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Hydrology and Earth System Sciences
Discussions

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.

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We would like to thank reviewer 2 for the constructive comments. Below, you will find a point by point description of how each comment was addressed. The reviewer comments in bold font, and our response in normal font.

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 modelling 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:

We thank reviewer 2 for the appreciation of our work. When performing our studies we analyzed historical data, as any other hydrological study. We are, obviously, interested in forecasting the hydrological cycle components, but this necessarily relies on the availability of the meteorological forcings. It is possible to forecast (in the sense of meteorology) discharges (for instance) if we have had rainfall (and other meteorological) data. This assumes that we have access to real time data in the basin, which we do not have. More relaxed forecast, or better, projection, could be made after acquiring appropriate climate projections. But for this, to have a model system, which is validated for a given basin, is the first step. This is actually one of the goals of the present paper.

We will use as much as possible the suggestions given by the reviewer to improve our new manuscript.

Major concerns

(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.

We will improve as much as possible the layout (see detailed comments) and the writing of our manuscript. In page 15 there is not Tables. There are Tables in page 13, and we assume the reviewer refers to them. We will try to improve the quality of their caption to present the results in the most clear way we can.

(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?

Data limitation does not mean total absence of data. Certainly we have some precipitations and discharge data. However these data are in 35 locations for precipitation data in an area of 175 thousand square kilometers. Meaning, just a station every 5000 square kilometers or squares of around seventy by seventy square kilometers of side (on average). Convective processes generating precipitation can be as small as 10 kilometers square, so the optimal gauge network distribution should be as small as that, to capture all the relevant phenomena. Considering this fact, almost any region in the world is data-scarce, but some regions such as the Upper Blue Nile basin are even more hydrometeorological data-scarce regions than others. For discharge analysis, the numbers of hydrometer stations are very few (16 hydrometers) with a data set having lots of missing data and gaps. So for the objective outlined in the study, estimations of spatially and temporally hydrological information of the basin, UBN surely can be characterized as data limited basin.

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?

Yes, this is actually what it was done. We use all the data available to calibrate the model and we “interpolate” (propagate) all the data (hydrological information) by means of the model in the inner points. Actually, if with regionalisation the reviewer means statistical techniques, we did not use any of them. If the reviewer asks for the transferability of our approach, we can confirm that it can be extrapolated to any basin with similar or larger size.

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.

Even if hydrometeo data are available in fewer stations, satellites allow us to have rainfall forcing at a much finer scale. Partition of the basins in 402 parts is functional to use all the rainfall spatial information we have, in a trade-off with a reasonable computational demand. It also serves to accounts for the morphological structure of the river network, which, obviously counts very much in forming the hydrologic response. On the latter topic, the last author co-authored some papers that can support this fact. We will add a clarification on this in the revised manuscript.

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.

If we did not clearly communicate the objective of the paper, obviously, it is our fault. However, the objective of the paper is to estimate spatio-temporally distributed water budget of the UBN basin. Hence, the methodology followed and the results presented

all are for the whole basin, not for specific sub-catchments. When in-situ data is available, that specific sub-catchment is used to verify the performance of the model estimations. If the reviewer wants to select some catchments, we can provide part of this information in the complementary material of the revised manuscript.

c. Considering data uncertainties, would it be wise to believe the higher model reliability and hence results?

We considered ground measure as true. Untrusting them would lead us to absolute ignorance. However, the data provided by the model solution we used show that there is consistency between discharge gauges and rainfall estimates, give parameters that work decently also for the validation periods. Model and data are consistent (once the model is calibrated). That is all we can say. But what can we say else?

The authors need a good and elaborate justification of how the errors cancelled out during the simulation.

