.

Page 4, lines 18 and after: the problem of such a model (event-based model) is to fix the initial conditions for each simulation: how do you proceed?

This is explained later in the article (page 7 line 1 and 30). The initial properties of the soil have to be prescribed to run KINEROS2. We have calculated the time required for the top soil to return to an initial state (dry soil over the first few centimeters) using soil moisture profiles available for different soils via the AMMA-CATCH observatory (described by de Rosnay et al., 2009). This time is rather short (of the order of 48h, depending on soil type) and it is used to separate the different rainfall events. This justifies to reset the soil moisture to initial condition before each event, especially in an area where Hortonian runoff dominates.

Page 5, "Precipitation and meteorological data...": you need to more detailed your data. Some analyses are needed

We have added the location of the Hombori station in the revised Figure 1 and we give the time scales of the field data (15 minutes for meteorological data used for the STEP model input). In addition, we will add the references to Guichard et al. (2009), Frappart et al. (2009), Timouk et al. (2009) and Gal et al. (2016) who have already analyzed and detailed the in-situ data used here. In addition, we have included new figures, one of them figuring runoff/precipitations for all events.

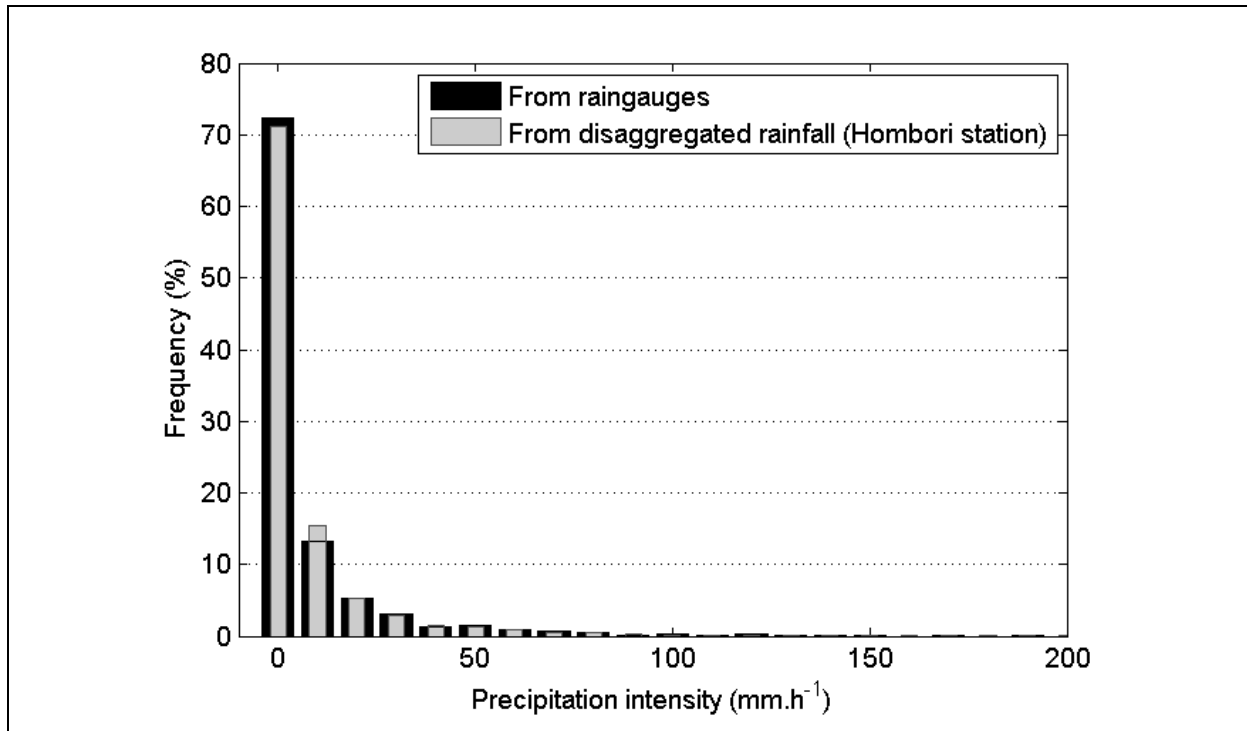


Page 6, “landscape...” : did you discuss of your results with the local population? Did they validate your maps of landscape/.drainage network evolution?

The site has been visited by our team during several field campaigns per year until 2012, and we are working with locals since then (security issues prevent site access for French scientists). One local village chief has been involved in the project since the beginning, and different other persons have been involved in data collection. One of us (Pierre Hiernaux) is extremely familiar with the site, his first measurements in this area started in 1984, and he has lead the work done on the landscapes map (L. Kergoat and M. Grippa also spent time in Agoufou each year in 2004-2009). We are in regular contact with people living in Hombori and Agoufou, who provide us with valuable information on this region and field data (including regular photographs of the lake, water height, and the vegetation development at the long term monitoring sites).

