We thank reviewer #1 for his/her valuable comments. We have implemented the recommended suggestions. Doing so the quality of the paper has greatly improved. Point-to-point answers to the comments are given here below.

Comment 1 :

The title suggests that the paper aims to "characterize hydrological processes of eco-logical importance" but in fact the study is not looking at hydrological processes, such as interception, transpiration, infiltration etc., but more on "determines hydrological variables that are of ecological importance" (as also indicated in the abstract). I would suggest changing the title to be more consistent with the content of the paper. In line with this comment, I would also suggest to change the title of section 6.3 "hydrological processes" to be consistent with the content of this section. The structure of the pa- per is very confusing, the sequence of the methodology section is not in line with the sequence of the results. Results section is mixed results between the analyses of the observed information and the results of the TIM model. The structure should be up- dated, starting with 1) analysing the observed information (rainfall, discharge and flood extent) filling the data series and then 2) setting up the water balance model and 3) calibration against observed data/ GLUE approach, 4) analysing results of the model. Results section should reflect similar sequence.

Answer 1:

As suggested by Reviewer 1 and the other reviewers, we have changed the name of the paper so that it is more in accordance with the paper's content. In a similar fashion, its structure has also been entirely remodeled and improved.

The paper is now entitled: "Characterizing floods in the poorly gauged wetlands of the Tana River Delta, Kenya, using a water-balance model and satellite data".

The paper now has the following structure:

1/ Introduction: Wetlands are important ecosystems under threat. Lumped hydrological models, in combination with satellite data, can be used to characterize the flooding events for poorly gauged wetlands. The objectives of the study are then given, followed by a rapid presentation of the structure of the paper.

2/ Presentation of the study site (the Tana River Delta). We first present the social context, then the hydrological, climatic and pedological characteristics that are important to undertake hydrological modelling.

3/ Material and methods. We first present the general strategy used to combine hydrological modeling with remote-sensing techniques, then develop these two sections.

4/ Results. This section follows a similar sequence as Material and methods. We first focus on the remote-sensing results (use of water index, time series of flood extents, then the spatial extent of the floods), then detail the results related to the hydrological model (calibration and validation of the two parts of the model, then quantification of the hydrological variables of interest).

5/ Discussion and conclusion. We discuss the results pertaining to the remote-sensing data, then the hydrological modelling, then we discuss the utility of the results of this study for decision makers.

Comment 2:

There is no objective of the paper in the abstract, the link between the issue of upstream developments and the impact on the TRD and the development of the TIM is not clear. (…) The abstract should reflect the new structure of the paper.

Answer 2:

The abstract has been entirely rephrased to fit the new structure of the paper. In addition, the objectives have been clarified in the abstract.

Comment 3:

Objectives should be rephrased; "developing a model" is not a scientific objective, rather a method to be able to reach a certain objective; what is the objective of developing the model? Objective number 3 is not presented in the paper (the relationship between the inundation extent and the hydrograph). Page 11270, lines 7-17 are an introduction to the methodology and explained later in the methodology section, I would suggest to delete here.

Answer 3:
The authors agree with the reviewers' comments. We have therefore rephrased the objectives as: "The objective of this study was to quantify the main water fluxes and flooding characteristics in a poorly-gauged East African wetland: the Tana River Delta". We then introduce the hydrological model and the main results of the paper: "we constructed a lumped hydrological model (the Tana Inundation Model, TIM), that allowed to determine the role of river fluxes in flooding events and the number of flood events from 2002 to 2011, and to characterize the extent and duration of the floods. In parallel, the analysis of satellite data provided distributed inundation maps".

Comment 4:
2. Hydrological modelling and the use of remote sensing techniques in poorly gauged basins Section 2.1 is confusing as the title suggests an overview of hydrological modelling concepts and approaches, but the section starts with the problems with hydrological data and ends with the decision to develop a water balance model in the TRD. The section also refers to model choice being a function of the objective of the study, which is very relevant, and therefore the objective of the study should be made more clear in the earlier section, the choice of the water balance model is also not well supported. The question remains if this section is a review of literature or to support the model choice. Section 2.2 is also confusing as the title suggests that it will elaborate on the role remote sensing information in hydrological modelling, but the section describes the different types of remote sensing information that can obtain flood extents and which index was used to identify wet and dry pixels. It is not clear from this section why flood extent is the information sought from remote sensing and what its role is in the study (it is off course the calibration of the model, but this should be clear when you read the title).

