1. Main comments

Shortcomings in data analysis and methodology

Problems in Sahelian/Sudanian AMAX series

The definition of Sahelian flood that is used to build the AMAX series is problematic. Sahelian flood peak values are influenced by river flows coming from the Guinean catchment. The discharges coming from Asongo have their own interannual variability which contributes to the interannual variability of the Sahelian flood peak values (visible in Figure 10). As a first consequence, each year the magnitude of the Sahelian flood peak is influenced by the upstream discharges. A second consequence is that years where the Sahelian peak cannot be distinguished in the hydrographs should not be considered as gaps as made by the authors. By ignoring these values the authors produce a bias in the AMAX series analysis that prevents the conduction of a reliable study of trends and attribution. My comment also applies for the Sudanian floods.

The authors should rework on the definition of the Sahelian/Sudanian floods by quantitatively evaluating the contribution of the Guinean discharges on the Sahelian/Sudanian flood peak values. A suggestion could be for instance to directly study the data of the Sahelian tributaries (if available), or to separate the Guinean discharges from the Sahelian discharges by substracting the Asongo and the Niamey discharges - which would necessitate to take into account the transfer effects in the river bed from the upstream to the downstream stations.

About Attribution:

Overall methodology

The authors state that precipitation is the “main driver” of the recent changes in flood regime and that land-use change is of “minor” influence. There is however no quantitative arguments to support this statement. The framework used by the authors to attribute trends is a data-based approach within a hypothesis-testing described in Merz et al. (2012).

- A description of this methodological framework and how it is applied for the purpose of the study is missing
- According to Merz et al. (2012) a condition to the hypothesis testing framework method requires “three ingredients of attribution: evidence of consistency, evidence of inconsistency, and provision of confidence level.”
    - The authors show evidence of consistency between increase in precipitation and AMAX (annual and heavy precipitation) but they do not prove any physical link.
    - It seems that the authors try to find evidence of inconsistency between land use and the recent evolution of flood peaks. However the argument used to minimize the effect of land use is based on a detrended runoff coefficient that is computed with a very questionable method (see below).
    - Neither quantitative indicators nor provision of confidence levels are provided that would justify assigning a “main” or “minor” contribution to each factor of influence.

As a consequence, the reasoning used to attribute the increasing trend in flood peaks to rainfall more than land use change is weak. At best the authors can formulate hypotheses that rainfall might have

contributed to the recent changes in the Niger flood regime. As this has already been pointed out by several other authors (e.g. Lebel and Ali 2009; Panthou et al. 2014), it diminishes significantly the value of the paper.

Lebel, T., and A. Ali, 2009: Recent trends in the Central and Western Sahel rainfall regime (1990-2007). Journal of Hydrology, 375, 52–64.

Panthou, G., T. Vischel, and T. Lebel, 2014: Recent trends in the regime of extreme rainfall in the Central Sahel. International Journal of Climatology, doi:10.1002/joc.3984.

Runoff coefficient

The runoff coefficient is computed by dividing detrended discharge series (AMAX or annual) by precipitation (not clear if it is annual or heavy or both). By doing so (detrended series of runoff/detrended series of precipitation) I do not see any reasons to expect a trend in the obtained coefficient. If these reasons exist I do not understand how it could relate to land use more than rainfall. This absence of trend is however the only argument given by the authors to justify that land-use change plays no-dominant role in the AMAX.

The hydrological meaning of a coefficient defined by the ratio between the annual daily maximum flood peak and the annual rainfall is not clear to me. The runoff coefficient is most often used to understand the rainfall-runoff relationship on small catchments at an event based scale. Its computation at annual scales to analyze the evolution the rainfall-runoff relationship over mesoscale catchment (>10000km^2) is very questionable.

Scale issues

More generally the use of annual scales to identify hydrological processes in the region is very questionable. Runoff production in the region largely depends on the occurrence and the intensity of the convective systems that produce the majority of the rainfall. Trends in annual rainfall are thus not suitable indicators for analyzing trends in annual maximum discharge. An increase in annual and even on heavy precipitation daily rainfall can be reflected in different manners: it can be produced by changes in occurrence of the event or change in the intensity of the events. In the Sahel, where runoff is almost exclusively of Hortonian type (infiltration excess runoff) the hydrological response of the catchments are very sensitive to intra-event rainfall intensities. An increase of rainfall intensities will effectively accentuate the runoff production and might contribute to maximum flows, however a change in occurrence can be reflected linearly on runoff without modifying the discharge frequency distribution. The confrontation of trends in rainfall (annual and heavy) and AMAX as done by the authors is thus not a demonstration of the role of rainfall in the increase of AMAX. The response to the question lies in a better documentation of how rainfall intensities within the rainy systems have changed during the last decades. This necessitates studying rainfall trends at sub-daily time scales and at spatial resolutions lower than the regional catchment scales proposed in the study - which I recognize is not an easy task.