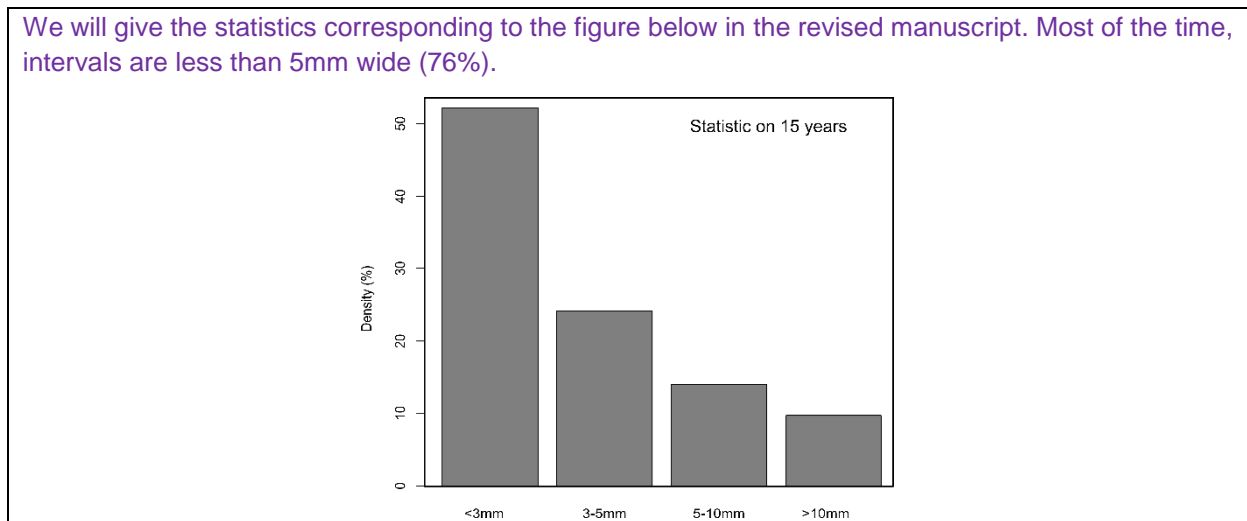
Page 6, “Rainfall...” : astute approach but you have to validate it. That’s why I asked before to more analyse your climatic data.

We have compared the histograms of rainfall intensity (5-min) obtained by the rainfall disaggregation of daily data from the Hombori synop station (figure below, in grey) to 5-minutes data from different rain gauges around the Agoufou watershed available during the 2005-2011 period (black). The figure below shows that the histograms are quite comparable, particularly for the high intensities, which are the most important for runoff production.



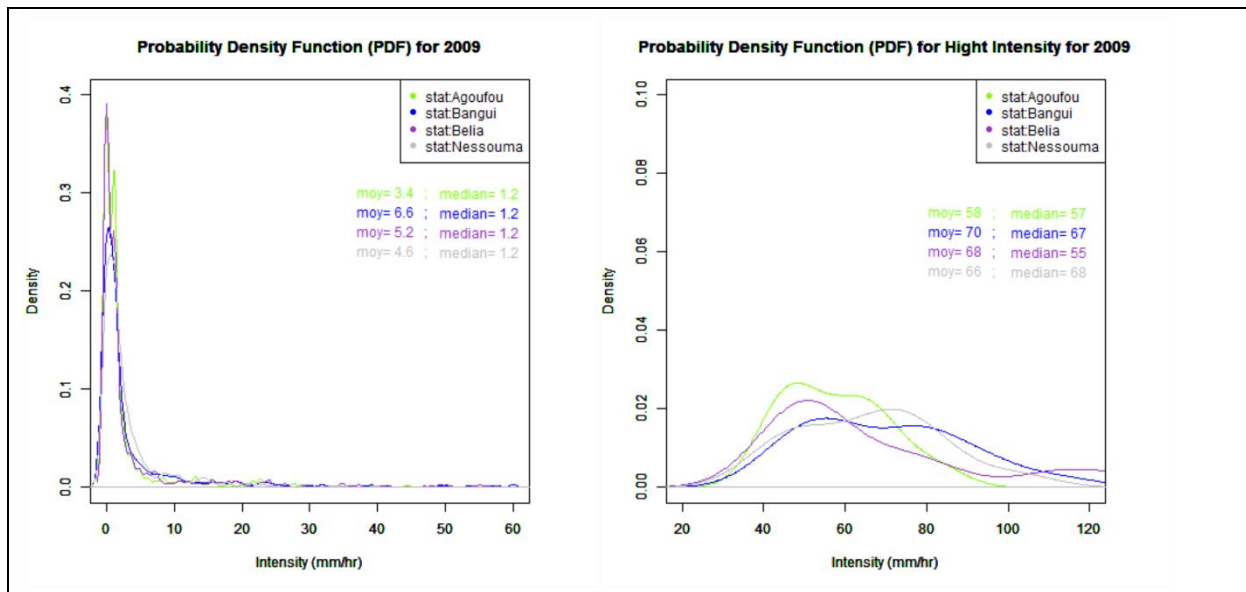
Page 6, lines 35 and after : can we have an idea of how many times you have to widen your intervals?