Answer 4:
As the paper has been entirely re-structured, the sections relative to the literature review have been re-incorporated into the introduction, while the justification of the model structure and use of specific remote-sensing data have been put within the sections presenting the study site and the methodology.
In particular, we developed the advantages of using lumped models in poorly-gauged wetlands in the introduction and justify our use of a lumped model after the description of the available data.
Concerning Section 2.2 (in the first version of the paper), the literature review and the methodology (index used to identify dry and wet pixels) have been separated.
Finally, the utility of the extraction of flood extents from remote-sensing data has been described in the methodology section: "Calibrated parameters and their associated uncertainty were calculated through comparison of these flood extents with those measured from MODIS satellite data."

Comment 5:
Title of section 3 is not consistent with its contents. What are the 'important questions'? Section 3.2, the title is ambiguous "the Tana river in its lower catchment", what does this mean? This section is also not well structured, it starts with general characteristics, historical developments and the impacts, then something about seasonal rivers and their impact and then future developments. It is not clear what the authors are working towards.

Section 3.3 contains a number of suggestions for the fluxes in and out of the delta, which are not substantiated by literature or empirical evidence. One of the scientific research questions could be to establish these ungauged fluxes. The section should focus more on what is known and derive what it intends to investigate by doing the presented research.

Answer 5:
This section has been entirely remodeled. Doing so, the terms “important questions” and “in its lower catchment” have been deleted.

Comment 6:
Section 4.1: Why does figure 2 show an example of 1988, when this is not part of the studied period? The statement that "floods are attenuated between the two stations.." is confusing to me. The station at Garsen is located downstream of a floodplain (as suggested by the statement of strong evaporation in the floodplains)? Table 1 is also confusing, why do the authors split the year into two periods and then group them as one to obtain the so called "water gain or loss". This could be an option when the two periods are separately studied and a relationship is established for the wet season and one for the dry season (it is not clear how many of the 22 seasons used are wet and how many are dry). The conclusions the authors draw are not well supported from this analysis, to reconstruct a hydrograph from data obtained at a nearby station, total annual volume ratios are irrelevant. The approach the authors take in section 4.2 shows that indeed the annual volume ratio is not relevant (the 0.76 ratio is not appearing in the equations). Is there proof there is overflowing into the floodplains upstream of the Garsen station? This section should be shortened and more to the point.

Answer 6:
Figure 2 shows the major differences between the upstream and downstream hydrographs (attenuation of peak discharge and delay time). 1988 is part of the calibration period for the flood-routing model, and was chosen as the hydrographs are representative of the general trends found when the hydrographs were analyzed, and because there are few missing data for this year. As data at the downstream gauging station (Garsen) stop in 1992, it was not possible to represent more recent data.
We added the discharge rates modelled by the flood routing model on the graph so that a visual comparison between the observed and modelled data is possible. Furthermore, we added a small graph (in the upper right corner) representing the modelled versus observed data for the validation periods to show match between the two.

We agree that the statement “floods are attenuated between the two stations” is inadequate, as we should rather refer to the discharges. We therefore used “peak discharge rates are largely attenuated and smoothed out between the two stations” to describe the differences between the two hydrographs.

When reconstituting discharge rates, it is important to conserve the flood peaks, their timing, as well as the volume of water transiting at a gauging station. Fig 2 shows that the discharge peaks are attenuated between the two stations and that there is a delay time. Table 1 provides a rapid analysis of the transiting volumes to show that water losses are also important between the two gauging stations.
In doing this analysis, we were confronted with the problem of missing data. We chose to split the year into two seasons, so that we would remain with enough periods to calculate the mean transiting volume. Indeed, if we had taken one year as the study unit, then most years would have had too much missing data to be able to interpolate between the points and calculate the total volume transiting at the gauging stations.
This pre-analysis (on peak discharges, delay time and volume), and the statement by Maingi and Marsh (2002) on the strong evaporative losses between the two stations, justify the use of the flood routing model.

