Hydrology and Earth System Sciences Discussions

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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 #2

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The paper deals with a very interesting and significant topic of modelling and assessment of hydrological fluxes in poorly gauged wetlands. The study uses a combination of methods, selection of which is obviously guided by the available data and the specificity of the location. In spite of this, the paper has a broad scientific relevance, as it illustrates specific approaches in a unique combination.

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The paper has, however, several conceptual deficiences, which should be addressed before it is published. Also, it is imperative that the paper is edited for language.

General comments:

1. Structure of the paper The paper has a very unclear structure and non-informative naming of sections. There seems to be a mixture of general information, methods and results present in each of its sections. A strict editing for sequence of information and content would make it much more readable. For example, section 2.2 promises an overview of RS in modeling of inundation, titles of subsections suggest it is a description of methods/data used in the paper, while the contents of subsections delivers a combination of both. Section 4 contains elements of methods, data and results. Section 5.1 suggest description of available data, while in fact it is a description of methods used in the paper. I would suggest sections 4 and 5 to be reworked into a clear and succinct materials and methods, with parts relating to "preanalysis of hydrological data" moved to the result section. The paper also contains several unnecessary elements. For example, RMSE and MAE and Nash-Sutcliffe coefficient are indices that do no have to be defined, It will be sufficient if the authors describe with adequate precision what model variables were these indices determined for. NB, is seems to me that AME is an inappropriate name for the index the authors use, and should be corrected, see one of the comments below.

2. Concept of inundation frequency The concept of flood (inundation) frequency as authors use it seems to be somewhat weak. Inundation frequency is not precisely defined in literature, but a common understanding would allow for the following definition: a number of times an area changes status from non-inundated to inundated during a certain period of time (a year, perhaps). According to eq. 3, the authors express inundation frequency as a fraction of images in which a pixel was flooded (inundated). Considering the temporal characteristics of flooding in the studied system, where in most of the area there are principally two flooding events during a year, it is possible, or even likely, that a single inundation event is counted more than once. Under as-

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sumption of uniform temporal distribution of images from which inundation is derived, fi of Eq. 3 expresses percent of time inundated and not inundation frequency. Additionally, whether the images are uniformly distributed in time, Although they state that the images are well-distributed throughout the year, they repeatedly stress that the time series of MODIS images is "discontinous". Data are not presented, but there are 48 8-day composites throughout a year, which gives 405 images for the 9-year period. The authors analyse only 67 of these. Considering the cloudiness-rainfall-flood association, it is difficult to believe that the flood/no flood periods are indeed uniformly sampled by MODIS flood maps. In fact, the statement from page 11289: " The total surface with a frequency fi >0.01 is of 450km2, which means that each corresponding pixel was flooded over four times in 2002–2011. ", which is impossible to interpret, only confirms the problems with the approach adopted by the authors. Perhaps the term "flood frequency" should be properly defined in the beginning, or a different, more appropriate term should be used instead.

3. Uncertainty of inundation mapping from MODIS images In section 4.3.2 the authors describe a measure of uncertainty of inundation mapping, and this measure is subsequently used to assess the quality of the hydrological model (section 5.3). Table 5 shows that this measure is determined as a mean percentage of dry pixels that were classified as inundated, and a mean percentage of inundated pixels that were classified as dry in classification of three images. The authors equal these to underestimated and overestimated inundated area. These two are, however, not the same, and the percent indices derived in this way cannot be used in the context presented in fig. 4, i.e. to set upper and lower bounds of derived inundated area. Additionally, these uncertainty bounds are entirely arbitrary, i.e. there is no clear relationship between them and characteristics of distribution of the error such as standard deviation of inundation size (if one assumes error to be normally distributed) or usually used percentiles (e.g. 10th and 90th). Because of that, all errors (AME, MAE, RMSE) calculated from RE are essentially meaningless in absolute terms. The concept of error of inundation classification has to be revised. Additionally, for the sake of consistency,

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values of "% underestimation" in table 5 should be calculated accounting for errors of classification for 25 May 2009, even though there was no flooding during that period (e.g. % underestimation for th=0.09 should be around 10% and not 15%.

4. The model has been run on hourly basis, which seems absolutely not compatible with the nature and amount of data used, and the nature and variability of hydrological processes in the study site. It would be prudent to provide justification for this hydrological modeling "overkill".

