Answer to comments of the reviewers;

Firstly we warmly thank the reviewer for their deep work in this review and for help us in try to improve this paper.

We wrote our comments after each comment of the reviewers

Reviewer N°1

General Comments:
The study describes an assessment of the impact of land cover change in two small watersheds in the Sahel utilizing field observations and a simple lumped model. While the motivation and goals are to be commended, the study falls short in many respects. In a nutshell, the manuscript is both a poor observational study (due to missing details, descriptions, analysis and interpretation of field data) as well as a poor modelling study (due to missing details, analysis and overinterpretations of a very simple model). In addition, the manuscript is poorly constructed, described and discussed. It should be rejected. Below I provide some specific and technical comments that may help the authors in a significant (if not total) reconstruction of their work. The manuscript needs a lot more work than just those that I have highlighted below. One idea is to simply focus on the observations and analyze these in more detail for their intrinsic value. The modelling part will probably not be publishable in any form. I wish I could be more positive, but this manuscript is simply not very good.

Specific Comments:

Page 1570.
Line 7. Please indicate how different or similar the two catchments are.

They are similar;

Line 9. Where and when did vegetation clearing occur?

The land use change commented here is the one observed between 1993 and 2007

Line 14-16. An explanation of the future work is inappropriate in an abstract.

OK it can be removed

Line 1-16. Please focus the abstract on the major quantitative conclusions.

The main land surface change is the increase in 75% of crusted soils area. Runoff increased in more than 20% in average between the two periods, however, it did not change in the lower northern basin, due to a strong increase in in-channel infiltration. Flood durations decreased in 30% in average.

Line 18. References are needed for the changing land use in the Sahel. When was the natural vegetation altered?

Land use is rapidly changing in the whole Sahel (Prince et al., 1998; Fensholt and Rasmussen, 2011; Huber et al., 2011). Vegetation cover decreased significantly according to studies based on remote sensing, of Western Niger, Eastern Mali and Eastern Burkina Faso (Loireau, 1998; Cappelaere et al., 2009; see also maps provided by Prince et al., 1998 and Fensholt and Rasmussen, 2011). Natural vegetation had already almost disappeared at the end of the 20th century in extended areas replaced with crops and fallows (Ada and Rockström, 1993; Leblanc et al., 2008; Hiernaux et al., 2009a; Descroix et al., 2009; Mahamane, 2009). Furthermore, within the croplands, crop areas are increasing while fallow lands are decreasing, both covering 20% of the total space in 1950, and close to 80% in 2000, the remaining space being unproductive lateritic plateaux (d'Herbès and Valentin, 1997; Raynaut, 2001; Guengant and Banoin, 2003; Hiernaux et al., 2009b, Ruelland et al., 2011; Mahé et al., 2011).
(see at the end of this document, the references newly cited in this answer to reviewer)

Line 20-22. The description of the land use change and subsequent erosion are too vague. Be more specific to the sites or regions of interest.

Land use is rapidly changing in the whole Sahel (Prince et al., 1998; Fensholt and Rasmussen, 2011; Huber et al., 2011). Vegetation cover decreased significantly according to studies based on remote sensing, of Western Niger, Eastern Mali and Eastern Burkina Faso (Loireau, 1998; Cappelaere et al., 2009; see also maps provided by Prince et al., 1998 and Fensholt and Rasmussen, 2011). Natural vegetation had already almost disappeared at the end of the 20th century in extended areas replaced with crops and fallows (Ada and Rockström, 1993; Leblanc et al., 2008; Hiernaux et al., 2009a; Descroix et al., 2009; Mahamane, 2009).

Line 26. References to support this statement are needed.

Le Breton, 2004, Massuel, 2005;. Leblanc et al., 2008; Le Breton, 2011 (added ref at the end of this document)

Page 1571
Line 4-8. These descriptions of hillslope crusting and downstream infiltration are very confusing to the reader. Please clarify and support with references.

The observed soil crusting (Casenave and Valentin, 1992, d'Herbès and Valentin, 1997) led to a decrease in infiltration (Vandervaere et al., 1997) which increased runoff and soil erosion (Descroix et al., 2009; Descroix et al., 2011);

Line 7. What are the physical mechanisms for increased transmission losses? Explain.

removed material was deposited downstream and constituted sandy deposit which are new infiltration areas (Esteves and Lapetite, 2003; Leblanc et al., 2008; Descroix et al., 2009), causing transmission losses and a reduction of discharge.

Line 9-21. Is this paragraph needed? The discussion of the two theories is tangential to this effort.

We agree with the reviewer, this paragraph can be removed

Page 1572
Line 3. Please define millet biomass.

Hiernaux et al. (2009a) determined that in spite of land clearing, in Western Niger, total herbaceous yield was increasing because of the growth of crops area; effectively they measured that this yield for millet field (1200 $kg.yr^{-1}$ in average) is significantly higher than this of fallow and rangelands (850 $kg.yr^{-1}$)..

We propose as a new study area (answer to question following)

The Sahelian environment consists of a mosaic of three distinct sunits: shrub bush (referred as tiger bush because of its banded pattern), fallow savanna and millet fields (D'Herbès and Valentin, 1997). Soil water balance is controlled more by surface than by deep soil conditions (Collinet and Valentin, 1979; Chevallier and Valentin, 1984, D'Herbès and Valentin, 1997). In particular soil crusts, which develpp even in very sandy soils, impede infiltration (Vandervaere et al., 1997, Descroix et al. 2009; Descroix et al., 2011). The dry season lasts from October to May at the mean annual rainfall is about 560 mm in Niamey. The region is dominated by a complex geological formation of loamy sandstone of Miocene deposits called the Continental Terminal. It covers the Precambriancrystalline basement complex, part of the pan-African shield (D'Herbes and Valentin, 1997).

