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Authors' Response to anonymous referee #1

Title: Modelling stream flow and quantifying blue water using modified STREAM model in the Upper Pangani River Basin, Eastern Africa.

We would like to thank anonymous referee #1 for his/her comments to improve the quality of the manuscript. We acknowledge the valuable time that was put in towards providing detailed and helpful suggestions. The suggestions are very valuable and we have incorporated them as indicated in the detailed response.

Here are point to point comments and issues addressed in the revised manuscript.

Overview

I have read the manuscript entitled "Modelling stream flow and quantifying blue water using modified STREAM model in the Upper Pangani River Basin, Eastern Africa" with great interest. This research article discusses an interesting approach towards modeling stream flow and quantifying blue water using modified STREAM model in the Upper Pangani River Basin in Eastern Africa. I think, the manuscript is valuable for the scientific community and recommend its publication. However, I will comment critically to further improve the paper and suggest the authors to revise the paper for its possible publication.

Specific comments are responded as shown below:

Referee comments 1-9 and general remarks.

1. The authors have used the double mass curve to derive the rainfall up to mountains peak using the rainfall data of the neighboring stations. Please explain in detail about double mass curve or at least provide suitable reference. Of course, this is well known method, but for readers it would be good to understand in detail.

Authors' response

We agree and have added further explanation and references to explain the concept of the double mass curve.

2. P15778, L8

The authors discussed the short comings in the remotely sensed rainfall data. I agree with it, but the bias can be removed using limited ground data.

For instance see Cheema and Bastiaanssen (2012), Local calibration of TRMM rainfall data. It will be nice to briefly discuss the short comings.

Authors' response

We are in agreement that the bias of remote sensing data can be removed using limited ground data. But the argument was that the ground data on the mountainous areas are also missing and the RS data will also encounter similar shortcoming.

We have elaborated the argument and added additional references.

3. P15778, L18

In model development section P15778 L 18; Is 8 day time step is a constraint? Can it be applied at daily time step? If you use the time step smaller than 8 days, how much your results would change. I am very much interested in checking the sensitivity of the developed model in the form of scenario analysis. Is there any sensitivity analysis performed? Would be nice to include it as well.

Authors' response

The actual ET (8-day) has limited the model timestep to 8-day, the model therefore cannot run at a daily time step. The 8-day time step provides sufficient detail for the purpose of the model (ie assessing blue water use), as the time scale for agri. water use is larger than 8 days. The interception that has lower time step was calculated as precessor at daily timestep and aggregated to 8-day. It is therefore expected that the results will not be much different if the model would have run on a daily time step. We have improve this explanation in the revised version of the manuscript.

The suggestion of performing a sensitivity analysis for all model parameters is very much appreciated and has been included in the revised version.

4. In the paper, the ETb is calculated from the groundwater as capillary rise C(t).

Which equations have been used to calculate the capillary up flow from groundwater? I would suggest the authors to read papers by Vervoort and Van der Zee, 2008, 2009 regarding calculating capillary up flow. They have developed analytical equations for capillary upflow and effect of capillary up flow on the soil moisture. Integrating these equations in your analysis would give another angle of checking your results.

Authors' response

Our modified stream model does not include the capillary flow process. The water use by plants may include water from capillary rise and this is implicitly taken into account through our measured actual ET and minimum moisture depletion factor (*f*) which are an input in the STREAM model. The capillary flux is only derived through a simplified water balance to maintain the evaporative demand of the unsaturated zone. Using this procedure, the capillary rise becomes non influential parameter because it is dependent on ET and *f*.

However, the capillary rise estimates where comparable with the capillary fluxes calculated using the analytical modelling framework (Shah et al., 2011) in similar landscape and climate conditions. This has be added in the revised manuscript.

5. For simulating the model, how many years of rainfall data were used. For calculating long term average of discharge and other water balance components, you need to simulate the model for longer times. This may be another reason of underestimating/overestimating the discharge. I would suggest the authors to generate long term rainfall from the Poisson rainfall function and use this generated rainfall data for calculating the long term averages. For long term simulations, the moments become stabilize and output becomes more promising, if possible.

Authors' response

We appreciate the suggestion by the reviewer to extend the time series for the modelling in order to get better appreciation of long term water balance components. Unfortunately, rainfall is not the only input data required to run the model. ETa data is also required and we only had three years of data for this purpose, additional data would require substantial additional effort to

generate. The modelling therefore does not provide long term average results, but the selected years do provide a range of wet (2008), average (2010) and dry (2009) years.

