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Dear Dr. Wanders,

Please find our answers to the reviewers below. We have answered all questions and made all possible changes suggested by the reviewers. The revised version of the manuscript has been uploaded using the Hydrology and Earth System Sciences (HESS) online submission system.

Best regards,

Dipanjan Dey

Reply to Mr. Dominik Schumacher *Thank you for the response on our article. We are grateful for all your constructive suggestions, which have helped us improving the manuscript. Below you can see our answers. The line numbers are from the revised manuscript*

Major Comments

Based on my current understanding of this manuscript and previous publications of the authors, the mass of each water 'parcel/particle' released when E-P>0 is given by the net evaporation amount, and hence simply E-P. Such a 6-hourly net evaporation event may range from a water amount of nearly zero to 1 mm or more, especially over subtropical oceans. Is it true that for every 'water release event' (E-P>0 at the surface), this water is then advected in accordance with the Eulerian water fluxes, behaving as a coherent 'parcel' with constant mass until 'reaching the surface', that is, precipitating? Moreover, I would like to understand if the advection of water is fully independent of the mass that it represents in this framework. I suspect that for the 'regular' version of TRACMASS (tracking air or ocean water), considering that "mass transport is linearly interpolated within the grid box" (Döös et al., 2017), this is not the case. I thus wonder whether the same analysis had a different outcome if, e.g., large net evaporation events were represented by several water parcels of uniform mass, rather than a single one. To compare, in Tuinenburg & Staal (2020), each mm of evaporation corresponds to 2000 parcels, and sensitivity experiments were performed for a range of 10 to 10000 parcels.

Answer : *Thank you for raising this point. To clarify your query we have now clearly stated on line no: 124 - 129 that "These water trajectories were started at the surface every 6 hours during 2016 where E > P, then advected by the 3-D mass transport of water and followed until they reached back to the surface where P > E. In total more than 89 million water trajectories were started with more* *than 7 million trajectories each month. The position of a given atmospheric water trajectory within a grid box is solved analytically in space and with a stepwise-stationary scheme (Döös et al., 2017) in time. The trajectories were integrated in time with six intermediate time steps between each 6-hourly output data from the ERA-Interim." Note that the "mass transport is linearly interpolated within the grid box" is a part of the analytical solution which determines only the trajectory position within the grid box and nothing to do with the water trajectory itself whose mass is constant throughout the journey. The effect of the number of water-mass parcels on the existing result lies outside the scope of the current study. However, we think that increasing the number of water parcels will not significantly change the outcomes presented here. This since the validation performed by Dey and Döös (2020) used the same Lagrangian resolution i.e. one water parcel per grid box every 6-hours during 2016, which resulted in almost identical E - P patterns as one will get from the ERA-Interim E - P.*

According to Dey & Döös (2020), annual mean E-P as diagnosed here generally agrees well with ERA-Interim data. But what about, e.g., E-P>0 for a single time step - which should be roughly equal to the total E, at least if the author's assumption on E and P not coexisting holds? I would expect severe underestimations for both E and P in tropical forests, where this assumption is rather poor, and believe that this limitation should be emphasized in the manuscript. Also, a related sentence to this (L. 211) may benefit from rephrasing, which is not entirely clear to me as is.

Answer : *Yes, indeed you would expect an underestimation of evaporation and precipitation estimates. However, the present study noted a stronger hydrologic cycle than the previous estimates. The reason for this could be explained by the way E - P has been computed in the current study which omits diffusive atmospheric water transports, specific rain and snow water content. Additional reason might be related to the use of 6-hourly cumulative net freshwater transport in the* *present study which prohibits the inclusion of processes occurring at a shorter timescale. These are all now mentioned clearly in the revised manuscript between line no: 231 - 242. The global E - P computed from the atmospheric water-mass conservation equation (or commonly known as the moisture budget) and from the individual evaporation and precipitation data at different time scale (starting from 6 hr to month) has been compared in Dey & Döös (2021). Please have a look into the Figure S2 and Text S2 in supplementary material of Dey & Döös (2021).*

Subgrid-scale turbulence, and in particular vertical mixing is not considered here. It is also assumed that water fluxes are 'constant' in each 6-hourly period - a single grid box can either have a net upward or downward water flux, but not both. Therefore, processes occurring at shorter timescales than the 6-hourly model analyses, such as convective precipitation, may not be captured adequately, and the precipitation diagnosed with the presented framework is not necessarily consistent with the 'underlying' reanalysis product, i.e. ERA-Interim.
I would therefore suggest rephrasing a statement in the introduction (L.51-55), which implies that this approach enables insights into the "true" precipitation. As far as I am concerned, this would require online rather than offline tracking as performed here, because only then are the mass (or air/water) fluxes fully consistent between the calculated trajectories and the 'driving' Eulerian model data. Clearly, online tracking is not an option when it comes to such reanalysis-based analyses and I think such offline approaches are still valuable, but the reader should, in my opinion, nevertheless be informed about this limitation.