The landscape of the Banizoumbou-Tondi-Kiboro area, located 70 km east ward from Niamey, Niger, on the western part of the Iullemeden sedimentary basin (fig. 1). It is dominated by a dissected plateau (Fakara plateau) at the East, with soils formed of 25 cm of gravely loam over cemented ironstone or gravel. The valley of Dantiandou fossil kori (the local name for wadi and river) is a system starting below the relatively steep plateau escarpment, a breakaway at the edge of this plateau with slopes of 4-8 %. Close to the scarp, soils are shallow and contain ironstone gravel but a few meters downslope, the sand deposit are close to 10m thick (D'Herbès and Valentin, 1997, Peugeot et al, 1997); these sandy aeolian deposits adjacent to the plateaux are referred as "sandy skirts",) and they constitute the basement of the basins, as a 1 km-long sandy hillslope. They are extensively cultivated with millet; their topsoil has 10% of silt and clay, and are very vulnerable to crusting (Van de Watt and Valentin, 1992, D'Herbès and Valentin, 1997). In the lower part of the small basins, the thickness of sandy soils decreases and iron-indurated layers outcrops, creating a shelf where very temporary ponds allowed infiltrating the whole water volume flowing in the Tondi Kiboro basins until 2009. Downslope (non connected with the basins until 2009), a convex toe-slope segment links the slope to the temporary stream bed of the Dantiandou kori.

The banded vegetation patch of tiger bush is very dense and includes mainly combretaceae (Combretum micrantum and combretum glutinosum) and other species, mainly guiera senegalensis and sparse balanites aegyptiaca and prosopis Africana (the latter being endangered).

The scarp is characterised by little vegetation in the upper seriously crusted part, woody vegetation is concentrated on the lower zone (mainly Combretaceae)..

The vegetation cover of the skirt is mainly an alternation of crops and fallow (currently the rotation is 5 years of crops and 2 or 3 years of fallow, that is insufficient to restore soil fertility, thus crop yields are decreasing (Hiernaux et al., 2009). The fallow contains mostly the ubiquitous Guiera Senegalensis , with pilostigma reticulatum and combretum glutinosum in old fallow, rather aristida mutabilis, cenchrus biflorus and digitaria gayana in the grass fallow. Crop is mostly composed by millet (pennisetum glaucum); fallow and crops include some big remaining trees, mostly the “gao”, faidherbia albida, known as a good fertility maker for the fields.

Page 1573

Line 4. A photograph of the study areas would really help the reader understand the site.

The Tondi kiboro aval basin towards uphill

The Tondi kiboro aval basin towards downhill

Line 12. How were the water level recorders installed? Are flumes or weirs utilized? More details on the measurement type and accuracy are warranted. What impact does the change in the water level recorder have on the comparison of the two periods?

Stream gauge stations were implemented in flumes at the beginning of 1991 (PICTURE); they were equipped with “Chloe” (Elsyde, Paris, France) water level recorders during the first period, and “Thalimedes” (OTT Messtechnik, Kempten, Germany) water level recorders with float during the second period. The precision of both devices is 3 mm. Measurements were done at each level change, or twice an hour. Measurements with floats and with pressure sensors level recorders were compared by the authors in experimental framework in French Southern Alps and their measures were completely consistent.

The streamgauge of the AMONT station

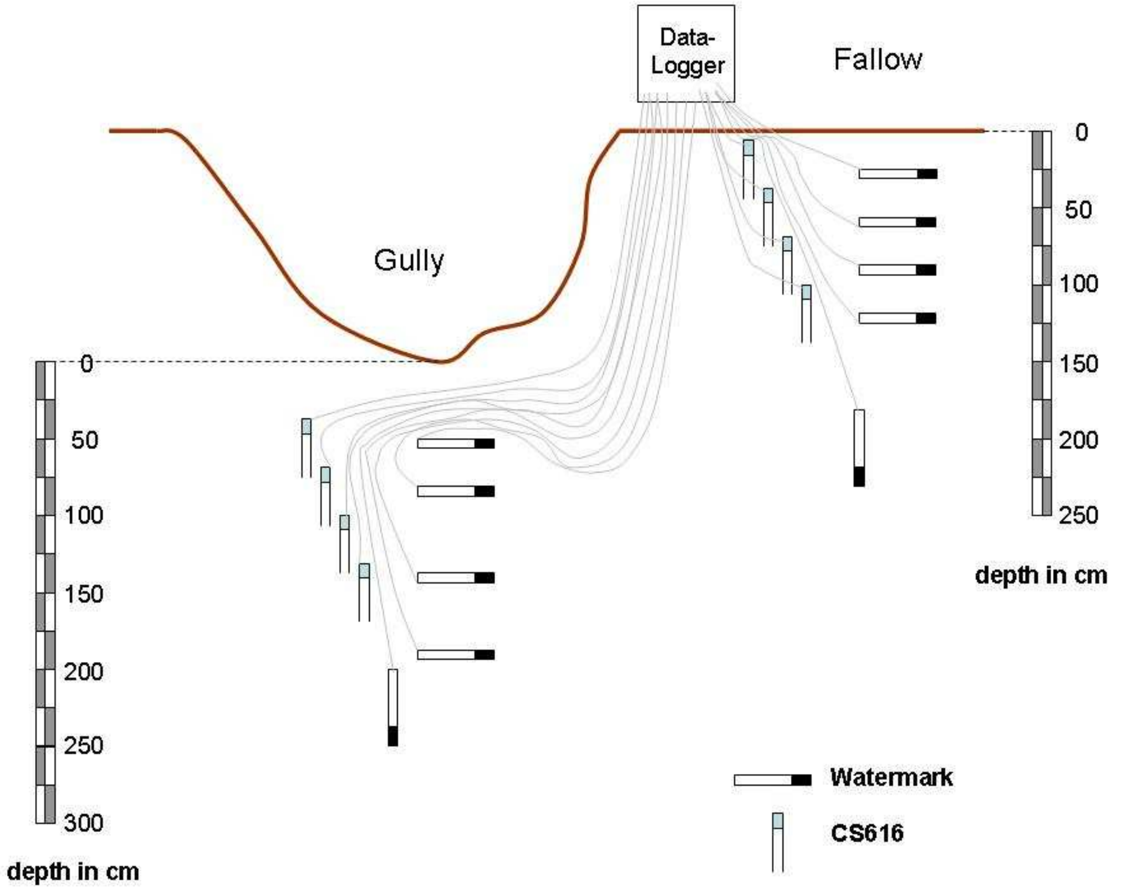
Line 22. Where were the soil moisture sensors installed? At what depths? In which vegetation types? Which specific type of sensor? How were these installed and calibrated?