6. The regression line shows R2 more than 0.5, but the Qo and Qs don't lie on the 1:1 line especially for greater magnitude of discharge. Of course, you have explained that clouded satellite images create uncertainty and that's way both discharge differ a lot. Please write some words about possibilities of reducing this uncertainty in clouding. This is a great challenge to do further research. I also think that there are sources of errors in addition to clouding that generate theses uncertainties.

Authors' response

We appreciate the reviewer's sentiments that obtaining good quality ETa input data is crucial, but challenging because of the cloud cover. But indeed this is not the only potential source of error. Other sources of error include a) model conceptualization errors, b) errors in hydro-meteorological data - discharge data (poor maintenance, rating curves especially in high flows, and uncertainties in actual readings) and including rainfall. There is also the potential of improving uncertainty in clouding in future by using passive microwave imagery.

We have included the discussion on other possible error sources in the revised manuscript

7. P15781, L15

In the equation 3, Is it total Ta?

Authors' response

Yes. Total Ta, includes soil evaporation. We have changed the symbol to (E+T) in the revised manuscript.

8. In the conclusion section it will be good to provide %age of various performance indicators used.

Authors' response

We have added the percentages of the performance indicators and made other improvements in the revised manuscript for clarity. Indeed a helpful comment.

9. General remarks

1 P15587 L2 Use "five" instead of 5.

Authors' response

We have revised accordingly

L18 Eqs. (15) and (16), respectively.

Authors' response

We have revised accordingly

L20 "squared" instead of square

Authors' response

We have revised accordingly

2 P 15774, L10 Resulted instead of resultant

Authors' response

We have revised accordingly

L25 Remotely instead of remote

Authors' response

We have revised accordingly

3 P 15775, L14

Please use latest references. E.g. Cheema et al 2013 has used satellite derived rainfall to parameterize the SWAT model while Eta from ETLook was used to calibrate the model to determine contribution of groundwater use in total blue water use.

Authors' response

We have updated the literature with the latest publication - Cheema et al., 2014.

L20 I am not convinced with this statement. Such effects can be avoided using passive microwave imagery.

Authors' response

We agree. The lack of technical know-how is better term to describe the difficulties in extracting electromagnetic satellite information including the use of cloud-free sensors such as passive microwave imagery. Similar suggestion was made by the 2nd reviewer. We have revised this section accordingly

L23 Kindly rephrase the sentence "method of using of Eta".

Authors' response

We have revised accordingly

4 P 15777 L7 Remotely instead of remote

Authors' response

We have revised accordingly

5 P 15778 L2 “developed by” instead of “from recent research”

Authors' response

We have revised accordingly

6 P15779 L1

“was” instead of “is”

Authors' response

We have revised accordingly

L23 “calculated” instead of “calculate”

Authors' response

P1580 - We have revised accordingly

Author's Response to anonymous referee #2

Title: Modelling stream flow and quantifying blue water using modified STREAM model in the Upper Pangani River Basin, Eastern Africa.

We would also like to thank anonymous referee #2 for his/her comments and valuable time that was put in towards providing detailed and helpful suggestions. The suggestions are very valuable and we believe they have improved the quality of the manuscript. We have incorporated them as indicated in the detailed response.

Here are point to point comments and issues addressed in the revised manuscript.

Overview

This paper highlights an approach of incorporating remotely sensed satellite data in distributed hydrological modeling. Such an approach is warmly welcome in the sub- Saharan Africa where most hydro-met stations are pitted against vandalism and lack of capital investment from relevant authorities. It is worth noting there a relatively few hydrologists who are conversant with the technical details of manipulating raw remotely sensed data into meaningful hydrological signatures as presented in this paper. The paper goes into deeper depths of detailing the model structure but with limited focus on results and discussion – especially on the capabilities of the modified STREAM model in capturing the various hydrological signatures over space and time. This is one of the major weaknesses of this paper on and above several typo errors.

Specific comments are responded as shown below:

1. Abstract L1. The sentence could have a better meaning if “water uses” could be replaced with “water resources management”.

Authors' response

We have revised accordingly

2. Abstract L11. The abbreviation “STREAM” needs to come after a description of the full name. This is applicable to all abbreviations in the document.

Authors' response

We have revised accordingly

3. P 15773 L2 ,: : :impacts of different scenarios; The word “management” is missing here! Otherwise what scenarios are we talking about?

