

The authors have addressed some comments/suggestions and made changes to the paper, which has improved its quality considerably. However, I still have some major concerns:

1. Based on the following response and the revised manuscript, I cannot agree with the authors that their contribution was linked all the components in one model. Because there's no field observation to validate the modeled water components (except runoff) in this manuscript. Evapotranspiration (ET) was estimated using the Priestley and Taylor (PT) Formula. But GLEAM, which was also estimated based on the PT method, was adopted to validate the estimation. If estimates of each component have large uncertainty, who believe the final output?

3. The authors claimed that the JGrass-NewAGE system are described in a series of papers and not re-discussed in this manuscript. What's the difference between this study and the previous papers? What's the main contribution of this work?

The previous papers contain description of the single components that were validating separately on other catchments of small size where there was relatively abundant ground meteorological information. Those papers cover the informatics of the system, DEM treatment and river network schematization, and finally radiation, runoff, and snow modeling.

In this paper those components are linked in a unique modelling solution and work all together cooperatively to solve the water budget closure.

In addition, another important contribution of this paper is the application of the obtained modeling solution in a large basin using various data (satellite and in-situ), which is what NewAge was originally developed for.. In poorly gauged area, modeling in our opinion, working in this way is the only way to obtain spatially distributed water resource information that can be used reliably for management purpose.

2. As mentioned in my previous comments, the discussion section should be enhanced. However, the authors revised limited content in section 4 (Results and Discussion) in this revision (track change in 'hess-2016-290-author_response-version1.pdf'). For example, it seems that the authors kept discussing some water components in other submitted manuscript. Since the authors think combine modeling all the water components is important, why not focus on this study and discuss the possible limits of such a solution? I know the study area is data-scarce and validation using field observation is impossible. However, reasonable discussion on the comparison and the possible errors may persuade the readers (e.g., see specific comments 5, 9, 11, 14, 15, and 18 for detail). Otherwise, the readers may question about the modeled water components and finally your method.
3. Both reviewers mentioned figure quality. However, the authors may have paid little attention. For example, although units were added to axes in Fig. 1, units for axes were still missing in the rest of figures (e.g., Figs. 3, 4, 7, 8).

Following specific comments may help the authors understanding my concerns and improving the manuscript.

The numbers in front of the comments indicate page and line number.

1. 1-6. ‘to obtain the estimates of all the components of the hydrological cycle (precipitation, evapotranspiration, discharge, and storage)’. It would be better to revise the claim, because precipitation, evapotranspiration, discharge, and storage are main components. For example, interception and infiltration are also components of the hydrological cycle, which you did not address.
2. 4-3. ‘Specifically studies’ contains grammatical errors. I encourage the authors to check the entire manuscript carefully to avoid such mistakes.
3. 4-8. Please correct the unit for temperature.
4. 5-3. Table 1. I cannot found any description on ‘JAMI’ or ‘three temperature’ in the manuscript. If you decide putting such information in the manuscript, please be sure the readers can understand it or find relate context.
5. 8-5. Eq. (3). In table 1, the author said PT method was used as one method to estimate ET. However, ‘S(t) and Smax’ were added to Eq. (3). Is Eq. (3) valid for this study or is it used for all JGrass-NewAGE application? Furthermore, what’s the advantage of using water storage information when estimating ET, especially in data-scarce regions?

In addition, GLEAM estimates are based on the PT method. Can it be used to ‘validate’ Eq. (3)? Which version of GLEAM did you use?
6. 9-9. Are ‘the ADIGE model’, ‘the well-known HYMOD model’, and ‘The NewAge Hymod’ the same? If yes, please consider unify the description.
7. 9-12. I’m confused by the description that ‘The main inputs for the ADIGE model are J(t) and ET(t)’, because ‘Q is modelled as functions of basin water storage’. How did you get the water storage? What are the five calibration parameters?
8. 10-17. ET validation is questionable. See specific comment 5.
9. 10-21. ds/dt validation. Similar to ET validation, lacking of discussion on GRACE product and the modeled ds/dt. Did you use the GRACE product directly or

perform any correction? As reported by studies (e.g., Long et al., 2015, *Water Resour. Res.*, 51, 2574–2594), GRACE data are noisy in smaller basins less than the GRACE footprint ($\sim 200,000 \text{ km}^2$), as well as in areas with intensive irrigation. Considering the UBN is approximately $176,000 \text{ km}^2$ and the highlands have high water demands for irrigation, the product may include typical errors. The author should discuss such uncertainty and the possible impact on the modeled ds/dt .

10. 10-26. The headings in section 4 are the same as those in section 3, and the authors claimed that in this way there is a clear relation between the topics of the two sections. I cannot agree with them. For example, in rainfall section, the spatial distribution was described and then compared with some published results. But for ET section, both spatial and temporal distributions were presented, as well as ‘validation’. I don’t think the headings in results section reflecting any useful information.

11. 11-1. Table 2. The unit for SM2R-CCI’s spatial resolution is missing. Could you provide time periods for these data used? Little information about the used data was presented.

In section 4.1, can you discuss what’s the difference between corrected and uncorrected SM2R-CCI products? That is how systematic error (bias) of SM2RCCI affects rainfall amount, as some ungagged basin has no in-situ observation to perform the correction.

12. 14-6. The section is to ‘validate’ NewAge ET. I’m not sure why the authors talk about GLEAM estimation.

13. 14-10. A better correlation between NewAge ET and GLEAM is because they both estimated using the PT method.

14. 14-25. Table 3. It’s difficult for readers to know what these parameters represent.

Furthermore, α_{PT} has a value of 2.9. It’s relatively high compared with the commonly used value (1.26) in the PT method, or the value (1.5-1.8) recommended for estimating ET in more arid regions (ASCE. 1990. *Evapotranspiration and irrigation water requirements*. ASCE. Manuals and reports on engineering practice. No. 70. New York, NY, USA). In this case, ET may be

overestimated. It can be seen from Fig. 4 and Fig. S3 that the NewAge ET higher than GLEAM and MODIS ET, especially the peak value. If ET is overestimated, runoff should be underestimated when precipitation unchanged. Fig. 5 did show that in most cases, the modeled runoff is smaller than the observation, and obvious difference occurred also at peak values. The authors should discuss such physical processes that may cause model uncertainty. Only insightful analyses and discussion on the mechanism behind can highlight the scientific merit of the manuscript.

15. 15-1. Table 4. There may be something interesting, i.e., KGE varied with basin area and may have a poor correlation with area. It's often taken for granted that a hydrological model will perform much better in relative smaller basins than in larger ones. Can you discuss why some times the JGrass-NewAGE System performed good or bad in sub basins with similar area?
16. 16-1. Fig. 5. Is there any observation used in Fig. d?
17. 18-13. What does S mean? Please consider defining the abbreviation.
18. 21-1. Fig. 9. It would be better to change the water components to percentage.
19. 22-1. Fig. 10. I'm curious why ET was so low in the hot season. Supposing most of the rainfall infiltrated into the deep soil (high ds/dt values) in the hot and wet seasons, can they evaporate easily in the dry season? Again, more discussion may be required to persuade the readers about the modeling results.