The paper is too ambitious

Documenting comprehensively the flood risk and the reasons for its recent evolution over the whole Niger basin is very ambitious and cannot obviously be made in one single paper. Some aspects are thus addressed only in a superficial way or with very uncertain data.

Value, vulnerability

In particular the analysis of the value, vulnerability components is quite weak. It relies on a dataset of very low quality (as recognized by the authors). The link between floods and the increase of affected people is not demonstrated. A simple correlation between flood and the number of affected population is not a demonstration of causality. The vulnerability component (adaptation strategy, societal dynamics during floods, …) is not studied. This makes the contribution of the paper on this aspect quite low.

Climatic attribution

The authors propose to link AMO and AMAX. What is the objective here? It seems in the conclusion that the authors want to provide operational tools for dam management. The West African Monsoon is a very complex system that results from both oceanic and atmospheric structures interacting at various space and time scales. This complexity explains why rainfall variability in the region is so difficult to understand and model. Why only using AMO as indicator? What about other atmospheric structures (Saharian heat Low, Easterly Waves, Madden Julian Oscillations,…) that have been demonstrated as major factors of influence of rainfall variability? The hydrological processes also add a lot of complexity in between oceanic/atmospheric synoptic structures and river discharges. Thus the development of statistical link between large scale structures and AMAX cannot be treated as a small part of a paper about the flood risk. To me this question of climatic attribution is off-topic in the present study.

Difficulties to follow the overall reasoning

Probably because the paper is too ambitious, it is difficult to understand the logical approach used to address the paper issues.

1. The scientific questions are not clearly stated.

2. Some details about the overall methodology are missing:

- details on the data-based approach within hypothesis-testing and how it is applied for the specific study
- Section 3.2 provides a list of statistical methods but their usefulness and relevance for the overall reasoning is not explained.

3. Distinction between data analysis (trend), attribution analysis and discussion is not clear. The three elements are sometimes mixed all together. The analysis of one result is sometimes scattered across several sections with sometimes new elements that can contradict the previous ones. The result scattering also produces a lot of redundancy in the paper.

2. Detailed comments

p. 5172 l. 23-24 what are the “both factors”?

p. 5177 l.24-25 It seems from Figure 1 that Malanville does not intercept the whole Sudanian catchment. Is the Malanville station relevant to study the Sudanian contribution to flood.

P. 5177 l. 25-28 The definition of the Sahelian/Sudanian floods is very problematic (see the main comments).

P. 5178 – 5182 Section 3.2 Statistics. This Section is a listing of statistical tools often disconnected from the purpose of the study. Please explain more clearly the purpose of using such statistics methods. To which dataset are they applied? How do these tools contribute to the questions addressed in the paper? This is sometimes done like in p. 5179 l. 18. More generally excepting the (too) short paragraph in introduction (p. 5174, l.9 – 26) the methodological approach used is not detailed. This makes the reasons of the use of the list statistical tools very difficult to understand.

p. 5178 l. 21 "3.2.1 Standard.."Why are the listed methods considered as standard compared to chang point , wavelet or frequency distribution analysis. Please find a more appropriate title.

p. 5181, l11-12. What can justify the time-dependence of the location and shape parameter, while the scale parameter is constant? This is quite puzzling as in practice the shape parameter is often very difficult to estimate reliably.