Answer : *Thank you for your suggestion. We completely agree with your views and thus removed the word "truely" from the revised manuscript. The limitations of the method are now emphasized in line no: 78 - 84. Also, the impact of the limitations on the result and how it can be overcome is now stated in line no: 231 - 242 and line no: 287 - 293.*

To enhance the comparability to other studies, recycling ratios of, e.g., Amazonia, or the Mississippi or Congo basin would be of great interest (e.g., Trenberth, 1999; Tuinenburg et al., 2020). It could also be interesting to provide a global mean (or median; see Sodemann, 2020) residence time, which has been debated in recent years (Läderach & Sodemann, 2016; van der Ent & Tuinenburg, 2017; Sodemann, 2020).

Answer : *The global atmospheric water residence time maps and global average water residence time are now included in the supplementary material and also in line no: 202 -207. The objective of the present study is to get a global picture of the atmospheric water connection between the Ocean basins and global landmass. We did not divided the global landmass into various basins or continents and thus it is not possible to compute the recycling ratios for Amazonia, or the Mississippi or Congo basin from the present study. However, we have now mentioned in the the abstract (line no: 13 -15) and also in line no: 198 -200 that the land-to-land atmospheric water transport is prominent over the Amazon basin, western coast of South America, Congo basin etc.*

Minor Comments

When used to trace atmospheric air, a time-dependent analytical or stepwise-stationary scheme can be employed in TRACMASS (Döös et al., 2017) - does this also apply to the water-tracking version used here? Since no 'substeps' are mentioned in the manuscript, I assume that the analytical solution was employed, but perhaps this should be stated explicitly.

Answer : *Thank you for raising this point. We have now mentioned in line no: 126 -129 that "The position of a given atmospheric water trajectory within a grid box is solved analytically in space and with a stepwise-stationary scheme (Döös et al., 2017) in time. The trajectories were integrated in time with six intermediate time steps between each 6-hourly output data from the ERA-Interim".*

Cloud liquid & ice water: Is this treated differently with respect to Dey

& Döös (2020)? If so, where is this described? To me, the ability to include not only water vapor but also liquid and frozen water is an advantage of this approach, and deserves to be mentioned.

Answer : *In line no: 116 -117 it is now mentioned that "The inclusion of the specific cloud liquid and ice water content in the water transport calculation is an update as compared to the Dey and Döös (2020, 2021)."*

The global land recycling estimates are remarkably similar to the numbers presented by Tuinenburg et al. (2020), yet their approach is notably different despite also tracking water through the atmosphere. Perhaps this agreement could be mentioned; unfortunately, most other studies I am aware of only provide numbers at much smaller spatial scales, or for specific 'sink' and/or source regions and sometimes individual seasons (e.g., DomÃnguez et al., 2006; Dirmeyer & Brubaker, 2007; Keys et al., 2012; Keune & Miralles, 2019), and not the entire land mass.

Answer : *Thank you for your suggestion. We have now compared our global land recycling estimates with the previous studies and discussed it in line no: 220 - 230.*

I am not sure if the data employed (2016 & 2017) warrant the use of 'complete' in the title. After all, there appears to be considerable interannual variability when it comes to atmospheric moisture advection, even at large spatial scales such as for (tropical) Atlantic-to-Pacific moisture transports (Yang et al., 2021). I do not think that an extension of the analysis period is crucial for the outcome of the study, but a brief discussion could still be appropriate. Similarly, I was a bit surprised to see that ERA-Interim - and not ERA5 -data are used for this study.

Answer : *Thank you for raising this point. The title has now been changed to 'Atmospheric water transport connectivity within and between Ocean basins and land'. As mentioned on line no: 119 -120 "It is noteworthy that to satisfy the mass* *conservation property of the Lagrangian model TRACMASS it requires data at model levels and not at interpolated pressure levels". The requirement of data on model levels restricts our ability to use the ERA5 data. This is since the ERA5 data on model levels are vast in volume and slow to access due to higher spatial and temporal resolution than its precursor ERA-Interim. However, it has been found that our estimates are similar to the estimates provided by the earlier studies where they have used the ERA5 data (line no: 220 - 222). So we think changing the reanalysis product will not severely impact the outcomes of the study.*

L206: I struggle a bit with this sentence - the transports presented here should be lower than Eulerian estimates such as Trenberth et al. (2007) due to relying on net evaporation and precipitation events, is this what is meant? If so, stating clearly whether these estimates are actually lower (or only should be, but aren't) would be helpful.

