

Reviewer's Comments and Suggestions on Manuscript Entitled

“Intercomparison of freshwater fluxes over ocean and investigations into water budget closure”

by Marloes Gutenstein, Karsten Fennig, Marc Schröder, Tim Trent, Stephan Bakan, J. Brent Roberts, and Franklin R. Robertson

General Comments:

It is an overall well-written manuscript comprehensively addressing a truly important subject on freshwater fluxes over ocean and (the associated) water budget closure by using and intercomparing seven credible and lengthy globally-covered products of evaporation and/or precipitation (six generally satellite-based datasets, along with one reanalysis). This reviewer would like to acknowledge the authors for their willing to challenge themselves and tackle such a subject that would almost (if not absolutely) guarantee and require tremendous efforts and time, and persistent commitments, let alone equipping with solid expertise and knowledge, and great ideas and insights.

However, a few relatively major concerns may need further elaboration or revisions. Several minor revisions are also suggested.

Finally, this reviewer considers that the authors deserve a solid credit, again for their tremendous efforts and crucial works. This reviewer will highly recommend this manuscript for publication by Hydrology and Earth System Sciences (HESS) (an Open Access Journal) should the following suggested revisions be properly conducted accordingly.

Major Comments/Suggestions/Revisions:

1) E-P uncertainty involving E uncertainty and P uncertainty:

The uncertainties of E-P of different sets of products involved and targeted in this study should mainly be depending on the respective uncertainties of the P and E products, e.g., for $IFREMER-G(E-P) = IFREMER(E) - GPCP(P)$, the uncertainty of $IFREMER-G(E-P)$ should supposedly be an added sum of the uncertainty of $IFREMER(E)$ and the uncertainty of $GPCP(P)$. These respective E and P uncertainties, however, are not among the main focuses of this study (as the authors have indicated). It'd be very understanding and foreseeable that such (additional) tasks of investigating and analyzing (or may even need to newly generate or estimate) the respective P and E uncertainties would add up another level of difficulty and efforts, especially if/when researchers were not directly involved in those P and E productions, and the currently available related uncertainty info's have been quite limited (which have also been revealed in this manuscript). As for “*We conclude that for a better understanding of the global water budget, the quality of E and P data sets themselves and their associated uncertainties need to be further investigated*”, this reviewer has fully agreed on this critical “conclusion” finding, which, honestly speaking, has also been “expected” during the midst of review. It might also be fair and reasonable to alternately say “*for a better understanding of the global fresh water (E-P or P-E)*”

distributions and their uncertainties, the quality of E and P data sets themselves and their associated uncertainties need to be further investigated". As mentioned in the general comments, this reviewer has highly acknowledged the authors' tremendous efforts and time invested in this truly important and genuinely challenging "project" (must have involved multiple tasks!), it would be irrational if this reviewer would suggest the authors to further consider performing similar rounds of uncertainties analyses on P and E, respectively. However, this reviewer would like to suggest that the authors may consider drafting one additional paragraph or a set of sentences (and add it in, e.g., near the end of the Introduction section) including brief justified reasons (why the P and E uncertainties are not in the main focuses of this study) and useful messages (P and E, respectively, are by all means critical for the E-P quality and uncertainty). Of course, the authors' current concluding remarks may remain intact unless the authors may also wish to elaborate a bit more accordingly.

2) E-P uncertainty involving ∇Q and ΔW (a comment triggered by seeing the assumptions made in Eqs. 1-3)

The assumptions of neglecting ∇Q or ΔW for global or regional scales have triggered this reviewer wondering about their potential impact on the E-P uncertainty. Here's the comment that the authors may feel free to respond or not (**Optional**).

In the case of omitting ∇Q or ΔW , it might cause two folds of potential impacts, hypothetically: 1) if neither ∇Q nor ΔW would carry uncertainties, then the currently estimated uncertainty of E-P in this study could have been overestimated since part of the estimated uncertainty might have been implicitly contributed by the being-omitted "true" amounts of ∇Q or ΔW (even though being small, but had been neglected), 2) if either/both ∇Q or/and ΔW would carry uncertainties, then the currently estimated uncertainty of E-P in this study could have been underestimated, especially if/since the uncertainties of ∇Q or ΔW (if available and "certain") would be sufficiently large (greater than their true amounts) and add an additional net amount of uncertainty into E-P, otherwise. The E-P uncertainty could have still been overestimated as in "1)" if the true amounts of ∇Q or ΔW are larger than their respective uncertainties.

3) On line 272 (related to Fig. 1):

A seemingly correct but actually questionable (not quite proper) statement that needs a "major" attention and revision.

"A and B shows that the E-P pattern is mainly determined by P, as there is less spatial variation in E."

