Hydrology and
Earth System
Sciences

Discussions

Open Access

Interactive comment on "Water budget modelling of the Upper Blue Nile basin using the JGrass-NewAge model system and satellite data" *by* Wuletawu Abera et al.

Anonymous Referee #2

Received and published: 27 September 2016

GENERAL AND IMPORTANT COMMENTS ABOUT THE MANUSCRIPT

The Manuscript (MS) is an attempt to integrate various sources of satellite remote sensing data towards macro-scale hydrologic modeling in a region in Africa. Such a concept is novel considering the eminent data limitations pertaining to lack or limited observed in-situ hydro-meteorological data important for model calibration and validation purposes. In this study, the authors seem to be interested in historical cases of the water budget, and hence may elect to put this is the title, or justify why they are not interest in forecasting. From the present standpoint, however, the paper can be considered for publication in the near future, but only after addressing some serious technical issues that degrade the novel concept proposed and applied by the authors. In this

respect, and to improve and make the MS much better, I wish to recommend major revisions before further consideration. The following are some of the major comments that need redress:

(a). Language Limitation: the MS is poorly written and generally very difficult to read right from the abstract to the conclusions. This may be due to language limitation/culture of the authors, but considering that the MS will have a bigger readership; it would be nice to English edit the MS so that the actual intentions - technical and linguistic- can come out clear. The way the results, especially the statistics and maps, are presented makes one question the objective of the work. In some cases, it is difficult to understand it the authors intend a comparative assessment at various spatial scales of the regions in the basin? There is also the random use of difficult expressions appearing from nowhere without prior definition, i.e. in defining the table in page 15, he used Figure 5, Table 2 which is difficult to understand.

(b). the author claim that his research is motivated by data limitation. However, he seems to have some stations with streamflow data as by the hydromet stations in the study area map or otherwise, the hydrographs used in the validation exercise. This begs the question: So where is the boundary of this data limitation he is claiming? Could it be possible to use the available data to parameterize the model and later regionalize the model? Or is it possible to develop criteria to extrapolate the results after calibration and validation of the satellite estimates with the limited but available observed data-sets? The authors may also need to justify why 402 sub watershed were delineated considering the limited river gauging stations shown in the study area map. If he wants to retains them, then he should define use a criteria to choose at least 10-15 sub-catchments and provide their morphometry together with the simulated values of the water balance components in the results section, for consistency and clarity. A table (and not maps) in this respect would quickly help things out here.

(c). considering data uncertainties, would it be wise to believe the higher model reliability and hence results? The authors need a good and elaborate justification of how the

errors cancelled out during the simulation. Furthermore, the author seems to be using some part of the available data for calibration, and the same half plus the rest within the time frame for validation. In my opinion, the conventional way would be to divide the data-sets into two, one for calibration and the other for calibration. Could this be the reason for the good efficiency realized? The authors need to justify this methodology very strongly.

SPECIFIC COMMENTS ABOUT THE MANUSCRIPT

(1) TITLE

The title is okay and acceptable, but may sound better if the authors consider the conventional way of staring a sentence with a verb i.e. Modeling/Estimation/Assessing of the Water Balance etc. This is however trivial at this moment.

(2) ABSTRACT In my opinion, the first sentence can be made simple and realistic i.e. . . .by saying the region is one of the data scarce regions is the developing regions (but not in the world as this raise a lot of questions and may temp one to ask for proof of review in the introduction.

Are there basins in the UNRB that have data? Is the justification of one of the data scarce regions in the world thus still valid?

In my opinion, the water budget components of study can be explicitly mentioned in the sentence without the brackets, and the tools used well captured and summarized. This makes the section clear and easy to read.

Considering that modeling procedure employed, and the possible uncertainties involved, the results need to be rounded off i.e. by saying that precipitation values between 1000-1600mm were estimated depending on seasonality etc

Generally, the abstract can be well written and summarized in good English language, and only important content.

(3) INTRODUCTION:

This section can be language edited and the phrases backed with the latest references. The references also need to be ordered either from the latest to the oldest or vice versa as required by the journal.

In my opinion, the text in lines # 4-10 can be summarized and well captured within the text without using bullets or points.

Lines # 27-28: the sentence beginning with [The use of RS precipitation products. . .] can be well written, more content added and justified. Here the authors can introduce and justify the use of other products such as GLEAM, MODIS data products etc for simulation. The author seems to neglect this section/paragraph and YET it forms the basis of their novel idea of using RS for data scarce regions. In my opinion, 'at least two paragraphs' on this section should be added to improve and justify his methodology where he has introduced a lot of RS products from nowhere. For instance, how have these RS tools and methods been applied in other regions of data scarcity? What were the results achieved? Can the methods be replicated in the current study basin? Has the JGrass Newage (JGNA) model been applied elsewhere and what were results and strengths etc? This section should a major part of the MS and if not well captured then it can be concluded that the MS contributes very little value to hydrological science.