- ; two types of sensors were used to monitor the soil water content of the two catchments: water content reflectometer probes (model CS616, Campbell Scientific Inc., Logan, UT), and soil water tension meters (model Watermark, Irrometer Co., Riverside, CA). Table 2 gives their location at different depths. Two field campaigns of soil gravimetric water content sampling were carried out in order to calibrate the two series of sensors at the same depths. The first campaign was made in the dry season, and the second one during the rainy season, to take advantage of a larger range of water content and soil water tension values. Based on several tens of measurements for each set, the results were found close to the calibration provided by the manufacturers (RMSE CS616 = 0.0207; r^2 = 0.65, 89 measurements; RMSE Watermark = 0.37, r^2 = 0.57, 54 measurements) (Descroix et al. 2011).

Page 1574

Line 1. Please show the soil moisture sensors and neutron probe access sites in the studyarea map. How were these sites selected?

All the sensors were automatically recorded and connected to a datalogger (Campbell CR10X Campbell Scientific, Leicester, UK) at the frequency of one measurement every minute averaged on a 30 minute basis. Fig Y gives the design of the station.



The site location is shown in fig X; it was implemented in 2005 in this double location in order to monitor the soil water content and its evolution in a site representative of the fallow as well as the gully; it allows observing the progression of wetting front in both features, significantly different.

