Note: The original comments by Referee 2 (R) are in regular text. Replies by the authors (A) are colored in green and changes in the text are in italics.

R: The paper addresses a very challenging issue about the recent evolution of flood risk in the Niger basin. The authors attempt to provide a comprehensive overview of flood risk by documenting damages, hydrological hazard and to attribute it to land use and rainfall changes. Other aspects as climatic attribution are also considered. The authors made a serious effort to gather and confront different available databases (damages, rainfall, discharge) in a region where data access is notably difficult.

The paper provides some original elements that contribute to the documentation of trends in hydrological variables in West Africa- for instance by studying series of annual maximum discharges. However the subject treated here is too broad and too complex to be addressed completely in one general paper. Moreover, I am really not convinced by some of the results. In particular, several major statements made by the authors about hydrological attribution are not supported by the data analysis and the methodology I provide in the attached pdf the main reasons for my position and detailed comments to add to the interactive discussion.

A: We thank the Referee very much for the constructive comments and the very helpful suggestions how to improve the manuscript. We share the view that the paper was ambitious and in some places too ambitious. Therefore, we focused the scope of this paper by defining two distinct research questions, one on the attribution of the flood hazard and the other on the link between precipitation variability and the flood risk. We restructured the paper completely by removing chapters which not directly support the general aim of the study as suggested, and by deleting unnecessary information and analysis which did not directly support or further expand the two new research foci. The discussion was completely rewritten, focusing on the two research questions; this resulted in a considerable condensing and streamlining of the article, which, - we agree -, was previously too broad for a single paper. Accordingly the conclusions and the abstract have been renewed.

We followed and discussed all main and detailed comments of the referee and addressed all mentioned shortcomings. The technical suggestions helped to make the analysis more reliable. Finally the quality and understandability of the paper improved significantly.

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 subtracting 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.

A: We agree with the referee on the methodological shortcoming by using the Sahelian time series with gaps for statistical test. The suggestions of the referee on using time series of tributaries in the Sahelian basin were implemented to overcome this shortcoming; we would like to thank the referee for this help.

Therefore two new gauging stations at the Sahelian tributaries to the Niger River, which are not influenced by the Guinean flood and therefore have no gaps, were used for an additional statistical analysis: we identified two stations near the tributaries Sirba (Garbe-Kourou) and Gorouol (Alcongui) and redid the analysis. Although there were no complete time series available from the two stations, it was possible to read the AMAX times series from publicly available annual hydrographs from the website of the project Nigerhycos of the Niger Basin Authority (http://nigerhycos.abn.ne/, 2014). The time series of the tributaries have been used to redo all statistical tests including trend, correlations, NSGEV, and wavelets analysis

For the Sudanian region, please see the answer to detailed comment P. 5177 l. 25-28.

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.

The referee is right that the data-based attribution approach cannot quantitatively prove the minor influence of land use change on the increasing AMAX trend. We also agree that the methodological framework has not been clearly defined and added therefore a new chapter in the methodologies explaining the attribution framework. According to the framework of Merz et al. (2012), we show the consistency of changes in precipitation with changes in AMAX. The method we used for testing the inconsistency of the land use change signal with AMAX has been removed (please see answer to comment “Runoff coefficient”). However the analysis of flashiness as proxy for land use change influence still shows an inconstancy between the land use change signal and the AMAX. The analysis of flashiness shows an increasing trend starting in the 1960s. This influence of land use change is said to lead to an increase in direct runoff and thus river discharge (e.g. Descroix et al. 2012). For the Guinean and the Sahelian region the decreasing rainfall trend lasted until the 1980s. In both regions this decrease in rainfall had more influence on the AMAX compared with the land use change since this increasing signal starts in the 1960s. This is discussed in detail in the discussion chapter. Merz et al. (2012) distinguish between a hard and a soft attribution. Following this terminology during the provision of confidence level we state that the criteria for a hard attribution are not completely fulfilled and communicate the limitations of the attribution clearly. In agreement with the referee we argue for a simulation-based attribution study in order to quantify the share of influence on the changing AMAX and to gain more confidence in the attribution.

