

Interactive comment on “Evaluating the hydrological consistency of satellite based water cycle components” by O. López et al.

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Dear authors,

This paper describes an evaluation method to explore correlation of satellite-derived water budget components P-ET and Terrestrial Water Storage. It uses a spherical harmonic analysis to analyse correlation and differences between multiple time series of P-ET and GRACE Terrestrial Water Storage. The method uses consistent GPCP input on P, and three different ET methods. The method results cannot explain differences of the analyses in the three large-scale basin studies.

This study uses a novel approach to estimate differences between two time series. The fact that this analyses does not lead (yet) to valuable results, is no reason to disqualify it in any way. Therefore, it makes this paper a valid and potentially useful

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addition to the journal. However, the descriptions and approach are, in my opinion, not fully crystallised and require more research. Some of the descriptions are also rather confusing and need a better structuring (see further comments below). I get the feeling that recommendations described in this work should have actually be part of the paper.

The whole term 'hydrological consistency', which is everywhere in the paper, e.g. the title, is not very clearly explained. Could you consider a better way to describe what you want to research? For example, 'the ability to balance the water budget' or something containing the words 'hydrological closure'?

In terms of concise descriptions: in my opinion, this study knows too many research questions and too little answers. It needs serious work on structure, correct descriptions and conciseness. The authors describe several paragraphs saying: 'the objective of this work', or 'a secondary objective of this work was..', or 'one key motivation of the study.'. Or they reword their aims in questions, such as: 'how accurate are the hydrological components derived from satellite observations?' or 'is hydrological consistency achieved with a particular product....?', etc. To me, that makes it confusing. Can you please clearly state the objectives and motivation at first, and then relate back to exactly those? That would make this work better readable. Furthermore, the many different aims and questions also makes you wonder about the conciseness and correctness of this study. For example, the questions "how accurate are the hydrological components derived from satellite observations?" cannot be answered with the results of this study. If all these research objectives and questions would be compiled into only two research questions, I think the paper would be more understandable.

It is unclear what the added value of the spherical component analysis is. After all, correlations between water budget components can also be made in a different way, and the spherical component analyses does not lead to any new insights. Moreover, the lag between some data make the results of the analysis method less obvious. The method does not lead to any quantifications of if the water budget is in balance, or by how much it is off (for example, in percentages of the total water budget). That makes

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this study for me hard to judge: it evaluates water budgets but this does not lead to any new insights (unless you describe that part better), and cannot answer obvious explanations for imbalance of a water budget.

It is unclear why the focus is put on the ET component. You should pitch that better. After all, any uncertainty of the P component would result in larger uncertainty. I think the discussion and conclusion of this study need to point out that comparisons in catchment study need to be undertaken using the regional information on hydro(geo)logy and ground estimates of P, ET and streamflow. My guess is that you want to say that, despite being the second-largest component of the water budget, ET is most uncertain?

The word groundwater is mentioned in the description of GRACE data. However, it is surprising that the word groundwater is not mentioned when discussing the phase lag to GRACE data, nor the separation of the $P-ET(=Q)$ into streamflow and groundwater flow. One could for example compare global data of baseflow (BFI) and look if these compare to the differences in lag. See for example the wonderful work of Beck et al. (2013): Beck, H. E., A. I. J. M. van Dijk, D. G. Miralles, R. A. M. de Jeu, L. A. Bruijnzeel, T. R. McVicar, and J. Schellekens (2013), Global patterns in base flow index and recession based on streamflow observations from 3394 catchments, *Water Resour. Res.*, 49, 7843–7863, doi:10.1002/2013WR013918.

It is also surprising that nothing is said on snow storage in the discussion on P-ET.

Furthermore, the description of input data is not clear. Especially the ET part should contain what part of the data we use. For example, do we use PET from MOD16 or AET? Also, MOD16 contains an unclear description that first suggests it is a Penman-Monteith method, and then suggests it is Priestley-Taylor. Furthermore, the spatial detail in Table 1 on MOD16 is not correct to my knowledge and resolution should be 1km (at least if the data from Mu et al from <http://www.ntsug.umt.edu/project/mod16> is used). See more detailed text comments below.