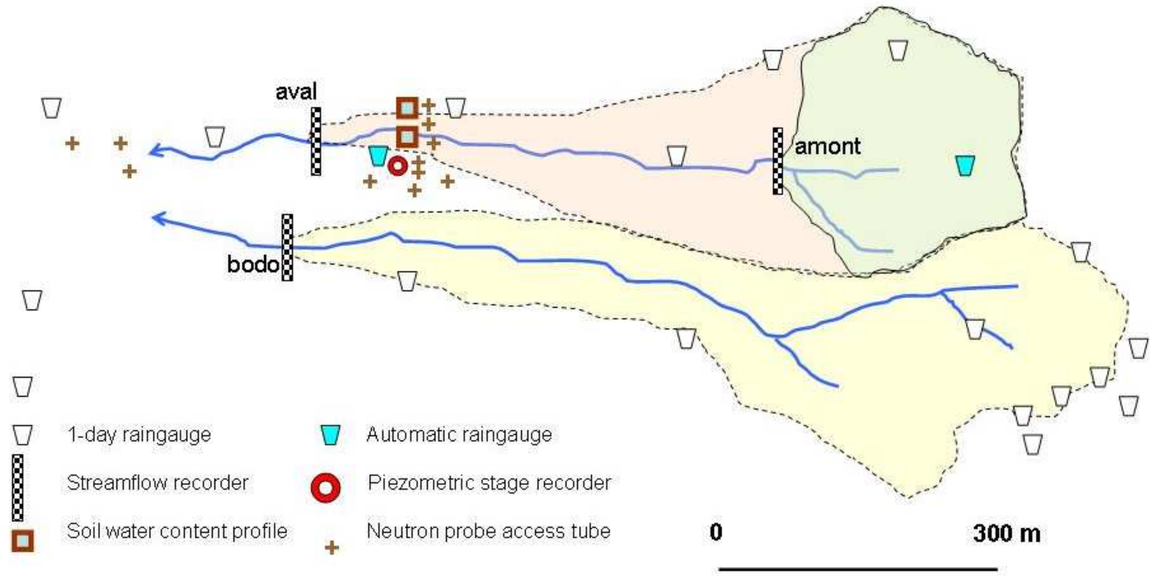


Fig X showing the location of devices

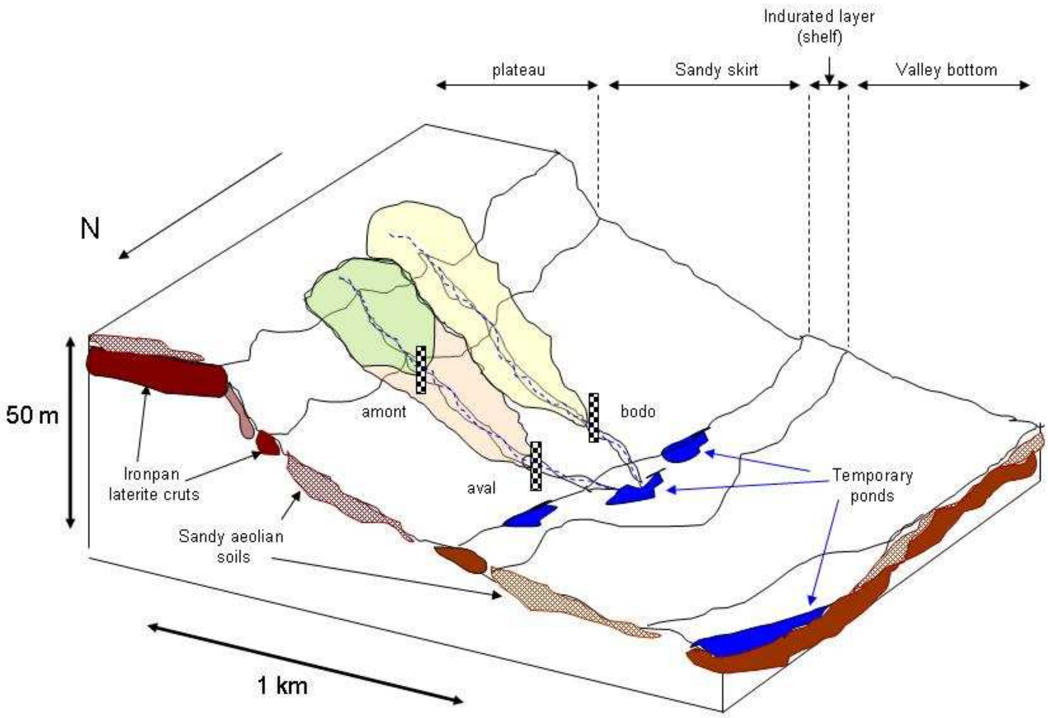


Fig T : schematic diagram of the site of Tondi Kiboro basins

Line 5. Where was the piezometer installed? How frequently were data collected?

a recording piezometer (Diver and Baro sensors, Eijkelkamp, the Nederlands) was used to monitor the water table level under the main gully (the mean depth of the water table was 46 m from the surface). Their precision is 1 mm and the frequency of measurements was 30 mn. A manual measurement was done twice a month to control and calibrate the Diver. Its location was chosen in order to observe any recharge under the gully. (see location in the map)

Line 8. Please provide more details on the sdrone. Describe how the data were processed to generate contour lines and vegetation maps. Describe the resolution and accuracy. Was field validation performed?

PIXY ® . This is a motorised drone ((http://www.drone-pixy.com/apropos/pixy.html) flying at 10-500 m high and which camera can provide pictures with some centimetres of resolution; Some objects smaller than 10 cm can be localised. However, several field campaigns were done in order to achieve the validation of the maps. Map realised using pictures of 1993 were validated with maps realised at the moment (Peugeot et al. 1997; Esteves and Lapetite, 2003). The camera used is a Canon 350 TI.

Line 11. When were the images taken? If there is seasonality in the system, how was this accounted for in the two maps? Where the two aerial images taken in the same season?

Pictures were taken each year in october at the beginning of the dry season.

Line 15. The model does not account for the 'main hydrological processes', it is a simple model with only runoff as a model predictand.

It allows defining the impact of Antecedent Precipitation Index on the stream flow. It was used here as a diagnostic tool, the evolution of parameters allowing comparing the rainfall/runoff relationship in time and space; it provided proxies of the soil current water content, of the maximum soil water capacity and of the maximum runoff coefficient. Here it is not used for modelling, rather to classify basins, years, and the impact of antecedent precipitation index (as a proxy of soil water content).

Line 17. Which variable in the model is soil moisture? Which variable in the model is the maximum runoff coefficient?

The proxies of soil water content is the API (defined in appendix A) and the maximum runoff coefficient is K_{max}.

Page 1575
Line 1-24. The description of the statistical analysis is poorly constructed. What is the motivation for using this? Is it valid to carry out for a very small number of years? Why is it not carried out at the event scale?

It is not used at the event scale because what who survey is any rupture or change in hydrological behaviour due to land use change, then it is to be used yearly (no intra seasonal changes). Clearing is processed during the dry season then there is no seasonality
As the reviewer thought there is no enough years in this analysis, it was removed; There no interest in do it at the event scale (clearly, we have the data for each second for rainfall and runoff, but the evolution of land use changes is monitored only yearly, then the study of trends of ruptures should be interesting only at this time scale.

Line 25. Prior to the results section, the authors should have discussed the instrumentation in more detail, the site characteristics and seasonality in more detail, and a few examples of the data sets collected (rainfall, soil moisture, piezometric levels, runoff).

The description was done in previous part of this current document, except for rainfall measurement:

On-site daily rainfall values per event was measured by 12 gauges, mostly by the so-called “Malian peasant” device (SIMPLAST, Bamako, Mali). On top of these manual rain-gauges, two recording gauges recorded automatically after every 0.5 mm of rainfall (model PM 3030, Précis Mécanique, Bezons, France), connected to dataloggers, Oedipe type (Elsyde) during the first period, and HoBo, OnSet Computer Corp., Pocasset, MA, the USA) during the second period (Fig. X). Every cumulative value of rainfall event was averaged by kriging, best linear unbiased estimator defined by Matheron (1963), using the two series of datasets (with SURFER-8 ® software).

Page 1576
Line 1. What are the crop rotations in the area and what is their timing relative to the natural vegetation seasonality. The differences seen could be due to images taken during different periods of the year, not necessarily decadal-scale changes.