Answer : *Yes, it was not written clearly. We have now modified the sentence in line 231 - 235 by stating "The strength of the hydrological cycle in the present study is stronger than previous estimates such as Chahine (1992); Trenberth et al. (2007). This despite one should expect the opposite since in the present study the atmospheric water is traced from the net evaporation ($E - P > 0$) to the net precipitation points ($E - P < 0$) and not from the total evaporation (E) to the total precipitation (P). The reason for this could be explained by the way $E - P$ has been computed in the current study which omits diffusive atmospheric water transports, specific rain and snow water content".*

Also, I am not convinced if the conceptualization of 'evaporation' and 'precipitation regions' employed throughout the manuscript is justified, since most regions are clearly both (and some even within 6 hours, as commented above).

Answer : *We have now changed the 'evaporation' and 'precipitation regions' to net evaporation and net precipitation regions or evaporation-dominated and* *precipitation-dominated regions wherever applicable.*

Further Comments

L. 18: "[...] coupled ocean-atmosphere system [...]"; I would strongly prefer the inclusion of land here, and since this would make the sentence harder to read, perhaps it is better to refer to the "climate" or "Earth system" as a whole?

Answer : *Changed it to "Earth System (line no: 20)".*

L. 69: "[...] this trajectory calculations [...]"

Answer : *Removed "this" and replaced with "these" (line no: 73).*

L. 184: "[...] waters are stay in [...]"

Answer : *Removed "are" (line no: 194).*

L. 207: "This since in the present study, [...]";

Answer : *The whole paragraph has now been modified (line no: 231 - 242). Thank you.*

Reply to Dr. Ruud van der Ent *Thank you for the response on our article. We are grateful for all your constructive suggestions, which have helped us improving the manuscript. Below you can see our answers. The line numbers are from the revised manuscript*

Major Comments

My first major comment is that the authors tend to overstate the novelty of their results and I found that a lot of relevant literature is not taken into consideration when putting their own results into context.

Answer : *We have now modified the sentences that might be overstating the novelty of our results and compared the results with previous literature wherever applicable (e.g., line no: 220 - 230). However, we still thinks most of the results presented here are novel to some extent. This since no previous studies have constructed an atmospheric water transport connectivity within and between ocean basins and land. We have also changed the title of the manuscript to "Atmospheric water transport connectivity within and between Ocean basins and land" in order to emphasize on the actual contribution of the present study.*

My second major comment refers to Figure 7, Table 1 and L207-214: "Note that this net evaporative and precipitating transports should underestimate the earlier Eulerian estimates (Trenberth et al., 2007). This since in the present study, atmospheric water is traced from the net evaporation (E - P >0) to the net precipitation points (E - P <0) and not from the total evaporation (E) to the total precipitation (P). The computation of the vertical mass transport of atmospheric water in the present study omits diffusive atmospheric water transport, specific rain and snow water content and thus leading towards an overestimate of the net evaporative and precipitating transports as compared to the total evaporation and precipitation estimates from previous studies, e.g. Trenberth et al. (2007). At any given time, the instantaneous net evaporation (E

- P > 0) and total evaporation might roughly be the same, if assuming that evaporation and precipitation cannot coexist at the same time but the present study uses 6-hourly cumulative net freshwater transport." If you do the conversion for example for land evaporation using the numbers from Table 1 (0.20+0.48+0.61+3.30 =4.59 $\times$ 10^9 kg s^{-1}) this equals 146 $\times$ 10^3 km^3 $year^{-1}$ if I haven't made any calculation mistake. Comparing this to generally accepted values of land evaporation of around 70 $\times$ 10^3 km^3 $year^{-1}$ (Rodell et al., 2015) or 81 $\times 10^3$ km^3 $year^{-1}$ for ERA-Interim evaporation fields directly (I used the values from van der Ent and Tuinenburg, 2017, Figure 1) one can easily see that the method in fact does not lead to underestimation, but rather a huge overestimation, which I would say cannot be assigned only to missing diffusive atmospheric transport, specific rain and snow water content. So this tells us that much bigger problems exist with the Lagrangian scheme presented here especially when applied to reanalysis data that normally does not close the water balance by design. One would expect such a striking problem of severely overestimating the intensity of the hydrological cycle to be investigated and discussed at great length in the context of the assumptions made by the applied method and a strong warning in the abstract, captions of all tables and figures and not just in the final sentences of the results (L207-2013).