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Methods and results are in my opinion too intertwined, and should be separated more. For example, in the results, there are some descriptions of the basins. For example, descriptions of the CRB, ASB and LEB basins contain texts that should in my opinion be put into 2.5 Study Regions.

I am not an expert in spherical harmonic analyses, so I cannot judge on the correctness of the method. However, I think it would make the text a bit clearer if the term 'degree' (l) would be explained a bit more in detail, so non-experts in spherical harmonics would also understand. In that way, your plots 4-7 would be easier to understand.

A weakness of this paper is that it does not incorporate uncertainty. After all, estimates of ET using ground observations already contain quite a bit of uncertainty (see e.g. Westerhoff, 2015, with work on Uncertainty of Penman and Penman-Monteith estimates). Another weakness of this paper is that it is not clear what specific ET has been used (PET, AET, P-T, P-M). A quick fix could be a better description of the input data. However, a discussion of different methods used (e.g. P-T or P-M, or PET or AET etc) should be clear on pointing out differences in methods. It would be unfair to judge that satellite data does is not hydrologically consistent, if the ground-estimates already are so uncertain and if the different methods have caused this uncertainty. It is also unfair to put 'the blaim' on ET, because P is a much more likely candidate for this uncertainty. You need to describe better why ET is so important in this analyses. My guess is that you want to say that, despite being the second-largest component of the water budget, ET is most uncertain. Can you derive uncertainties of all estimates? This study would be much stronger if it could quantify what absolute values of inconsistencies we are talking about.

If the document would be structured better, I think it would improve the quality and probably I would understand better what the exact goal is of this paper. Reducing the number of research questions/objectives would probably clarify why this paper should be published. Therefore, I recommend a major revision.

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More detailed comments:

Page 1, Line 10: 'regional-to-global', I think it should be just 'regional to global'. Check throughout the document.

Page 2, Line 14: 'in-situ' should be 'in situ'. Check throughout document.

Page 2, Line 16: insert space between 'low,independent'.

Page 2, Line 18: 'land-surface' should be 'land surface'

Page 3, Line 16: I think you should mention that ground-observed estimates of ET also contain uncertainty.

Page 5, Line 1-11: Are you using PET, AET, PLE or LE?

Page 5, Line 10. You should use the correct definition. Mu et al use a P-T estimate for plant transpiration. Consider removing this sentence, since it is confusing and it now reads as if Mu et al use a P-M and a P-T approach for ET.

Page 5, Line 13-19: what are you using? AET?

Page 5, Line 20-26: please mention that GLEAM is AET.

Page 6, Line 9-15: starting with mentioning 'although not used for...' confuses the reader. Please start by saying what it is, what is has been used for in this study, and what is has not been used for.

Page 6, section 2.5. Some of the texts of methodology that described the basins, should be in here.

Page 7-8: Please describe what the term 'degrees' is and what it means. Since you rely on it quite heavily in your figures 4-8.

Page 9, Line 20-25. This is quite a statement. You assume that clear challenges are needed to reach hydrological closure using satellite data. That is already a paper conclusion. I think you should include something here that mentions that this is nothing

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new (i.e. closing a water budget has always been a challenge, with or without satellite data) and that the other uncertainty of ground-observed estimates and models of ET is still under-explored.

Page 13, Line 6: 'interrelated' should be 'inter-related'

Page 13 - Page 15, Discussions. I had a really hard time reading through the discussions. This should be better structured in topics.

Page 22, Table 1. The resolution of MOD16 is 1km on the sinusoidal MODIS grid if you take Mu et al data. It is unclear what the 0.05 arc-degree is doing here.

Page 24: Figure 2: it would have made more sense to compare P-ET with Q in these studies. The color scale in the figures could use a legend (is this rI?).

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