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Hydrology and Earth System Sciences
Discussions

Interactive comment on "The effect of water storage change in ET estimation in humid catchments based on Budyko framework and water balance models" *by* Tingting Wang et al.

Anonymous Referee #3

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General comments

This study analyzes the effect of changes in water storage at annual and multi-annual timescales for humid catchments in China. The authors perform a quantitative comparison of evapotranspiration (ET) estimates from the water balance, Budyko's framework, and two other models. They show that it is erroneous to obtain annual ET estimates from the water balance when neglecting changes in storage. I find the approach interesting, but numerous points need to be addressed. Particularly, the quality of the text needs to be greatly improved before being able to thoroughly assess the scientific merit of the study.

Specific comments

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It is not right to assume the validity of Budyko for annual timescales. The sentence: "Subsequently analysis on annual water-energy balance have proofed that the Fu's equation can be used in both long-term and annual water-energy balances in non-humid catchments (Yang et al., 2007) and humid catchments as well (Tekleab et al., 2011; Xu et al., 2013)", is not accurate. Both Tekleab et al. (2011) and Xu et al. (2013), together with many other studies (e.g. Gentine et al. 2012; Roderick and Farquhar 2011) indicate that the validity of Budyko's framework requires steady-state conditions, which are generally achieved by using data at time scales significantly longer than 1 year.

I think it would be better if the storyline focuses from the beginning on the issue of neglecting ΔS for ETwb.

The approach for analyzing the inter-annual variability of ET needs to be clearer (equations 8 and 9). Is the effect of ΔS not accounted for? I believe it would be better to not include this section in the paper, and consequently Figure 10b. The authors already convey your point about the higher variability of ETwb compared to the other estimates of ET in Figure 10a. They could strengthen this argument by showing the histograms or pdfs of P, PET, and ET. It may be also possible to identify years with $\Delta S>0$, for which ETwb > PET. I would also recommend plotting the time series of PET in Figure 8.

Section 4.1: It seems trivial to calibrate w with observed P, PET, and Q, and then compare to ETwb = P – Q. Is w calibrated for each catchment based on long-term mean annual P, PET, and Q? Do you have any arguments for the underestimation of ET estimates from the abcd model at multi-annual timescales (ETabcd)?

Section 4.3.2: As I understand it, ETwb – ΔS better represents actual ET than ETwb. Therefore, I think it would be more intuitive to compare ETwb – ΔS with ETBudyko, rather than ETwb with ETBudyko + ΔS.

Technical corrections

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The text needs to be improved overall.

Define what you mean by humid and non-humid catchment at the introduction. References for studies about non-humid catchments are not really accurate; data from these studies also include humid catchments. Missing reference to Greve et al. (2016) for studies considering ΔS.

Caption of Figure 1: The aridity index should be < 1, instead of > 1.

Criteria

1. Does the paper address relevant scientific questions within the scope of HESS?

Ok.

2. Does the paper present novel concepts, ideas, tools, or data?

Yes, but it could benefit from a different focus of the study. It is interesting for quantifying the influence of water storage changes in ET estimation, more than its importance being a new finding.

3. Are substantial conclusions reached?

Ok.

4. Are the scientific methods and assumptions valid and clearly outlined?

The main issue I find is with Budyko's assumption of steady-state conditions.

5. Are the results sufficient to support the interpretations and conclusions?

Ok.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Data is not accessible. I would strongly encourage to report all relevant data, e.g. long-term average P, PET, Q, catchment coordinates, etc, in supplementary material. The

methods could also benefit from a more detailed explanation.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes. Some additional references should be included, e.g. missing reference to Greve et al. (2016) for studies considering ΔS. Include Zhang et al. (2004), since Fu (1981) is in Chinese.

8. Does the title clearly reflect the contents of the paper?

Yes, but it should be written properly, e.g. "The effect of changes in water storage in humid catchments on evapotranspiration estimates from Budyko's framework and water-balance models".

9. Does the abstract provide a concise and complete summary?

Improve text. It would benefit from a more objective/quantitative way of presenting the results. Statements like "works fine" and "much improvement" are not too informative.

10. Is the overall presentation well structured and clear?

The structure is fine. It may help storyline to have a stronger focus on the annual over multi-annual timescale since the beginning of the paper.

11. Is the language fluent and precise?

No. It definitely needs to be improved. At the current state it is difficult to assess the scientific merit of the manuscript.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Ok.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

It would be useful to have a more rigorous test for the statistical significance of the improvement with size shown in Figure 9. I would recommend to exclude figure 10b and the corresponding methods section.

14. Are the number and quality of references appropriate?

Could be improved. See comments from other reviewers.

15. Is the amount and quality of supplementary material appropriate?

No. See for example point 6.

References

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