The traditional crop system in this part of the Sahel is alternation of 3-4 years of crops and 10 to 15 years of fallow; due to the demographic pressure (population growth is around 3.5% per year for 20 years), this extensive system was degraded and now, 4-5 years of fallow are followed by 2 or 3 years of fallow, what is insufficient to maintain the soil fertility; therefore, crops yields are decreasing (Hiernaux et al., 2009a). But this constitutes the same crop system, thus crops and fallow are gathered in the map, as a system. There is no need, at the hydrological point of view, of separating crops and fallows. Otherwise, the quality of the pictures and the field validation do not let any doubt for separating fallow and crops on the one hand from degraded soils in fallows and crops on the other hand. As well in pictures as in the field, both categories are very easy to distinguish the one from the other.

Line 12. Please provide quantitative evidence that the sandy deposit increase in volume.

Sandy deposit caused a silting up of all the hydrometric stations, from 3 to 6 cm par year during 6 years depending on the location and the slope of the kori bed. Considering This represents 6 to 12 m3 of sediment per 100 m of river bed and per year. In the sole station where the comparison is possible with the first measurement period, the mean annual deposit between 1991 and 2009 is 1,8 cm.

Line 14. How much confidence is there in the statement that fallow areas or sandy deposits did not exist in 1965?

The quality of aerial pictures of 1950 and 1975 and of the Corona pictures from the second half of the sixties is widely sufficient to map the sandy deposit (an example will be given below)

Line 23. Are the differences significant in a statistical sense? The authors need to qualify their results of 'significance' with statistical evidence, here and throughout the manuscript. Some of the 'significant' changes appear to be rather small.

At our sense, due to the low number of years, the expertise of the hydrologist is more robust than any statistical test;
Although, the Student test was applied in data of table 2 (evolution of runoff coefficient between the two periods);

The probability of nil hypothesis concerning the two populations (runoff coefficient, column 4 in table 2) is:

For the amont catchment	0.067
For the aval catchement	0.36
For the bodo catchment	0.49

Only in the aval catchment, the two means can be considered as significantly different.

However, taking into account the shortening of the stream duration, the values are quite different:

For the amont catchment	0.056
For the aval catchement	0.087
For the bodo catchment	0.036

In this case, in all the basins, the probability of nil hypothesis (no statistical difference between the two means and populations) is enough low and allows considering that they have a significant probability to be different.

Line 26. The spread among the years within a period is about the same as the difference in the two periods. This lends does not lend support to the 'significance' of the change.
Page 1577

The aim of this study is compare period 1 (1991-1994) and period 2 (2004-2010) data; then the remark of reviewer has no sense.

Line 1. How useful are the runoff analyses at annual scales? Should the authors consider seasonal or event scales to increase their sample sizes and improve the significance testing?

The daily or event analysis was made at the first version, results are presented in Table 2, 5th column.
The number of events per year will be added in table 2;

Line 8. The use of the term 'runoff coefficient' here is confusing with the prior analysis. This is in fact a regression coefficient, that is not precisely a runoff coefficient. Please clarify.

Yes, we agree, the right sense of the sentence is:
At the event time scale and using the single rainfall/runoff relationship, there is a more significant increase in the regression coefficient of the relation R = a.P+ b (where R is runoff and P is rainfall), at the "amont" station (from 0.56 in 1991-1994 to 0.73 in 2004-2009) and at the "bodo" station (0.53 to 0.87) than at the "aval" station (0.43 to 0.46).

Line 13. This is the first mention of the rainy season. Thus, the system must be quite seasonal and the vegetation is likely to respond seasonally, giving more weight to the need for vegetation imagery in the same season.

It was taken at the same season (october in each year); furthermore, in this mapping, due to the resolution (some centimetres) of pictures, there is no doubt about mapped objects

Line 17. Is the decrease in the runoff duration from 28 to 18 hours possibly within the measurement error at the water level recorder? We don't know specifics of the two different measurement techniques or dataset to understand the significance of this change.

The error of an electronic watch or electronic recorder is less than one second per day or per event; If unfortunately the error should be cumulative (always in the same direction), the maximum error possible is 1 second; multiplied by 150 rainy events then less than 150 seconds, or 2,5 min, very small in front of 10 hours.

Line 21. How is the wetting front quantified? We have not been told how the soil moisture sensors were placed, so its difficult to understand the comparison made here.