We will give the statistics corresponding to the figure below in the revised manuscript. Most of the time, intervals are less than 5mm wide (76%).



Page 7, line 4 and after : can't you validate this assumption with the synop station and the stations network of Amma-Catch program?

Indeed AMMA-CATCH stations have been used to address this question. 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). To further investigate the question, 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.



Page 8, “model calibration...” : why don't use an automatic calibration? Why these intervals : you tell later that some values found in the literature are higher? Can we have the dispersion of your ten simulation for an event?

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 combinations 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 too much precision is meaningful. It is interesting to note that, once the plane parameters (Ks, MAN etc.) are prescribed according to local map and survey for texture and built-in FAO soil K2 classification, consistent channel values (compared to the literature) are obtained through the calibration. MAN values of about of $0.03 \text{ s.m}^{-1/3}$ are commonly reported for Sahelian channels with Ks being more variable depending on the material being eroded on the basin (here mainly silt and clay).

We do not work at the scale of the event so we did not calibrate at that time scale. However, we also calculated the bias at the intra-annual scale (with all observation data) and we get an average bias of -8% (RMSE = 4.6×10^5), which is also acceptable.

Page 9, “reference ...” : it is a too simplistic assumption which have an impact on your results... Isn't it possible to use “interpolated situations”?

We are not sure that we completely understand the question but if it is about providing intermediate steps between the "present" case and the "past" case, it is not possible because we have no data (runoff and land cover evolution) available over this intermediate period. LANDSAT satellite data are rare in the 90ies in this region do to the unavailability of a ground reception station to record acquisitions, over West Africa.

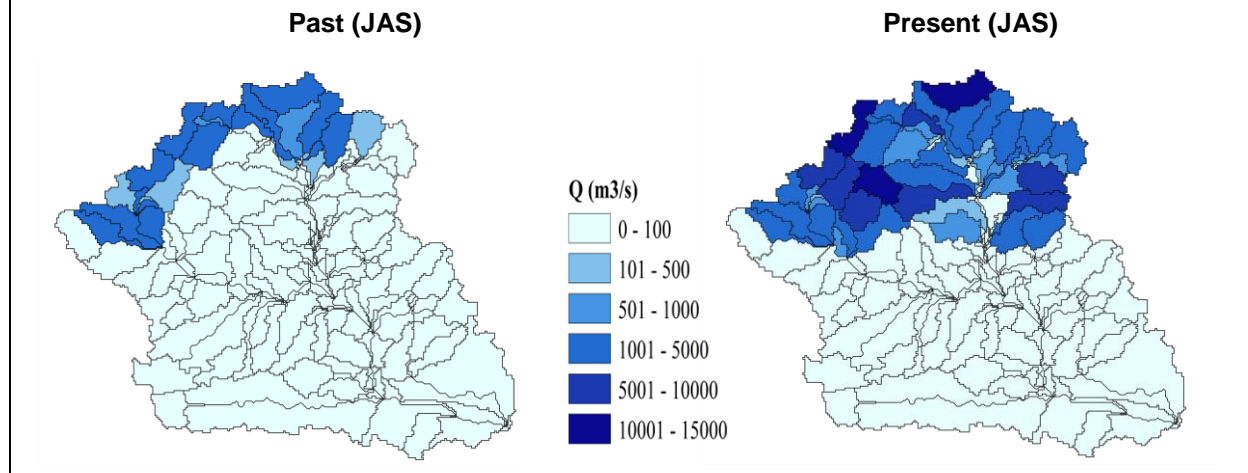
Page 9, “soil land...” : give ranges of the uncertainties of your process. How can you say that in 2011, the western area of the basin contribute to the Agoufou pond? In the Inner Delta of Niger, there is the same phenomenon of interconnected lakes during some strong events; these interconnections are not necessarily permanent ant can disappear for a while; It's certainly the same here. How can you be sure that it does not happen before, between 1960 and 1975?

On the aerial photographs of 1956 it is clear that the western part is not connected to the main network unlike in 2011. This connection is mainly due to an increase and increased concentration of surface runoff given that the heaviest rains before 1956 were not sufficient to connect the western part.

As specified in the article (4.3.2), even if the connection occurred prior to 1975 it should not change the

results significantly, since the western part contributes only weakly to the outlet (annual volume contribution = $3.3 \times 10^4 \text{ m}^3$).

The maps below, which will be added to the revised paper, shows the spatial distribution of runoff over the watershed. It highlights the important changes in the northern part of the watershed, which more than doubled the contributing area.



Page 13, line 37 : Pierre at al., 2016 is not referenced

Thank you for highlighting this, it will be corrected

Page 14, line 32 : "erreur..." ????

Corrected. Apologies for this error

Page 15, line 34 : what are the "stocking rates"?

It is "Livestock stoking rate" («pression de pâture" in French)