

Referee comment on “The paradoxical evolution of runoff in pastoral Sahel: Analysis of the hydrological changes over the Agoufou watershed (Mali) using the KINEROS-2 model.”

The manuscript clearly improved in clarity and quality after the revision and now most of the objections for its publication do not apply any more. Nevertheless, there are some technical and scientific aspects that should be modified before it being acceptable for publication in HESS.

The main question was already sated in the first revision and the corresponding changes introduced are not still satisfactory to this reviewer. The sentence *shallow soil being eroded and replaced by impervious soils* in the abstract and the corresponding one *with shallow soils being eroded and being replaced by impervious soils* in the conclusion sound very catastrophic, so the reader expects finding the description of erosion-deposition features such as new gullies and recently formed alluvial fans or sediment filled channels (phenomena not easily understood in such a short time and under deep droughts). But these phenomena are not described in the paper; if well understood, the only description of similar facts appears in Table 2, P4: *degradation of the tiger bush results in eroded and crusted soils which are largely impervious and produce important surface runoff*. Consequently, it seems that large parts of a landscape unit identified as “P3: Hard pan surface with tiger bush “ in the old surveys were identified as “degraded tiger bush” in the new ones, but the main difference found seems to be the degradation of the vegetation cover but not the erosion of soils. Similar facts seem to be applied to the changes between P1 and P1v (herbaceous vegetation layer indicating the occurrence of shallow sandy soils <30 cm).

In other words: the authors seem to have over enhanced the role of erosion processes in the abstract and conclusions in comparison with the observed facts. Soil erosion cannot be frivolously inferred or claimed if there are no clear observations.

Other comments:

Flooded areas and floodplains are sometimes used a synonymous, but it is important to make clear which are the changes: floodplains are geomorphic units that do not change easily in centuries whereas flooded areas can vary for every event.

Page 1, line 16: “...quantify and rank different processes...” because not all the processes are analysed

Page 2, line 28: “...reduction of soil hydraulic conductivity...”

Page 4, line 10: “...which favour the frequent generation of runoff...”

Page 9, line 8: “... from soil textures throughout the world...”

Page 9, line 10: 0.1% or 0.001 m³m⁻³?

Page 14, line 4: “... summarized in Fig. 8 and Table 8...”

Page 14, line 15: “... sand dunes interrupting the water flow...”

Page 14, lines 18-21: It is unclear which is the mechanism how the expansion of the drainage network in the northern area causes an increase of flow at the outlet. Is it because in the lack of this drainage network runoff arrived along planes where infiltration was higher than in the channels?. Please, explain the mechanisms behind this change.

Page 18, lines 24-26: (*Our study implies that enhanced and concentrated runoff and/or increase surface runoff results in an increase in both the number and the length of channels*) this is not demonstrated in the study. The study shows the role of changing drainage density in the total simulated runoff, but not which is the cause of channel expansion. I suggest (as done in my previous review) to take into account the classical literature about channel entrenchment in semiarid areas.

In several expressions throughout the paper there is an improper or doubtful use of plurals, for instance “geometrics parameters” (pag 5, lin 33); “fields measurements” (pag 4, lin 29); channels parameters (pag 11, line20)...

There are many typing errors with lacking spaces throughout the paper