We agree with the reviewer that the ratio 0.76 does not appear in the subsequent equations (24) and (15). Indeed, these equations are derived from the flood-routing model, which is non-linear, compared to this linear ratio. The 0.76 ratio reflects the average loss of transiting volume, whilst the flood-routing model calculates this ratio for different discharge measurements. For example, a hypothetical uniform discharge at Garissa of respectively 600 $m^3.s^{-1}$ and 100 $m^3.s^{-1}$ lead to a volume ratio of 29 % and 81 %. However, we find that the 0.76 ratio is not irrelevant in the preliminary analysis because it reflects that there is a loss of water between the two stations, and the flood-routing model must be able to reproduce this fact (which it does).
This pre-analysis of the discharge data was shortened as recommended by the reviewer.

Comment 7:
Section 4.2: Table 4, why is RMSE defined in text and table. Why not delete the equation from the text (page 11278, line 15)? How is RE defined? How is NS defined? Values for calibration and validation can be added to the table instead of listing them in the text. In the text it is suggested that the NS, AME and MAE are only calculated for the validation period (page 11278, line 24-27) this is off course not the case. The text should be made consistent with the presentation of the results. Why are the results of the RMSE not presented?
Validation period (1963-1998) includes the calibration period (1986-1991) this should be clarified. The last two sentences of this section are not necessary, upstream dam construction would not alter the relation between the flows at the two stations. It is interesting to see that the flood propagation did not change in the period of study, but the link to the upstream dams is not necessary.

Answer 7:
The definition in the text of RMSE was deleted as suggested by the reviewer.
RE was renamed epsilon, as it better reflects that we are calculating an error. Epsilon is the difference between the value measured by MODIS and that simulated by TIM, taking into account the uncertainty ranges. It is defined in equations 15 and 16, and depicted in figure 5.
NS is defined in the text (section 3.4 in the new version) and Table 4.
NS, AME and MAE values were inserted into a table as suggested by the reviewer.
The text concerning the calculation of NS, AME and MAE for both the calibration and validation periods was corrected: "The Nash-Sutcliffe coefficient (NS), the Absolute Maximal Error (AME), the Root mean Square Error (RMSE) and the Mean Absolute Error (MAE) as defined by Dawson (2007) calculated and the observed and measured hydrographs at Garsen were compared for the calibration and validation periods to assess the quality of the flood routing model".
The RMSE results were added in Table 6.
The validation period was clarified in the text (1963-1986 and 1991-1998).
We agree with the reviewer that the sentences concerning the upstream dams is not appropriate for the Result section. We therefore comment this result in the discussion section.

Comment 8:
Section 4.3: The first section 4.3.1 is too long describing unnecessary details. Put NDWI as a proper equation in the text. Section 6.1.1 can be added to this text as this is not really a result but pre-processing and analysis of the images. Even section 6.1.2 can be added here to show the areal extent of the flood inundation, which is later used in the water balance model.

Answer 8:
Recommendations concerning synthesizing the section and including NDWI as an equation in the text were followed.

The entire section was remodeled for better clarity.

Comment 9:
Section 5. Step 1 is an extension of the analyses of the flood inundation maps obtained with remote sensing and should be added to the previous section. This section also has duplications with section 4.3.
Why is only a sub-section of the remote sensing data set used to calibrate, as all images are processed and used for the frequency analysis. The frequency analysis is a very interesting analysis, but it is to me not clear how this is considered the first step in developing a water balance model (title of section 5)
Step 2 in my opinion should be step 1, the water balance. Qi is the discharge at Garsen? The explanation about obtaining Zmin is not clear (page 11284, line 20- 22). What was the final value of Zmin and why is it not presented in table 3?
Why is potential evapotranspiration downscaled to hourly timescale? If the model is running at an hourly timescale, do you have enough information for the input (Qi, P, E) into the model and to verify the output (Qs, flooded surface). How can you downscale monthly rainfall to hourly rainfall? How is this verified?