p. 5182 Section 4.1 Analysis of damage statistics

- The results largely depend on the capacity of medias to report the floods. Intuitively, I would argue that the increasing media and communication facilities during the last thirty years might explain a part of the increase in reported damaging floods. Moreover one might expect more reports in urban areas than in remote villages. So how far can we reliably consider that media reports and official sources can provide an homogeneous flood damage database in time and space? How does it impact the results?
- The discrepancies between the three databases (p 5183, l. 17-19) show that some reports can be missed which highly questions the reliability of the reports and thus of the results.
- At several places the general term "flood" is used although the documented floods are those reported in the database as damaging. Please use an appropriate denomination to avoid confusion.
- p. 5183 l.3 and Fig 1. The distinction between river flood and flash flood is not clear to me. How do they differ? River floods are reported in endoreic regions (North Niger and Mali). How can this be explained?
- P. 5183, l12 to the end of the Section 4.1. What rainfall and AMAX have to do with flood damage analysis? The correlation analysis at the end of the Section suggests that the authors try to find a causal connection between hydrological variables and people affected by floods.
    - Then why rainfall and AMAX are analyzed before 1980?
    - The AMAX data only represents the flood hazard at the outlet of the catchments which corresponds to river floods while people are affected by both river and flash floods. This may bias the correlation.
    - Some other factors may also explain an increase of affected people as the population growth rate for instance as discussed in Section 5.1.

- To me, this section should be only focused on flood damages description. Rainfall and AMAX trend analysis should be done in a separate section. The link between rainfall, AMAx (other?) should be exclusively carried out in the discussion (this will avoid redundancy in Section 5.1).

P. 5184 It is not clear in this subsection which flood (Guinean, Sahelian or Sudanian)is studied at each station. Please clarify.

P. 5185 l.10-17 This should be explained in Section 3.2.4

P. 5185 l.18-19. P. 5186 l. 1-3 What is meant by "most suitable", "sufficiently complex"? Please provide quantitative elements to justify.

p. 5185 l. 7-11 The wavelet analysis does not verify the results of the NSGEV as it does not help analyzing changes in the location or the asymptotic behavior of the AMAX distribution. However it seems to justify (too late in the paper) the use of a constant scale parameter. If it is the only objective of using wavelet analysis, it should come earlier in the paper (before section 3.2.4).

p. 5186-5189 Section 4.4 the title is not appropriate since this section does not discuss the attribution issue: 4.4.1 shows trends in some indicators but does not provide attribution analysis, 4.4.2 describes the link between AMO and AMAX which is a bit off-topic 4.4.3 shows that the Sahel Paradox ends after the 1980s. This section should be reorganized with the discussion or renamed.

p. 5188 l. 10-14 + Figure 10 This should be moved in Section 3.1.2

p. 5189-90 Section 5.1 A lot of redundancy here: literature revue then synthesis of the results then some additional analyses about trends, links between variables…what is the real objective of this section?

p 5190 l. 9-10. It seems that you do not consider that a change in GEV location parameter is not a change in flood regime. Then could you explain what do you mean by flood regime?

p 5190 l. 1415 It is wrong to write that "This holds for the Sahelian….Niamey" as Sahelian and Sudanian AMAX distributions are characterized by a constant shape parameter which differs from the Guinean AMAX distribution.

p. 5190 l. 16-18. Where are the scientific elements allowing you to state that: (i) "AMAX magnitudes…in all regions", (ii) "The trend is significant", (iii) "strongly correlated to the AMAX"?

p. 5190 l. 24 – p 5191 These are interesting hypotheses of explanation but they are very difficult to prove. This cannot be treated properly in this paper.

p. 5191 l.11-12. Correlation between AMO and AMAX goes from a fair "moderate" (p. 5188 l.1) to an exaggerated "high".

p. 5191 l. 17-25 Redundant with Section 4.4.3

p. 5192 l.4-6 This argument does not hold. The use of a detrended runoff coefficient cannot help dissociate the effect of land-use change from the effect of rainfall regime changes. Thus it cannot be stated that land-use change plays no dominant role in the increase of AMAX from the runoff coefficient analysis. See my main comments.

p. 5192 l. l6-13 This argument does not hold because of rainfall scale issues. See my main comments.

p. 5193 l. Correlation between AMO and AMAX goes from a fair “moderate” (p. 5188 l.1) to “high” (p. 5191 l.11-12) to “strong” here. Please do not oversell your results.

p. 5193 l.16-19 This is probably the only way to address the issue of hydrological attribution. This is however not an easy task.

3. Minor editorial comments

- P. 5175 l. 23, 5176 l. 2 and l13 Referred figures do not correspond to the purpose + reference to Fig. 6 while Fig. 3, 4, 5 have not been cited yet.
- P. 5204 caption replace 1985 by 1980
- Figure S1 annual discharge or annual precipitation?
- P 5179 l.23+ reference to Fig. 7 while Fig. 4, 5,6 have not been cited yet.
- P. 5185 l. 8 “…could *xx* a significant..” *xx* Missing word.