

## ***Interactive comment on “Modeling the Changes in Water Balance Components of Highly Irrigated Western Part of Bangladesh” by A. T. M. Sakiur Rahman et al.***

**L. Eamen**

leila.eamen@usask.ca

Received and published: 4 December 2017

This manuscript describes the application of discrete wavelet transformation (DWT) and different forms of Mann-Kendal test to study changes in water balance components (WBCs). The authors also develop a “wavelet autoregressive moving average (ARIMA) model” to forecast WBCs. The contribution of the manuscript seems to be detecting trends and identifying periodicities in WBCs along with forecasting them after removing the noise from the time series. The manuscript is statistical than hydrological and I would say that hydrological concepts are insufficiently addressed and not fully developed. Moreover, there are some theoretical inaccuracies and confusing statements

C1

(especially in hydrologic side) that undermine the quality of this manuscript. Overall, the authors address an interesting subject; but in the current form, there are concerns and shortcomings that warrant major revisions.

Below I summarize some constructive suggestions for improvement:

Theoretical issues:

1- There is a confusion in the paper about the concept of “Water Balance” and its “Components”. Water Balance Components (WBC) and some other parameters are frequently used in awkward and confusing sentences. As an example, potential evapotranspiration (PET), which is named as one of the WBCs in line 91 is called “the key parameter to estimate water balance components ...” in line 119. I strongly recommend that the authors provide the Water Balance Equation, briefly introduce Water Balance Components, and define which components they consider in their study, clearly. They may explain these concepts at the beginning of the “Methods” section (section 2.3). They may also mention the reason(s) for selecting each WBC.

2- Following the previous comment, both Potential Evapotranspiration (PET) and Actual Evapotranspiration (AET) are considered, surprisingly, as components of water balance equation (for example in lines 91, 265, 325) without any explanation on their application and role in the equation. However, the application of these parameters in Water Balance Equation is different and they cannot be considered both at the same time. I would also suggest the authors revise their manuscript to ensure that no confusing sentence remains on this subject.

3- In line 145, the authors stated that “When rainfall is greater than PET the soil always remains full of water and ...”, which is an inaccurate statement. I understand the authors try to explain the concept of surplus; however, surplus occurs when the soil becomes saturated and infiltration is hardly possible.

Title and Abstract:

C2

4- The authors should perhaps reframe the title to better reflect their work. The present title implies that the study is mainly concentrated on the interaction between changes in water balance components and intensive irrigation in Western Bangladesh.

5- I would recommend that the authors name water balance components that they consider in this study in the abstract. They may provide then a summary of the methodology and results in a more organized way.

6- I was wondering whether the authors apply ARIMA or ARMA models in their study. In case of having ARIMA, which stands for "Autoregressive Integrated Moving Average" they should revise the statement in line 17.

7- Line 34: The statement "... findings of study can be used to improve water resources management ..." is too generic. Please clarify in what respect this study can improve water resources management in the highly irrigated area.

The Structure:

8- In general, the paper has no flow and each section seems to be a separate part without proper connection to the other sections. I think the authors should improve the structure and flow of their manuscript.

9- I believe that the "Introduction" should significantly revised. For instance, the literature review on periodicity and using wavelet transformation is only limited to few sentences. The authors can elaborate more on what the previous researchers have done and how this study differs from previous attempts.

10- In section 2.2, "Data", I would suggest that authors provide the time duration they used in this study.

11- Headings are awkward and in some cases poorly selected. For example, in lines 265 and 325, (sections 3.2.1, and 3.2.2) it would be better to replace "PET" with "Potential Evapotranspiration" and "AET" with "Actual Evapotranspiration" respectively.

C3

12- In section 2.3, "Methods", I would suggest that the authors provide a general overview of their methods and then explain each section in detail rather than starting the section immediately with a sub-heading.

13- Section 3, which seems to provide results of the study, is poorly structured. Sentences are awkward and poorly written, which makes it difficult for readers to follow.

14- I was wondering why the authors consider the section "Model Selection and Forecasting Ability" as a sub-heading of Results section (line 352). The methodology of the modeling and considerations regarding model selection should be discussed in the "Methods" section.

15- Following the above-mentioned comment, section 3.3 (lines 352-416) contains the model selection, methodology, results, and some discussions. The section is too long and without proper flow. I suggest the authors break this section into methodology, results, and discussion to help readers better follow their work.

16- The authors use passive voice and active sentences alternatively in the manuscript. They may re-write these complicated parts. For example lines 293-294.

17- In the "Summary and Conclusion", the authors mostly repeat some parts of the manuscript. I would expect to read a more conclusive summary and conclusion. For example, in Lines 447-449, (as mentioned earlier in comments on the Abstract) the authors stated that results of this study "can be incorporated to water resources management plans ..."; but they didn't explain how this incorporation would take place. I suggest that authors add some explanations to the manuscript to clarify in what respect their work will affect water resources management in the highly irrigated lands.

Other Comments:

18- Lines 43-44: confusing and awkward statement: "Two important climatic variables like rainfall and PET that derives from the climatic variables are the main inputs in the water balance modeling". Please re-write the sentence.

C4

19- Lines 74-77: Please re-write the statement.

20- Lines 81-82: "... most of studies were limited to detect trends or forecasting of rainfall and temperature and few studies on PET and water balance." References are required.

21- In section 2.1, it is stated that rice, the main crop cultivated in Bangladesh is mainly rain-fed or irrigated by groundwater resources (lines 104-106). Unfortunately, the authors have not clearly explained the relation between their study and irrigated area or even irrigation water demand in the study area. They may define how their work will affect the "Highly Irrigated Western Part of Bangladesh".

22- Lines 144-147, as acknowledged earlier, the statement needs theoretical revision. However, references are required for the definitions of surplus and deficit.

23- For the statement in lines 147-151, on the AET and its "calculation", references are required.

24- Lines 398-400, awkward sentence. Please re-write this sentence.

25- In general, the writing can be significantly improved. The manuscript suffers from several poorly written sentences, awkward expressions, and some grammatical errors. Some of the sentences in need of being re-written are mentioned earlier. Some other examples include:

a. Line 34: "... findings of study ..."

b. Line 35: "... in highly irrigated ..."

c. Line 48: "... attracted attention for Bangladesh."

d. Line 53: "Almost most of the studies ..."

e. Line 408: "...verses...". This sentence is also long and confusing. The authors may re-write this statement.

C5

Overall, the subject of this manuscript is interesting, and of relevance to HESS readership. Therefore, following major revisions, some of which are mentioned above, it has the potential to turn into a good publication.

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-523>, 2017.

C6