Authors' response

We have revised accordingly

4. P15774 L10 “resultant” should be replaced with “resulted”

Authors' response

We have revised accordingly

5. P 15775 L15. This paragraph highlights challenges of applying remotely sensed data in hydrology, notably in hydrological modeling. This paragraph could benefit more if the authors could read the following article [Schultz G.A. 1993. Hydrological modelling on remote sensing information. Advances in Space Research, 13 (5):149-166)]. Most hydrologists are only used to

conventional hydro-met data sets and lack technical know-how of how to manipulate RS data in hydro-studies. This is a big challenge on wider adoption of RS data in hydrological studies.

Authors' response

This is a valid limitation for wider adoption of RS data in HM. We have included a section on the lack of technical know-how to transform RS data (electromagnetic information) to hydro-meteorological data in the text and provide reference.

6. Model Development.

The use of an 8-day aggregate (8-day time step) need more clarification. The authors indicate that this time step correspond to time scale that characterizes agricultural water use. I am lost here: : :what agricultural water use are they talking about? Is it irrigation scheduling? I am not convinced by this statement and have the feeling that the authors chose this time step mainly for convenience given the fact that the MODIS products are available on 8-day aggregates. If this is the case, then, what uncertainty is introduced by this “convenience”?

Authors' response

Indeed the time step of the modelling is predefined by the available data on actual evaporation. We argue that this does not affect the modelling results much because of the mentioned timescale for agricultural water use. This agricultural water use incorporates both rainfed and irrigated agriculture, which use water from the unsaturated zone. The time scale of this water use is between 10 - 30 days (unsaturated zone storage over transpiration rate).

The rainfall and interception that has lower time scales have also been calculated as precessor in the model using daily data and aggregated to 8-day time step.

We have expanded the section on the use of the 8-day time step in the revised manuscript.

7. Results and discussions.

The authors have not provided a rigorous analysis of results and discussions as what readers would have expected. The paper would have benefited more if the authors could have developed and analyzed at least a few water use management scenarios in the Pangani basin using their modified STREAM model and compare with baseline (the current situation). Otherwise the paper does not abide to the tone in their abstract and even introduction of the paper. Naturally, the use of a model in water resource management is in providing answers to the typical question “what if?”. This is what the authors need to highlight in this paper, and also water resources managers want to read and hear the same as well.

Authors' response

We have introduced 3 scenarios for water saving and increased crop productivity. This was a very interesting suggestion, and we believe that incorporating these three scenarios has improved the outcome and the general outlook of the paper.

8. STREAM Model

The authors have noted that the motivation of modifying the STREAM model was due to failure of the earlier version to account for actual transpiration in a realistic way. They have also provided references to support their argument. However, as a reader, I am more interested to see

a comparative analysis of the two versions of the model, with focus on actual transpiration and possibly other parameters as well. Possibly, a Table or graphical representation would suffice.

Authors' response

STREAM model was initially developed purposely to simulate stream flow generation in mostly natural landscapes - forest, woody savannas and dambos (wetlands that store large quantities of water) in upper Zambezi (Gerrits, 2005; Winsemius et al. 2006). The STREAM model was therefore not meant for use in a highly utilized landscape like the Upper Pangani River Basin (also from personal communication with Prof. H. Savenije involved in model devt). The motivation for modifying STREAM model was therefore to improve the model capability to account for irrigation water usage especially in a river basin where informal irrigation systems are dominant. In our view, it makes little sense to model the Pangani with the original STREAM model developed for the Zambezi and thereafter compare the outcomes with the modified STREAM. The two other references where STREAM was used in its original form were cases also dominated by natural landscapes (Abwoga, 2012; Bashange, 2013). However, in few irrigated areas, (E + T) was underestimated, fundamentally because of the model incapability to account for blue water use.

9. Sensitivity Analysis

Lastly, the authors need to address sensitivity and uncertainty issues of their model. This is a typical issue in hydrological modeling and the authors cannot escape this.

Authors' response

This was helpful suggestion particularly for the modified model. We have presented a sensitivity analysis for all model parameters.

10. Figures

Fig.5(a-d). It would be nice to have scatter diagrams as well.

Authors' response

This was also a good suggestion and has been included in the revised version of the manuscript.

11. Figures

all figures. The authors need to be consistent throughout the document. They have used both “Figure” and “Fig.” in the text.

Authors' response

We have used "Fig." in all the text.