Errors do not cancel. When possible, any of the modelling components used was validated separately. We have checked the functioning of each of them in many other cases, as testify by our own literature, even if in those cases data were less scarce. In this specific case, precipitation from satellites is verified and corrected using the available few in-situ observations, storage (at least at the whole basin scale) is verified using GRACE data, discharge is verified at about 16 hydrometer stations. So we know that each component, besides implementing sound science, works fine with the appropriate data. That is what we can trust. When we calibrate hydrological model just on discharge data, parameters' values become a collector of uncertainties (a garbage collector, as some colleague calls it), but we assume that this is well understood and does not require a further disclaimer.

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.

We don't. We will clarify it. We used some part of the available data to calibrate the model at the main outlet, and used the other part for validation. In addition, the other data sets available in the interior hydrometer stations are used for validation the model capability to estimate discharge at each links of the river network of the basin.

In my opinion, the conventional way would be to divide the data-sets into two, one for calibration and the other for calibration.

Correct!

Could this be the reason for the good efficiency realised? The authors need to justify this methodology very strongly.

As we said, we did not use the same data for both validation and calibration. Hence, we believe that the reason for good model performance is due to the explicit characterisation of inputs strategies and the goodness of the modeling solutions adopted.

(1) TITLE

1 - 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.

That's OK for us. We changed the title to: "Modelling the water budget of the Upper Blue Nile basin using the JGrass-NewAge model system and satellite data"

(2) ABSTRACT

2 - 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.

We accept the corrections, and we revised the abstract following the reviewers' guidelines.

(3) INTRODUCTION

3 -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.

We will do it in the revised manuscript.

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

In the revised manuscript, we will try to synchronize them in shorter sentences.

5 - 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.

The intention of these two sentences was to avoid the description of various remote sensing (RS) products, and instead suggest that the readers should look for this information in milestone papers in the use of RS for hydrology. We will add further information of this in the revised manuscript. In the same mood, we do not want to add much information about JGrass-NewAGE that can be better accessed in previous papers by the same authors. . We will try to improve this section.

(4) THE STUDY AREA

6 - 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 subcatchments . . .

Thank you, we will improve our mapping and make a larger figure. As suggested by the reviewer, we will dedicate one map describing the DEM, river network, and stream gauges, with some places such as Bahir dar marked on it. Since the sub basins are the scale at which the water budget is estimated, we will also be maintain this map along the former .

7 - (better more than 10) where he wants to focus his results using a table as mentioned above already.

We do not think that adding more catchments' details will be useful for the readability of the paper. However, DEM, important shape files to be used in GIS, and the list of catchments details will be provided as complementary material.

(5) METHODOLOGY

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

The lists of papers cited are describing different modeling solutions, each for one component of the JGrass-NewAge system. Since all components are used, it is important that we cited all of them. However, we will revise the sentences for making it easier to read.

9 - 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.

Thank you, we will improve the text and change the color shades.

10 - 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.

Actually what has been written in the first and second paragraph was the explanation why and how we used SM2R-CCI precipitation data. In any case, we will revise it.

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

We think it is not a bad thing, and let the citation.

12 - 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.

We realized that sub-sectioning of section 3 and 4 went wrong. New subsections will be:
3 Methodology
3.1 JGrass-NewAGE System setup
3.2 Precipitation
3.3 Evapotranspiration
3.4 Discharge
3.5 Water storage
3.6 Calibration

4. Results and discussion
4.1 Precipitation
4.2 Evapotranspiration
4.3 Discharge
4.4 Water storage
4.5 Water budget closure

5. Conclusions

We think that in this way there will be a clear relation between the topics of the two sections (section 3 and 4). So we argue that the approach followed in the paper is better than the one suggested by the reviewer. Obviously we will try to improve the description and the discussion. However, the reviewer should understand that dealing with the whole hydrological cycle is a complex task that requires attention. A detailed understanding of all its parts cannot be obtained without reading the other papers on JGrass-NewAGE where we cover a huge amount of work and testing.

13 - Some content in section 3.2.3 on page 7 are not necessary and can be avoided generally.

