

Interactive comment on “Floods and wetlands: combining a water-balance model and remote-sensing techniques to characterize hydrological processes of ecological importance in the Tana River Delta (Kenya)” by C. Leauthaud et al.

Anonymous Referee #1

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The presented paper deals with a very relevant topic on how to model a wetland area, with very limited amount of ground data, and how remote sensing techniques can improve a conceptual water balance model for this area. In addition, the remote sensing images provide good information about the flooding extent and frequency in the Delta. Although the study is very interesting, I have many comments on the structure of the paper, the objectives of the paper and the use of the English language. I tried to give

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as many comments as I could for the authors to improve the paper, and I hope they will appreciate it and work on the comments to present the work in a more presentable format.

General:

The title suggests that the paper aims to "characterize hydrological processes of ecological 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 paper 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 updated, 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.

Abstract:

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 paper does not deal with scenarios of changes in the upstream catchment and the link to the Delta. The abstract should better reflect what the rest of the paper describes. In my opinion it is about determining the hydrological variables through the development of the TIM (TIM is the method not the objective). In addition, a number of the results can be derived from analysing the observed data (such as the Modis images and observed discharge for the flood frequency and extent). The paper should

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distinguish between the analyses of the observed information and the results from the TIM model (e.g. the influence of the discharge to the water balance of the Delta). The abstract should reflect the new structure of the paper.

Objectives (page 11270):

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.

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 of course the calibration of the model, but this should be clear when you read the title).

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

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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.

Section 4

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.

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

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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.

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.

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. Q_i is the discharge at Garsen? The explanation about obtaining Z_{min} is not clear (page 11284, line 20-22). What was the final value of Z_{min} 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 (Q_i , P , E) into the model and to verify the output (Q_s , flooded surface). How can you downscale monthly rainfall to hourly rainfall? How is this verified?

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

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why was it then not referenced). What is the definition of SMYDmin and SMYDmax the methodology only describes the calculation of SMYD.

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.

Section 6.3.4 the values for the water transiting and flowing into the system should have a unit of $L^3 T^{-1}$, which means $km^3/year$. 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?)

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.

Specific comments:

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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).

Page 11269, line 17. Please rephrase the sentence on "wetlands with low moisture", it is not clear how a wetland can have low moisture.

Page 11273, line 25, change catchment into basin (Tana is large enough to be a basin and not a catchment)

Page 11274, line 3-9, put all references at the end of the sentences

Page 11275, line 2, change 'temporary rivers' into 'seasonal rivers'

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?

Page 11275, line 15 the units for rainfall should be mm/a and not mm (it is a flux, not a volume).

Page 11275, line 16&17, you can delete the statement on 'years with over one month missing data were excluded'

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?

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"?

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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)

Number tables and figures in the order of appearance in the text (right now, table 4 is the second table mentioned in the text.

In general the paper is too long and too descriptive and needs serious attention with regard to the use of the English language.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 11267, 2012.

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