Specific comments:

p. 11268, line 21: the model is not based on GLUE, the model is calibrated using GLUE approach

p 11269, line2: what's the hypothesized reason of wetland decline? Line 10: flood extent, timing, frequency etc. are not "hydrological processes in the adjacent river". Rather, these are characteristics of flooding or hydrological conditions.

p. 11270 line 22 "Important environmental questions…" what questions? In what context?

p. 11257, line 22 "precipitation would usually occur before flooding extent" seems to be a truism, while "precipitation does not induce the floods" sound counterintuitive, and perhaps need explicit explanation. It seems that distinguishing between "local", i.e. preciptiation over the wetland itself, and "upstream" precipitation would make the above statements much more precise.

p. 11281, first paragraph: "to determine threshold … that best differentiated…" - it is not very clear how the "best" value was obtained. It is also not clear what the "upper and lower uncertainty range" pertain tom and how they were determined. One would assume that the upper and lower uncertainty bounds describe a range of estimates of inundated area, obtained by using a certain, non-optimal values of the threshold. Or does the uncertainty pertain to the threshold itself? How exactly were these bounds

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calculated? Also "the upper uncertainty range was increased" – by how much? The authors have to be more specific here.

p. 11281, line 19: "frequency of floods corrected for cloud cover" – it is an awkward formulation. The flood frequency is not corrected for cloud cover per se. Rather, the method of calculation of flood frequency takes into consideration discontinuity of inundation maps that is caused by cloud cover.

p. 11283, line 7: "the ratio of flood extent to the visible zone was identical to the ratio of flooded extent to the clouded zone" – perhaps what the authors meant was that "ratio of inundated to non-inundated area in the visible zone was identical with that ration in the clouded zone"

p. 11287, line 8: "Parameter sets which did not allow a full resolution of differential equations..." what did the authors mean exactly? Parameter sets for which solution could not be achieved?

p.11283, line 1: "temporal frequency of flood extent" – this is a confusing term. Equation 4, as explained in the text does not give any expression of inundation frequency. Rather it gives a mean inundated area for times depicted in flood maps only.

p. 11284, line 15: "..coefficient a was expressed as a function of initial condition corresponding to the surface of permanent lakes'. It seems to me that the "reference" for coefficient a is "the surface of permanent lakes", and not the "initial condition". That initial condition is arbitrary and just happened to correspond to the stage at which only permanent lakes are filled with water. Expression " coefficient was expressed as a function" gives a wrong impression, it suggests a is a dynamic variable. It seems to me a is static and had to be calculated from volume of water stored in permanent lakes and the area of permanent lakes.

p. 11287, line 25: ".. each parameter gives a percentile for every time step." – what does it mean?

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p. 11288, equation 14: conditions describe in that equation are not exhaustive and mutually excluding. It is not clear how RE is calculated from RE_4 RE5 and RE6.

p. 11288, As we set the relative error to nil each time the respective uncertainty ranges overlap, the 25 indicators will have a higher value.

p. 11289 , line 23: "The total surface with a frequency fi >0.01 is of 450km2, which means that each corresponding pixel was flooded over four times in 2002–2011. ". It is difficult to understand how fi of 0.01 translates to 4 times in 9 years. Some additional explanations are needed.

p. 11306, table 4. The formula for "Absolute Mean Error" suggest that this term should rather be called "Maximum Absolute Error"

p. 11293, section 6.3.4: perhaps terms "flow" and "inflow" can be replaced with "flux" and "input"? Otherwise the sentence: "A mean absolute difference of 0.14 % between the incoming and outgoing flows for all parameter sets was observed and attributed to the use of numerical resolution and its related approximations. " can be interpreted as meaning that components of water balance other than river flows, i.e. rainfall, infiltration and evaporation were negligible.

Use of terms: I suggest replacement of the terms listed below with alternatives:

p. 11268, line 9: "water height' – "water level" or "water depth" (also elsewhere in the document); line 20: "primordial" – perhaps "a prerequisite"?

p. 11270, line 3: "rapidly" – perhaps better "easily", line 4 "parsimonious" – perhaps "simple"

P. 11271. Line 23: "uniqe" – "single" (also elsewhere in the document); line 24: "calibrated with . . .data" – "calibrated against ... data"

p. 11276, line 5: "here and there, small islands" – " small island present throughout the system.."; line 10 "inundations" – "flood water"

p. 11276 line 23 "chronic" – "time series", "missing data were completed" – "gaps in record were filled"; line 24 – "coinciding with flood event" – "when flood events occurred"

p. 11277 line 1: "lacunary" – "patchy" or "discontinuous"; line 6: "stage board" –"gauge plate"; "rating curve is under construction" – "measurements are carried out in order to establish rating curve"

p. 11278 line 3 "reconstituted" – "reconstructed"; line 7: "varying propagation trends" – "differences in propagation trends"; line 11: "floods" – "flood events"

p. 11279, line 1: "discontinuous flood surfaces" – "flood surfaces"

p. 11281, line 20: "discontinuous time series" – "irregular time series" or simply "time series" (also elsewhere in the document)

p. 11282, line 16: "for each non-masked pixel" – "for each pixel classified as non-clouded"

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