We are fully aware that previous articles already dealt with changes of the Niger’s rainfall regime (“return to wet conditions”), and we have added the new references including Panthou et al. (2014) to the introduction section of our work. However, to our knowledge, no study analyzed so far the relationship between the described precipitation changes and the resulting discharge regime across the Niger basin with a focus on flooding, except the study by Descroix et al. 2012. And Descroix et al. hypothesized that there is no relationship between changes in rainfall and discharge and that land use change is the only influencing effect – which we proved in this paper to be not correct. Therefore, we think that this study brings in valuable new insights on the flooding regime of the Niger basin.

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 landuse 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 (>10000km²) is very questionable.

A: The coefficient has been computed with the detrended AMAX and the detrended precipitation. The hypothesis is, that if there would be a trend influencing AMAX beside the positive trend in rainfall, it should become visible in the coefficient when removing the trend from the rainfall time series and AMAX. However the referee is right that the efficiency of this method cannot be proven in this paper and we agree that it cannot serve as prove or indicator. Therefore we removed the analysis and related conclusions from this article and restructured the discussion accordingly.

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.

A: We agree with the referee: this study cannot differentiate between changes in occurrence and intensity of rainfall on a sub–daily level since the only observation data set available to the authors is on a daily level. Detailed analysis of change in precipitation patterns in the region have been cited in the introduction of the article (e.g. Panthou et al. 2014, Lebel and Ali 2009). In addition the strong correlation for all regions between AMAX and both precipitation measures used is justifying the use of the data to some extent (Table 2). Still the point is mentioned more clearly that in this paper we cannot analyze the role of the rainfall intensities since the signal is hidden on the daily basis and only analyze general precipitation trends, on which we base the discussion.

Discussion: *"Though since changes in annual and heavy precipitation are consistent and the timing is similar, it is not possible to distinguish quantitatively the influence of heavy precipitation on AMAX. Therefore the role of the increase in rainfall intensity cannot be adequately discussed."*

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.

A: The shortcomings in the value and vulnerability have also been mentioned by referee 1 and we agree. We restructured the discussion and added definitions. In addition the limits of the analysis as also recognized by the referee deriving from the low quality of the data have been stated more clearly. Please see changes in the discussion and in the conclusions. Still the used data is the best data for the impacts of catastrophic floods in the Niger basin. The objective of the paper is, however, neither a focus on vulnerability nor the value. This is included into the paper to complete the picture and show that flood risk is only partly determined by the hazard and that other components are as important.

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 asmall part of a paper about the flood risk. To me this question of climatic attribution is off-topic in the present study.

A: We agree with the referee that the relation of AMAX to other teleconnections cannot be sufficiently analyzed in this paper. The analysis of the link between AMO and AMAX can only be a part of a broader study and do not contribute to the overall aim of this paper. Therefore it was removed. The correlation to the AMO is now only used to show consistency between the AMAX and the climate in regard of the decadal pattern.

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.

A: We agree with all three points of the referee and improved the manuscript accordingly. The scientific questions have been stated clearly (see first paragraph above) and the methods chapter has been extended and reorganized as suggested. In addition the other chapters of the manuscript have been restructured according to the comments of both referees with a focus on removing redundancy and making the study more consistent and clearer to understand.

2. Detailed comments

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

A: Our apologies, the sentence was a relict and the factors value, vulnerability and hazard are now later mentioned and explained in more detail, also according to a comment of referee 1.

p. 5172 l. 23-24: *however, very little research is currently being conducted on the factors contributing to flood risk and the associated flood damages.*

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).

A: The problems of the definition of the Sudanian catchment have also been commented by referee 1 and we already discussed it in the manuscript. We agree that it is problematic and follow the suggestion of referee 1 to remove the region from the analysis. All the relevant sentences have been deleted or changed. In addition, as stated by the referee 1, the results of the Sudanian region were very similar to the Sahelian region. This supports a broader interpretation of the Sahelian results also for the Middle part of the Niger to some extent. This has been considered in the restructured discussion.