It should be more accurate to state *"A and B shows that the E-P pattern is mainly determined by P in the tropical and high-latitude regions, but determined by E in the subtropical regions."* It's also the absolute magnitudes/intensities of E or P, not just only their spatial variation that would matter. Actually, if "warm" color (red) and "cold" color (blue) were applied for E (panels C and D) and P (panels E and F), respectively, such crucial features (reviewer's points) would have been better revealed. Perhaps, the authors may genuinely consider it.

4) The authors would sometimes describe, elaborate or discuss the features or findings shown via figures or panels, but without consistently and explicitly citing them along the presentation/writing.

Here are a set of examples related to a few **Fig. 3 panels**:

- **On line 310**: may add in “*In panel A*” to “*E data from HOAPS, IFREMER, and OAF flux are much closer to each other...*”
- **On line 325**: may add in “*In panel C*” to “*Apart from HOAPS E–P in March–April....*”
- **On line 337**: may add in “*In panel E*” to “*The three P data sets yield inter-annual variations with amplitudes...*”
- **On line 339**: may add in “*In Panel F*” to “*Apparent agreement is found among all E–P anomalies...*”

It’s also thus suggested that authors may want to apply similar revisions elsewhere consistently. Such changes should help reader’s reading and comprehension effectively.

Minor Revisions:

1) On line 46:

Suggest change

“*are the most widely used data sets.*”

→

“*are among the most widely used data sets.*”

2) On lines 58-59:

Suggest change

“*With W the total column water vapor and ∇Q the moisture divergence, i.e., the amount of moisture removed by-advection from the considered volume.*”

→

“*With W the total column water vapor and ∇Q the total moisture divergence or convergence, i.e., the amount of moisture associated with the moisture advection and the mass divergence or convergence scaled by water vapor from the considered volume.*”

3) On line 157:

Suggest change

“*Version 2 (GSSTF2.0, Shie et al. (2009))*”

→

“*Version 2; 2c (GSSTF2.0, Chou et al. (2003); GSSTF2c, Shie et al. (2009))*”

and the following respective reference should thus be included:

Chou, S.-H., E. Nelkin, J. Ardizzone, R. Atlas, and C.-L. Shie, 2003: Surface Turbulent Heat and Momentum Fluxes over Global Oceans Based on the Goddard Satellite Retrievals, Version 2 (GSSTF2), *J. Climate*, 16, 3256-3273. [10.1175/1520-0442(2003)016<3256:STHAMF>2.0.CO;2]

4) On line 206:

Suggest revise

“see, e.g., Kidd and Huffman, 2011; Tapiador et al. , 2017)”

→

“see, e.g., Kidd and Huffman, 2011; Tapiador et al., 2017)”

5) On lines 207-208 :

Suggest revise

“which makes us of its own P data)”

→

“which makes use of its own P data)”

6) On line 210:

Suggest change

“The Global Precipitation Climatology Project - 1 Degree Daily (GPCP-1DD; denoted GPCP hereafter)”

→

“The Global Precipitation Climatology Project - 1 Degree Daily (denoted GPCP and GPCP-1DD, respectively, hereafter)”

7) On line 235:

Suggest revise

“consisting of 10 seperate model runs”

→

“consisting of 10 separate model runs”

8) On line 389:

Suggest revise

“we are also examine the separate contributions”

→

“we also examine the separate contributions”

9) On line 458:

Suggest revise

“(Oki and Kanae , 2006 ”

→

“(Oki and Kanae, 2006;”

10) On line 574:

Suggest revise

“(e.g, Allen and Ingram , 2002; Held and Soden , 2006; Trenberth et al., 2007).”

→

“(e.g., Allen and Ingram, 2002; Held and Soden, 2006; Trenberth et al., 2007).”

11) In Fig. 3B:

There are four kinds of shading shown with three precipitation data sets. The extra “dummy” shading should be removed.

12) On line 573-574:

“its water-holding capacity increases at a rate consistent with the Clausius-Clapeyron relationship (e.g, Allen and Ingram , 2002; Held and Soden , 2006; Trenberth et al., 2007)”

Here’s one paper (i.e., Shie et al., 2006) that also addressed a Clausius-Clapeyron scenario focusing on tropical oceans. It may be considered and included as one reference.

Shie, C.-L., W.-K. Tao, and J. Simpson, 2006: A note on the relationship between temperature and water vapor over tropical oceans, including sea surface temperature effects, *Special Issue of Advances in Atmospheric Sciences*, Vol. 3, No. 1, 141-148. doi: [10.1007/s00376-006-0014-5](https://doi.org/10.1007/s00376-006-0014-5)

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