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

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. 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 parameterisations.

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. 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. 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.

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.

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

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

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".

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?).

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.

Page 2, Line5: During or after the drought?

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'.

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

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

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

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.

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.

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.

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)

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.

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

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).

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).

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.

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

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).

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.

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.

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.

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

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

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

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.

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

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.

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

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

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

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