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.

A: Referee 1 also suggests moving the methodological parts (P 5174, l9-26) from the introduction to the methods. Therefore we extended the method by explaining the purpose etc. for each method.

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.

A: We change “Standard” to “Basic”.

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.

A: We corrected this mistake: the expressions shape and scale have been mixed-up here and in the results chapter (the scale parameter was not constant in the analysis whereas the shape parameter was, as specified in Delgado et al. 2010.

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?

A: We agree with the referee that there might be a bias in the media coverage. However in Tarhule (2005) the authors evaluated newspaper reports on flooding between 1970 and 2000 and came to the conclusion that reported flood events in the Sahel “are broadly consistent with rainfall conditions in the Niamey area, which is a tribute to the quality of environmental reporting at the newspaper”. This does not prove that there is no bias but it gives more confidence to the numbers of the reports. In addition the locations of reported catastrophic damage are relatively homogeneously distributed along the river and its tributaries and clustering is limited (4.1, Figure 1)- This implies that a potential bias of urban and remote villages, which certainly can be assumed to a certain degree, would affect all regions and the relative numbers should be more reliable. However, we added these points of uncertainty in the discussion more clearly.

p. 5177, l. 8: “Another open question concerns a bias in media coverage, which might have increased during the last decades and could result in an increasing number of flood reports. Tarhule (2005) addressed this issue for the region around Niamey by comparing flood reports in the media with rainfall data and concluded that the quality of the environmental reporting of the newspaper is good in the Niamey region from 1970-2000. This cannot rebut the hypothesis of changes in media coverage however as all data sets rely mainly on newspapers if gives more confidence on their consistency during the last decades. Another aspect of the media coverage bias is the better coverage of urban areas compared to rural areas. However, as we do not analyze the spatial distribution of the flooding on the subregional scale, this bias does not affect the analyses directly. In sum, the datasets of people affected by floods are uncertain and should be interpreted with caution. Still, they are the best available source for damage data on catastrophic floods in West Africa. The data was analyzed equally, and so even if absolute numbers are uncertain, trends in the data are assumed to be reliable."

- 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.

A: Please see the answer to the comments on the previous question and the relative changes in the text, concerning the quality of the damage data. In addition all three reports agree mostly on the years with less than 1000 people affected by floods and for most of the years at least two of the reports in changing combinations agree roughly in the amount of people affected by floods. This gives more confidence to the data, especially when not looking at absolute numbers but trends.

- 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.

A: We thank the referee for this advice and changed the expression flood in the manuscript where it could be misinterpreted.

- 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 endorheic regions (North Niger and Mali). How can this be explained?

A: 1. This point was also mentioned by referee 1 and we added the definitions.

2. Floods in in Northern Niger or Mali where "regular floods" or a "river floods" have been reported, are probably lying near a riverbed which is only flowing periodically like typically Wadis. Unfortunately there is no more information from the databases then the discrimination between flash floods and regular/river floods.

- 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?

A: Rainfall and AMAX are analyzed before 1980 because the paper aims on understanding the relation between the flooding process and the climate variability which can be seen clearer in longer time series. This was not clearly expressed and the methodological framework has been extended therefore. As damage data is not available for the period this cannot be included the analysis 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.

A: For the correlation only data of river floods have been used. This has been clarified in the text.

P. 5183, l.6: *"For the trend and correlation analysis only the data on people affected by river floods have been considered."*

- Some other factors may also explain an increase of affected people as the population growth rate for instance as discussed in Section 5.1.

A: This is one of the main points in the discussion that not only the hazard determines the flood risk but also value and vulnerability. This has been included in the discussion and stated more clearly.

- 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).

A: We agree with the referee and followed the suggestion. An additional chapter on the general flood dynamics has been added and the link to the damage statistics is exclusively in the discussion.

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

A: The explanation has been added and the legend of Figure 5 has been clearly stated that both plots show the Guinean flood, on for the stations before the IND (left) and the other for the stations in or after the IND (right).