Section 3.2.3 contains totally twelve lines. It is very difficult for us to understand what we can avoid to say. We give information about the algorithm we use for reproducing discharges, and the validation method. We believe that this information is necessary.

14 - 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.

Regarding about section renaming, please see specific comment 12. We did not use the same data for calibration and validation, as described in major comment C.

6. RESULTS AND DISCUSSION

15 - 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.

We think that one figures convey more than thousands words if well understood. Evidently we were not able to convey clearly their meaning. We will work to improve figure captions and comments. Most of the data are (and will) be provided as complimentary material with some table of summary for what it is feasible to do. Finally all of our procedure are based on open software and can be repeated step by step by any researchers.

16 - The first paragraph in the results section may not be necessary, or better be summarised.

Thank you, we will summarise it.

17 - 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 visualisation and overall balance of presentation of the results.

Given our objective, the presentation of our results without maps is impossible. We limited one, if not two, figure (plot) for each component. Data are averaged over a subbasin and there is not internal spatial variability in the output. So it is clear that "polygons" stand out.

18 - 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 difference between annual long-term rainfall value of 1900 mm and 2049 mm, given by different data sources, can be considered small. Besides, if one considers the uncertainty pertinent to each data sources and estimation method, s/he should conclude that the difference is acceptable.

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

We will do it.

20- In section 5.1.1 of page 11, there is need for technical justification by the authors as this is 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 detail information about the GLEAM is provided in the methodological section (page 11 line 17 to 27), and obviously had several checks. The check of the product was not for a given area and not based on accurate hydrological modeling. Hence

we would not say that NewAGE over or under estimates the budgets. This assumes that GLEAM is the truth. As strictly mentioned in the methodological section, both of them are estimates, which differ but are somewhat coherent. NewAGE, in any case, forces mass to be conserved that brings into the game the whole set of hydrological measurements, and, in our opinion, can be trusted more.

21 - 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.

Figure 4b will be discussed more in detail in the text of the revised manuscript.

22- 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.

We believe that KGE is high because our model is good, and, bedsides, based on available measure, the components were tested separately from the whole, when possibile. So rainfall estimation was estimated with rainfall measurements (we dedicated a paper to this). Storage was estimated against GRACE data, and so on.

23 - 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.

We agree that we need to explain better what is shown in Figure 5. It seems that we did not clearly shows what we wanted. We modeled daily discharge at all river links of the basin for 16 years. The results were presented in two ways: (1) Time series simulations at few links of the river network where we have observed discharge to compare with.. These comparisons are connected to the basin river network map to show the locations

of these links within the basin (i.e. figure 5). The names of these locations is given in the caption, and information about them is also given in Table 2. (2) In figure 6, we presented a snapshot of discharge estimates for any river links of the basin. To this figure actually correspond a table which will be added in the complementary material of the revised manuscript. We think that these summarises and is the best ways to communicate our results effectively but we agree that the caption and the text can be very much improve to promote the reader understanding

24 - The results on page 14 can be summarised 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!

We will revise the section. Yes, it is the outlet of the basin. Probably we will add a column to the table to clarify further these results.

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

Please see specific comment 23. We will add appropriate comments.

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

Thank you for this, and we will add an explanation on how we modeled the discharge routing in the methodology section.

27 - 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.

Thank you for the suggestions you gave all through the paper.

7. CONCLUSIONS

28 - The paper needs to be summarised in the context of the study. Considering the uncer- tainties, 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.

We will try to improve our conclusions being more specific on uncertainties, and remarking the challenges we met in our studies. However, we will not take responsibility to indicate future research directions. In our opinion we already show something that is a little beyond the state of art of the discipline. These improvements include the use of various satellite sources for verifying and/or assessing all the water budget terms, and the production of the same water budget at various time scale, verifying mass conservation through the cycle. Besides, we produced the software to obtain it, we made it available, and everybody can replicate our results.

8. REFERENCE

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

References formatting have corrected accordingly.

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