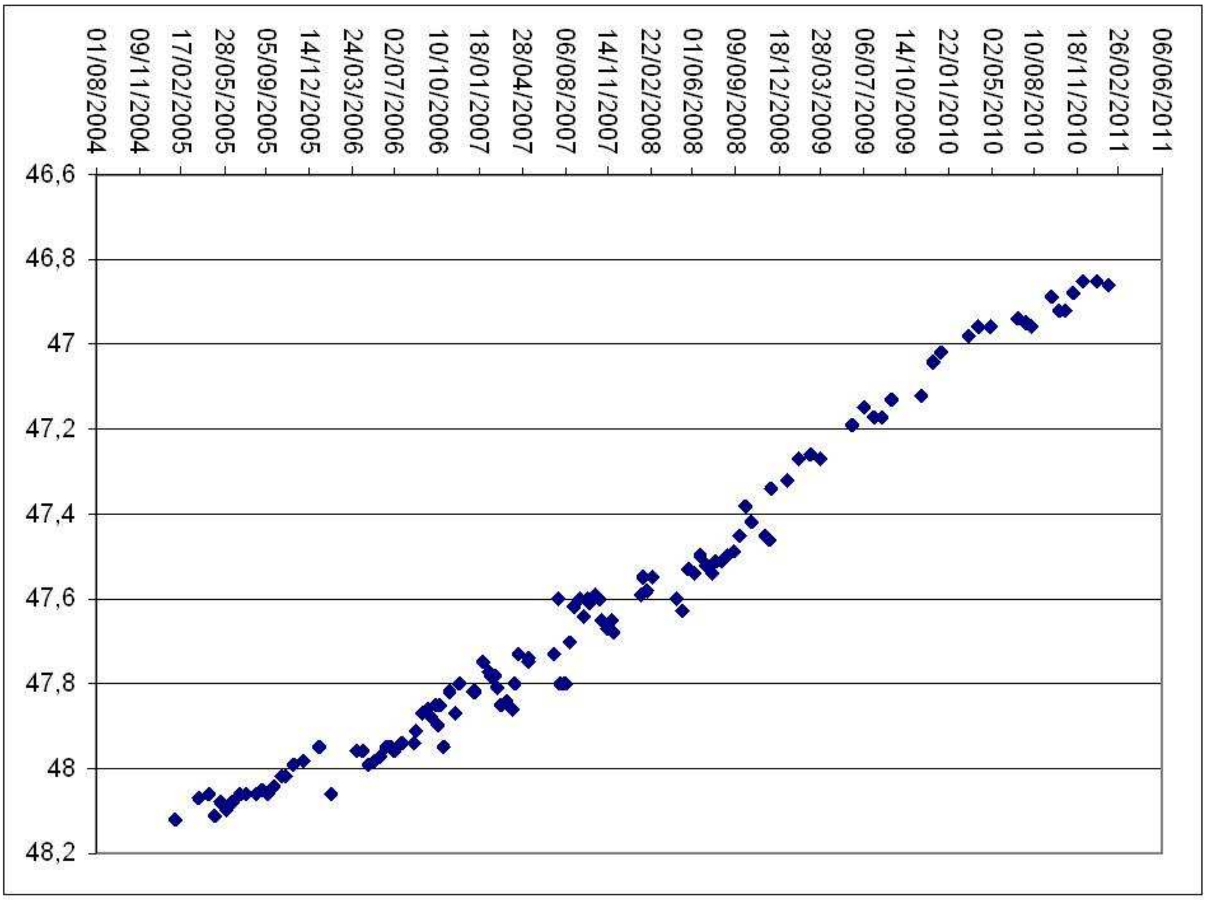
The evolution of wetting front is measured owing to the soil moisture monitoring station down to 2.5 deep, and with the neutron counting down to 10 m.

Page 1578
Line 3. The species of trees had not been discussed previously (but should have been).

Yes, this information was forgotten in the manuscript; we indicated it in this comment, above (page 3)

Line 12. It would be nice to see some of the data to be able to assess whether this statement is accurate.

This could be removed for the moment because this observation (see below the figure of rise in water table level) is not significant with the manual measurement (although it is noticeable in the 3 first years of monitoring).



Line 15. Given the simple empirical model, it is really not possible to say that it aims to 'improve modeling of rainfall-runoff relationships'.

Yes, we agree, we just used it as a diagnostic tool (see above): then the title of 3.5 chapter must be: "Using the Nazasm model as a diagnostic tool"
It allows defining the impact of Antecedent Precipitation Index on the stream flow. It was used here as a diagnostic tool, the evolution of parameters allowing comparing the rainfall/runoff relationship in time and space; it provided proxies of the soil current water content, of the maximum soil water capacity and of the maximum runoff coefficient. Here it is not used for modelling, rather to classify basins, years, and the impact of antecedent precipitation index (as a proxy of soil water content).

Line 16-23. There is repetition here with the prior model description and the Appendix A.

Yes it must be removed or shortened

Line 23. There is no Appendix B.

It has been shifted as Table B by the pdf maker ; table B of the mansucript is our “Appendix B”

Line 26. The model is referred to as NAZASM in some occasions and as NAZAS in others, please use consistent terminology.

Yes we agree; some times the final m (as model) was forgotten; it must be in place;

Line 24. It appears that the history of the events does not matter, given the subsampling of the events. How is this possible if the antecedent rainfall in taken into account?

The population of the rainy events was divided as shown, but its history was conserved, the API was continued in an independent column; therefore, YES, the history in going on being taken into account;

Line 27. Was the model calibrated manually? How? Which parameters are more important in this exercise? How are they kept with reasonable ranges?

Yes it was calibrated manually and all parameters were taken into account ; the ranges are indicated in APPENDIX B (table B in the pdf file)

Page 1579
Line 3-4. That Hmax is related to the soil degradation is conjecture. There is no support for this.

We completely agree; that is clear, it is the reason why the sentence was smoothed with the adverb “probably”

Line 5-6. That Pmax is related to infiltration under gullies is also conjecture. Please support with evidence from the model and field data.

We completely agree; that is clear, it is the reason why the sentence was smoothed with the adverb “likely”

Line 7-13. Similar comments as above for Kmax and alpha.

We completely agree; that is clear, it is the reason why the sentence was smoothed with the adverbs “possibly, probably and likely” are used 3 times;

How is it possible to use an empirical regression model to obtain any insight about physical processes at the level of detail proposed here (for specific land use types and for specific creeks). This is inappropriate.

Why ? it allows showing clear trends, comparisons and time evolution!!

Page 1580
Line 1-5. What is the purpose of this statistical analysis? It appears to contradict the earlier statements that trends exist in the data and that these are due to land cover.

We removed it, due to its too weak number of years

Line 1-5. Using these analyses for annual data is very limiting. Can you consider looking at event or seasonal scales where the sample size is much larger?