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

A: We agree and added the paragraph to the methods.

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

A: It means that the Chi-square test showed a significantly better fit of the model compared to a less complex model or constant values. The sentences have been changed for better readability.

P. 5185 l.18-19: *For the Guinean region, a model with a constant scale parameter but a third-degree location parameter has the highest value in the Chi-square test and is therefore most suitable for explaining the distribution of probabilities for the AMAX time series.*

P. 5186 l. 1-3*: The analysis suggests that linear models have the best fit are sufficiently complex to explain the dynamics of AMAX during the period analyzed, from the beginning of the dry conditions around 1970 until 2012.*

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).

A: The wavelet analysis was done in order to supplement the NSGEV analysis by showing the distribution of frequencies over time, which cannot be seen directly from the NSGE plots. Since there is no additional information in this case the figures have been placed in the supplement however they back up the interpretation of the NSGEV. In addition, the scale parameter was not kept constant but the shape parameter (as explained in the answer to comment p. 5181, l11-12).

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,

A: Please compare to the answer of the main comment at the beginning of this response.

4.4.2 describes the link between AMO and AMAX which is a bit off-topic

A: Please compare to the answer of the main comment. We agree and removed the paragraph.

4.4.3 shows that the Sahel Paradox ends after the 1980s. This section should be reorganized with the discussion or renamed.

A: We agree and distinguished between the part about the analysis of the Sahel Paradox is described and the part which belongs to the attribution.

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

A: We agree and moved the figure and the paragraph to the suggested section.

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?

A: This was also commented by referee 1. The discussion was accordingly restructured with stronger focus and finally a broader analysis of the implications of the results.

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?

A: The terminology of flood regime was misleading, as also mentioned by referee 1, and was changed.

p. 5190 l. 9-10: *The increasing AMAX in all four regions is not connected to a change in the flood probability distribution 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.

A: We agree with the referee. Since with the additionally considered time series of the Sahelian tributaries the results changed, the paragraph was updated.

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”?

A: (i) With “all regions” we refer to the analyzed regions in the Niger basin. This has been clarified in the text.

(ii) In paragraph 4.3 we present the results of trend analysis and all AMAX time series show a significant trend on which we refer here in the discussion.

(iii) The statement is partly wrong as only for the Sahelian and the Benue region the correlation is strong. For the Guinean region it is weak. This has been corrected in the text and the discussion has been extended in this aspect as also suggested by referee 1.

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.

A: We agree that it is very difficult to prove the hypothesis that the geomorphology in the Benue and the Guinean region are more adapted to wetter conditions like flood plains and wetlands. However it is broadly accepted that in regions where the morphology is adapted to dry conditions, the drainage of water is less efficient and in the same time more water can be drained or stored in regions with a more adapted morphology with broader river beds, wetlands, ponds etc. However we removed the argument since a sensitivity analysis of the subregions as done in Aich et al. 2014 would be only possible with a model.

The other hypothesis about the stronger increase of population and the traditional knowledge of strategies for flooding are backed by numbers and existing literature (Tschakert et al. 2010, Di Baldassarre et al. (2010). Therefore we argue for mentioning these observations but also stating clearly that they are hypothesis which cannot be proved but there are indications which back them.

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

A: Since we followed the suggestion of the referee to remove the AMO analysis this point is not relevant anymore.

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

A: We agree that there is redundancy which was also pointed out by referee 1. Therefore we restructured the discussion. Please compare with answer to the second main comment of referee 1.

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.

A: We agree that we cannot prove the efficiency of the method and removed the analysis and we also removed the statement on effects of land-use change and rainfall. Please see answer to main comment on runoff coefficient.

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

A: Please see answer to main comment scale issues.

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.

A: Please see answer to main comment climatic attribution.

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.

A: We agree that a modelling-based attribution approach as proposed by Merz et al. 2012 is difficult but with an appropriate and well adapted hydrological model it might be feasible.

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.

A: All minor editorial comments have been changed accordingly.