Answer 9:
During the re-structuring process, the different "steps" as written in the first version of the paper were deleted and replaced by Figure 3 (a workflow diagram) which shows the different stages of the work.
To calibrate the images, we selected only the images that had a low cloud cover (<10%), so that 1/ the error in calculating the observed flood extent was low and 2/ we did not need to make constraining hypotheses on the clouded pixels (e.g. spatial distribution of the clouds).
On the contrary, we chose to use all the images for the mapping and frequency analysis in order to have a large sampling size so that the calculation is relevant.
Finally, using only part of the satellite data for the calibration, allowed further verification of the model with the frequency analysis (Figure 9), because it used all the images.
Qi was discharge at Garsen, and has been termed Q2 in this new version for better clarity.
The section concerning Zmin has been rephrased.
We agree that the time-step at which the model's results can be analyzed is constrained by the time-step of the input variables. In our study, the main variables (e.g. MODIS, discharges with a decadal floating mean) are approximately on a 10-day time-step. Therefore, the results are averaged over this time-step.
The downscaling of evapotranspiration and rainfall at Garissa is justified a posteriori by the fact that their contribution to the water-balance is minor. Hence, even if the temporal distribution within the month was slightly different, this would not affect the results of the study. Finally, the downscaling of monthly values to daily values is justified in our case by the fact that no daily or decadal evaporation or rainfall (at Garissa) data were available.

The time-step at which the results were analyzed is different from the time-step at which Equation 5 was solved. The latter was solved at an hourly basis, so that the numerical resolution of this differential equation (by RK4) was stable. Hence, the equation was solved at an hourly basis, but the model is run on a 10-day basis.

These explanations have been included into the paper as:
"Equation 5 was solved iteratively using the fourth-order Runge-Kutta algorithm (Atkinson, 1989). For reasons of numerical stability, it was solved at an hourly time-step. Final output variables were averaged over 10 days so that the temporal resolution was close to that of the MODIS images and major input variables."
"In the absence of daily precipitation data for the subcatchment, R_{SC} was uniformly distributed daily values calculated from the monthly precipita- tion data at Garissa."
"In the absence of more precise data, uniformly distributed values of daily potential

evapotranspiration, e, were obtained from monthly estimates at Malindi (Woodhead, 1968)"
We added a short section on their use in the discussion section to clarify our opinion.

Comment 10:
Step 3. The model is calibrated against the flooded surface areas (but only the selected 76?). Did the authors come up with the MNS or is this presented in earlier work (and why was it then not referenced). What is the definition of SMYDmin and SMYDmax the methodology only describes the calculation of SMYD.

Answer 10:
MNS (re-termed L in the new version) is similar to a Nash-Sutcliffe coefficient used widely in hydrology, but differs in two ways: 1/ it compares flooded surfaces instead of discharge rates, and 2/ it takes into account the uncertainty of the observed and/or simulated data. We did not find in the literature any reference with these specificities, so we defined it in our text.
SMYDmin and SMYDmax (re-termed So,m and So,x to simplify the notations) have been defined in the text (section 3.2.3. in the new version).

Comment 11:
Section 6.3 Title is confusing, the section does not deal with hydrological processes (dynamics of inundation and flood extent and duration are not hydrological processes). Figure 6 presents the results of the model, why are there periods when the water balance model does not provide results? Section 6.3.1 describes the variability of the inundations based on the hydrological model results. The discussion in this section on uncertainties of the data should be moved to discussion or removed completely, these are not results.

Answer 11:
The title of the corresponding section has been re-phrased.
The periods where the model does not provide results correspond to the periods where discharge rates at Garissa are missing. This has been clarified in the text.
The section discussing uncertainties of the data has been moved to the discussion.

Comment 12:
Section 6.3.4 the values for the water transiting and flowing into the system should have a unit of L3 T-1, which means km3/year. The mean total volume is therefore not relevant. The values calculated as percentage to the total inflow and outflow, are they based on annual averages, does this differ in the dry and wet periods (eg during floods runoff near the delta is more important perhaps?)

Answer 12:
Units were corrected.
The values calculated as percentage are indeed calculated from the annual averages, which has been clarified in the text. We incorporated the annual water balance for a dry and wet year in Table 9 and discussed the differences in the text.