NO, because the land use changes are not seasonal, rather interannual;
YES, we agree with the reviewer who thought that there is few years, thus, we removed this analysis

Line 8-9. This is a bit confusing. It appears that runoff is the same in the two periods, but the volume divided by the duration varies. Is this not simply because the duration varied?

YES, this is an evidence of acceleration of runoff despite the strong losses in transmission.

Line 12. The changes did not seem so large.

We disagree with reviewer; for us, an increase in 100% (amont+ aval basin) or 50% (bodo basin) of the degraded soil (see table 1), whose runoff coefficient is 6 to 15 times higher than this of millet or fallow (see table 7) is at our sense, relatively important; THIS is, for us, the explanation of the general increase in runoff in the Sahel, and the cause of recent appearance of severe inundations in this area, without any significant evolution of rainfall intensity or rainfall temporal distribution.
If reviewer go on and thinks that this evolution is not "so large", then, YES, this paper MUST be rejected !! we completely agree with reviewer!

Line 14. How is it possible to use the reference from Casenave and Valentin (1989) to explain behavior that occurred later?

What is the problem ?? We frequently used edaphological or geological maps older than this reference of type of surface features and soil and rock classification did not change so quickly to impede the use of classification 20 years old ?? For us, use the Casenave and Valentin topsoil classification in the sahel is like using the FAO classification for soils types;

Line 16. Please link these two studies in separate regions to the findings here.

Sorry, these two PhD studies were realised in the same basin, and in this of Wankama, 15 km far (northward)
Page 1581.
Line 1-3. This explanation of the increasing area being responsible for the runoff coefficient greater than 100% is not well supported. Provide quantitative evidence for the reader to accept.

When there is more water at the outlet than rainfall amount, it only can be due to an increase in catchment. Since the bare soil stripe of Tiger Bush is characterised by a runoff coefficient higher than 60%, the sparse connection of a new stripe when occurred high intensity rainfall event (less than once a year in our statistical population), it is widely sufficient to increase the runoff coefficient of the basin (in its normal area) near 100% in these extreme events.

Line 4. The explanation now seems to have evolved away from land use change toward a variation in connectivity at a scale different from the land use change previously described. This is very confusing.

No land use change is clearly the main explanation; but for some few events, changes in connectivity due to land use changes (clearing in the plateau) can be added as a second explaining variable

Line 13-16. What is the relevance of the neutron probe results? Does it fit the overall theme of the study? Not clearly related.

The tiger bush is mentioned as an example; the great annual variability under gullies (and spreading areas) is due to deep infiltration during rainy season; that is not the case under millet (and fallow) where no water infiltrate more than 2.5 m, due to soil crusting (avoiding infiltration, and evapotranspiration). This explains why water table recharge cannot be due to infiltration under millet or fallow, only under gully and spreading area

Line 22. The explanation of runoff losses to deeper gullies seems speculative. Can this be shown with the data?

YES ! We observed an increase in 100% of the 2.5 hectares of newly crusted soil area (aval) where runoff coefficient is 60% instead of 10% this represents an annual volume of 6250 m^3, i.e. almost 50% of the total discharge, which is not measured at the outlet; as evaporation **during the rainy event** is negligible, we only could consider than this volume has infiltrated in the gully (as confirmed by neutron counting), as well as the discharge measured at the outlet which is completely infiltrated in the spreading area downstream of the stream gauge station.

Line 27. Did the great extension of gullies also happen at this study site?

Yes, but it only duplicate the area of the gully, rather their length.

Page 1582

Line 1-14. These references and cited values seem irrelevant for this study. If not, please link it better to the work here.

Why ? we measured comparable values in a 15km far catchment; for us, this is a good element of comparison; the cited paper was accepted with moderate corrections;

Line 18. Was a water table rise observed in this study?

Yes, see figure above

Page 1583.
Line 3. Please show evidence for the effect of soil crusting in this study site.

OK we added the Ks (saturated hydraulic conductivity) in table 7; each data of Ks is the mean of at least 15 repetitions in each surface feature.
Table 7: Runoff coefficient observed in plots (average of 4 repetitions per class, and 5 measurement years 2004-2008) (after Le Breton, 2010; Mamadou, 2010)
RAJOUTER KS

	Kr %	erosion kg/ha	Ks
millet	3,8	373	162
fallow	10,5	881	108
ERO crust	60	5566	18
ALG crust	26	863	35

Line 4. In a lumped model, parameters like Pmax are basin averaged, and thus their values cannot be attributed to particular locations such as gullies.

OK. the sand accumulation is observed otherwise (difference between aerial pictures, silting up of our stream gauges stations

Page 1584
Line 5-7. Are these volumetric differences within the measurement error?

It is more than 60% of the total volume discharged. As a hydrologist, I hope that my stream gauges station measurements and calibration give me some values with less than 20% of error

Line 8. Did the areas expand? Provide evidence.

This is the topic of the 2 PhD in achievement; just a picture to show the expansion of the spreading area of Tondi kiboro basin during the 2 rainy season 2006 and 2007 (the picture is approx 5 hectares in area)

September 13th 2005

September 27th 2007

Line 11. Is the shorter duration of the runoff possibly responsible for the higher transmission losses?

No, contrary, the higher infiltration is the cause of the reduction of the duration of floods

Page 1585

Line 6. The square root should include dn as well.

It iseems included in the original manuscript

Line 15. The variable K has not been defined.

K_n (in $mm^{-1/2}$) is a parameter depending on the soil surface hydraulic conductivity, on the catchment area and on the proportion of the catchment contributing to runoff

Line 16. What is meant by 'By assimilating the soil to a reservoir'?

Considering the soil as a reservoir, P_{0n} can be expressed as :

Line 23. Figure A1 is not referenced in the text.