(4) THE STUDY AREA:

There are loose statements here and there that can be tightened and generalized. For instance, in line 5, one would ask: where is Bahir Dar where the river originates? Such loose statements assume and make the MS only fit for regional publication.

In my opinion, one elaborate map of topography (DEM), river network and stream gauges can be sufficient here. I am also sure with good GIS skill, and added topological data, the rainfall stations can still be added without making the map look untidy and congested. Or else, he may also elect to take a map of the catchment delineations

and the rainfall stations in the methodology, and use that chance to highlight the sub-catchments (better more than 10) where he wants to focus his results using a table as mentioned above already.

(5) METHODOLOGY:

On page 4 lines #12-15, the authors may want to choose one or two more applicable references of the co-author.

In page 5, Figure 2 needs simplifications and better explanations. The color coding shades used will not appear if the paper is printed in black and white.

Some parts in section 3.2.1 ideally belong to the introduction. Let the authors focus on the data-sets used and why they were used.

The reference Abara et al., submitted is completely out placed and may not be necessary at this stage of the journal.

There are many good ways of structuring this section in hydrology. Let the authors develop a simple and flowing structure from section 3.1. For example, section 3.1 can be titled 'Data and Methods'. Section 3.1.1 can be on 'Water Balance Modeling'. Section 3.1.2 can be on 'The Modeling System'. Section 3.1.3 can be on 'Data and Modeling Procedure' etc. The authors are free to choose what structure they want to adopt. As it is at the moment, there is too much information everywhere, a majority of which is not well captured and explained. Some content in section 3.2.3 on page 7 are not necessary and can be avoided generally.

Section 4 on calibration and validation can be renamed as section 3.2 and well elaborated as explained before. In this section, the authors need to JUSTIFY WHY the same data period used for calibration is also available for Validation? This may infer a technical limitation that can affect the model results purported by the authors.

(6a) RESULTS AND DISCUSSION:

Generally, the results are not balanced and well presented. The spatial maps dominate all the results. Well structured tables may provide more information considering the many catchments of study.

The first paragraph in the results section may not be necessary, or better be summarized.

The authors should find a way of presenting the maps in a nice, simple and clear manner. As they are at the moment, the polygons dominate the results. An elaborated table with selected catchment justified in the methodology can be good enough. Only one or two maps can be used here for visualization and overall balance of presentation of the results.

In line 23-24 of page 9, is the discrepancy small as mentioned? Could it be that the SM2R-CCI was not properly corrected? Please explain into details.

The legend for Fig 3 needs to be well placed and elaborated.

In section 5.1.1 of page 11, there is need for technical justification by the authors as this a very strong section of hydrology. (i) If GLEAM has had validation in other areas, with a good match with observations, then I it would be ok to use it for plausibility checks. However, as it stands, the New Age simulation of ET highly over- or under-simulate the ET fluxes. Should the results thus be fully trusted with these graphs?

The author can elect to present one or two of the Graphs/Figures but well elaborated and discussed into details. As it is, figure 4(b) is of limited value and would rather be discussed in the text or annexed.

Considering the model/data uncertainties, a KGE of 93% may be theoretically high if not good enough. There is hence a need for a strong justification of how the errors cancelled out during calibration and validation.

Fig 5 is not well represented. This can be avoided or the authors can choose the sub-catchments to illustrate 'a prior in the methodology section' as mentioned already. The

challenge here is that with the many sub catchments, the author does not seem to know how to cluster them in a consistent manner throughout the paper.

The results on page 14 can be summarized and well written. On table 2, is the final outlet of Upper Blue Nile located at El Diem with an area of 174 000km2? No idea!

Fig 6 on page 15 needs to be elaborated and well explained or else moved to the annex.

On page 16, it would be good to justify how the discharge in the entire basin was modeled. I.e. did you add/route all the upstream discharges and accumulated downwards? This as a technical consideration for the paper.

All the results needs to be discussed from a hydrological standpoint. This section is important for the authors to justify the publication, and provide key element of study that improves the knowledge in hydrology in such areas generally.

(7) CONCLUSIONS:

The paper needs to be summarized in the context of the study. Considering the uncertainties, the results need to be reported with this recognition i.e. ET values between 650-750mm were estimated for various sections of the basin etc

There is need for more conclusions about the challenges of the study and the methods generally. This will form a basis for recommending future studies in areas with similar data limitation.

As it is, the section is completely lacking and does not provide future research directions in hydrology.

(8) REFERENCE:

The references are not formatted to the Journal requirements as required by HESS. Check and realign all of them.