Comment 13:
Section 7. page 11294, line 25 to page 11295, line 6, should be move to conclusions sections as recommendations. This whole section should be reduced in size and focus on discussion of the results rather than discussing the tool used (this would be in line with the new objective)
Section 8 conclusions are too general, there is no conclusion on the results of the water balance model and how this helps in understanding the delta system (the conclusions state that major hydrological processes are determined but it does not show what they are and how they are relevant). Plus the main analyses are done on annual basis and does not include inter-annual variability. This should be made much clearer. In addition a few lines on the use of remote sensing data as parsimonious to the water balance can be of use. Recommendations from the previous section can be added here.

Answer 13:
The Discussion and Conclusion section has been remodeled, and we believe this responds to the reviewer's comments.

Specific comments:
1/ Page 11269, line 9-10. The hydrological processes, listed, are not hydrological processes, but more hydrological variables. Adjacent is not the right word for a river flowing into the wetland (it is not adjacent).
Ans: processes has been re-termed variables, the sentence using "adjacent" has been re-phrased.

2/ Page 11269, line 17. Please rephrase the sentence on "wetlands with low moisture", it is not clear how a wetland can have low moisture.
Ans: this section was removed when we shortened the paper.

3/ Page 11273, line 25, change catchment into basin (Tana is large enough to be a basin and not a catchment)
Ans: the modification was done as suggested.

4/ Page 11274, line 3-9, put all references at the end of the sentences Page 11275, line 2, change 'temporary rivers' into 'seasonal rivers'
Ans: The sentences have been modified according to the reviewer's suggestions.

5/ Page 11275, line 12, although the irrigation project would not affect the peak flows, it would have an impact on the dry season flow right?
Ans: We agree that the irrigation project could affect dry season flow. As we are interested in this study in the flooding characteristics in the wetlands that occur during peak discharges, we clarified our sentence: "the Bura and TDIP irrigation schemes would not alter significantly the Tana River discharge during peak flows and hence the flooding characteristics in the TRD"

6/ Page 11275, line 15 the units for rainfall should be mm/a and not mm (it is a flux, not a volume).
Ans: Proper units have been inserted, including in the figures.

7/ Page 11275, line 16&17, you can delete the statement on 'years with over one month missing data were excluded'
Ans: the phrase has been changed for better clarity. However, we kept this statement in the paper as this methodological explanation is important to reproduce our results.

8/ Page 11275, line 25 'surface runoff from outside the floodplains is probably limited,...' is a very vague statement and not supported by any evidence. Page 11276, line 8 'clayey nature of the TDR soils probably limits ....' is a very vague statement and not supported by any evidence, same for line 12 on the regional groundwater flows. These are possibly results from the water balance model?
Ans: the sentences have been re-phrased

9/ Page 11276, line 17. "three gauging stations were historically available" this sentence implies that the stations are no longer functional, the placement of the word historically is also not clear. The sentence should be corrected to proper English standards.
Page 11276, line 23, what do the authors mean with "its chronic"?
Page 11277, line 1, what do the authors mean with "are extremely lacunary"?
Page 11277, line 10-11, the line with "the original discharge ..." should either be at the beginning of the section or deleted.

Page 11284, line 19, minimal should be minimum (see also table 3)
Page 11285, line 1, potential evapotranspiration can not have a unit of L L-2 T-1, this should be instead L3 L-2 T-2, which translates to L T-1.
Page 11288, line 6, delete Dawson (it appears twice)
Ans: these sentences have been rephrased. “extremely lacunary” has been replaced with quantified data. “minimal” and “maximal” were replaced by “minimum” and “maximum”. Units for evapotranspiration have been corrected, and “Dawson” deleted.

10/ Number tables and figures in the order of appearance in the text (right now, table 4 is the second table mentioned in the text.
Ans: Tables, Figures and Equations have been re-numbered in accordance with their order of appearance within the text.

11/ In general the paper is too long and too descriptive and needs serious attention with regard to the use of the English language.
Ans: The paper has been shortened and has been corrected for language by an independent firm.