OK

Technical Corrections:

Page 1570

Line 21. Period after decreasing is needed.

It is decreasing since the 1950s ;

Line 25. Most landforms are subject to erosion, thus 'erosion-caused landforms' is not an appropriate term.

OK

Page 1571
Line 2. The term biological crust is more appropriate than algal crust.

OK

Line 4. The verb 'constitute' is misused in this sentence, should be 'created'?

OK

Page 1572
Line 16. Replace 'water cycle evolution' with 'evolution of hydrologic processes'

OK

Page 1573
Line 17. Replace 'running event' with 'runoff event'

OK

Line 21. Remove 'was'

OK

Page 1581
Line 24. The use of the term 'creek' is probably not appropriate for systems that are dry 99% of the time. This occurs in other locations. A better term would be wash or ephemeral channel.

We propose "kori" which is the local (hausa originate) term for the temporary streams, as the arab "wadi"

Page 1582

Line 1. Please modify 'talking about the fields and degraded areas'. Does not make sense.

We replaced this sentence with « i.e. under the fields »

Reviewer n°2

Comments: The purpose of this research was to evaluate land cover change and commensurate changes in runoff on two small watersheds in the Sahel from 1990 to present. Some aspects of the work are interesting but unfortunately the results are not presented or interpreted in a particularly compelling fashion and as a result the manuscript, in its current form is primarily a case study that confirms what is now quite a well documented hydrological change in the Sahel in response to changing land cover. The work may be publishable but not in its current form. In order for the work to be publishable the authors need to make a much more compelling case that new and meaningful information has been generated from this project.
My recommendation would be a major revision that focuses on clearly articulating a basic problem and knowledge gap that this paper identifies.

The increase in runoff in the Sahel was highlighted effectively by Albergel in 1987.
We pretend here:

- show that the increase in land degradation and thus in runoff is going on nowadays
- show that from field measurements, comparing two data sets (1991-1994 and 2004-2010); it is the first study in this area of Western Niger well known owing to international scientific program (HAPEX Sahel, AMMA) that the increase in runoff is shown by runoff measurement instead of model or proxis such as the filling of ponds which catchment area, as it is well known, can vary of 1 or 2 orders of magnitude between two seasons, between two rainy events, and during a rainy event.
- Show that the increase in crusted soil in the basin in the first cause of increasing runoff

There have been several very strong reviews on the topic in the last couple of yearsâ˘A˘ Tmost notably Favreau et al. (2009). This might be a good place to begin. Using that paper and others summarize the basic phenomena. Ideally these review papers will have identified some key knowledge gaps that the current manuscript is addressing. Once the key knowledge gap has been identified one then has a strong basis of the research objectives of this project.

We agree with both the fact that aims (and new pretended advances in science) are not well presented and that Favreau et al 2009 is a good synthesis.
The gap in knowledge was in the field measurement of the increase in runoff.

We also show that in the two last decades, land degradation still go on in this part of the Sahel, in the catchment, not only by changes in drainage density which is a consequence, not a cause, of land degradation

The paper of Favreau et al is a very good synthesis of the evolution of water cycle in last decades in endorheic areas; but it only presents a case study of the square degree of Niamey (a part of the Sahel where water table is rising), not for the whole Sahel. The rise of water table in the square degree of Niamey is a local process, whose wide spreading is not yet known; at our knowledge, the other cases of rise in water table in the Sahel are punctual and specific cause linked : Niayes of Dakar due to urbanisation, some local examples in central Burkina plateau and Keita area in Ader (Niger) region, where there are (likely) due to land reclamation.

In fact,, the increase in runoff is observed in the (almost ?) whole Sahel (Mahé et al, 2003, Mahé et al., 2005, Mahé and Paturel, 2009, Mahé, 2009, Descroix et al 2009, Amogu et al., 2010); After having shown these processes at the regional scale (the last two papers) our aim is to show this process at the local scale in one of the AMMA experimental sites, based on field runoff measurement, not on proxies as the rise in water table. Favreau et al, 2009 well showed the link between land degradation and rise in water table but no data of runoff in the field with any correlation with land degradation and soil crusting and physical properties of crusts that explain the increase in runoff.

The current Introduction, to my mind, is quite disjointed. It bounces around from a brief and not very compelling overview of the issue to the Boserup theory to re greening of the Sahel. All of these topics may be of importance but it is not clear to the reader exactly how they are connected.

We agree and we removed this part of the introduction

The objective statement may be ok but since no fundamental knowledge gap has been identified it is not clear to me that this work is much more than a case study confirming what we already now know.The Results are quite straight forward but it is not clear to me what additional insight was provided by the modelling analysis.

It is the first paper since Albergel 1987 that shows by field runoff measurements that runoff is increasing; the additional insight (compared with Albergel) is the fact that, after 40 of rainfall deficit, land use go on in degradation, with as a consequence, an increase in runoff, here measured in the field and directly in the stream.

The modelling part has been re-entitled "Using the NAZAS model as a diagnostic tool**";** effectively, we used it to classify spatially and temporarily the sites or the events according to the main hydrological behaviour.

I would then rework the Discussion focusing on what new information has been gained from this study and how might it be applied across the region. What do we now know that we did not prior to this work.

Now we know the following facts, only based on land observations and measurements:

- the land degradation still goes on in some parts of the Sahel in the last two decades, contrary of the re-greening shown in other areas in recent papers;

- runoff still increases at the local scale (it has been shown at the regional scales in descroix et al., 2009 and Amogu et al., 2010)
- soil crusting is the main explanation to the increase in runoff.

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