Answer : *Thank you for the math and you are right that the net evaporative transports obtained in the present study are higher than the actual evaporation estimates. The reason for this could be explained by the way E - P has been computed in the current study which omits diffusive atmospheric water transports, specific rain and snow water content. Additional reason might be related to the use of 6-hourly cumulative net freshwater transport in the present study which prohibits the inclusion of processes occurring at a shorter timescale. These are now mentioned clearly in the revised manuscript between line no: 231 - 242, including a hypothetical situation explaining how the diffusive fluxes of water could modify* *the results. Note that this overestimation is nothing to do with the Lagrangian method presented here but associated with the way E - P has been computed in the present study. This since the net evaporative transports in the present study were calculated from the 6-hourly E - P dataset whenever E > P (E - P has been calculated using the water-mass conservation equation) and were only the starting points of the Lagrangian trajectories. This has been now repeatedly mentioned in the revised manuscript (e.g., line no: 131 -132, line no: 141 - 142, caption of Fig.2 , line no: 298 etc.). In the revised manuscript we have now stated clearly the limitations of the present study, its effects on the results and how we could overcome it in line no: 78 - 84, line no: 231 - 242 and line no: 287 - 293. The way we have stated the limitations of the present study in the revised manuscript should now be clear to the reader. However, we think it would be unreadable to put the limitations everywhere. Every study has its own limitations and it should be mentioned and discussed, which we have done in the present study now.*

Specific Comments

Lagrangian and Eulerian: I'd say the use of these terms is somewhat incorrect. It might be more intuitive to talk about the hydrological cycle with and without moisture tracking. See for example Figure 1 in Dominguez et al. (2020) for an overview of tracking scheme differences where Eulerian can also include moisture tracking and note that the online methods are also Eulerian tracking schemes.

Answer : *Thank you for raising this important point. We agree with your views and thus discarded the word "Eulerian" wherever it is not valid.*

Title: I think 'complete' is overstated, but one could say it is more complete than the view presented by, for example, Trenberth et al. (2011). Yet, one could also easily argue that other studies contained more aspects of the hydrologic cycle and as such make this study of Dey et al. less complete regarding those aspects. For example, Van der Ent et al.

(2014) and Tuinenburg and van der Ent (2019) showed atmospheric transit times and separated into evaporation from interception or transpiration, or yet others studied much more detailed, e.g., grid cell by grid cell (Link et al., 2020; Tuinenburg et al., 2020) or region by region (Singh et al., 2016) import and export matrices of atmospheric water. Yet other studies looked at the atmospheric water cycle in much greater temporal or spatial detail (too many references available to even start listing them). My suggestion is to be more specific in the title what the contribution of this study is.

Answer : *Thank you for your suggestion. The title has now been changed to 'Atmospheric water transport connectivity within and between Ocean basins and land'.*

L12: "recycling" I think this refers to recycling from land to land, but this is not obvious

Answer : *The word "recycling" has now been removed and replaced with "land-to-land" (line no: 13).*

L55: "In addition, knowledge about how much of the ocean/land evaporated water truly precipitates over the ocean/land itself and is transported to the land/ocean is not achievable. In the present study, these questions will be possible to address using a new Lagrangian framework." There are literally dozens of other moisture tracking methods with which it would be possible to address these questions or even have already addressed those questions. See for example Figure 1 in Dominguez et al. (2020) to start a more extensive literature study.

Answer : *We have now modified the sentences on line no: 56 - 60 as " In addition, knowledge about how much of the ocean/land evaporated water precipitates over the ocean/land itself and is transported to the land/ocean is not achievable. However, these question will be possible to address using Eulerian/Lagrangian* *atmospheric water tracing schemes (Van der Ent et al., 2010; Tuinenburg et al., 2020; Stohl and James, 2004; Stein et al., 2015; Dey and Döös, 2020). A list of atmospheric water tracing models and their advantages and disadvantages has been discussed briefly in Dominguez et al. (2020)".*

L69-70: "Note here that this trajectory calculations are based on atmospheric water-mass transport in kg/s and not transports of humid air." When the authors refer to transports of humid air I think they refer to the FLEXPART methodology (Stohl et al., 2015) or HYSPLIT (Stein et al., 2015) that track (E-P). However, there so many methods that track actual water mass (irrespective of the units) from evaporation to precipitation or backward. I again refer to Dominguez et al. (2020, Figure 1), but this is not even an exhaustive overview.

Answer : *The objective of this sentence is to state clearly what the present Lagrangian method is actually capable of doing and not to compare with other studies (this is also not the objective of the current study). The capabilities of the Eulerian/Lagrangian moisture tracing models are now mentioned in line no: 56 - 60.*

L95-96: "The vertically integrated zonal (Fx,i,j) and meridional (Fy,i,j) water flux was computed from the simulated water trajectories to describe atmospheric water transport pathways in longitude-latitude framework" Did I correctly interpret that the tracking scheme uses the vertically integrated fluxes only? It has been noted before that this may lead to significant errors, especially in some regions with a lot of wind shear such as West Africa (e.g., Goessling and Reick, 2013; van der Ent et al., 2013; Dominguez et al., 2020; Tuinenburg and Staal, 2020).

Answer : *No, we have used a 3-D atmospheric water transport field to compute the trajectories and the vertically integrated zonal and meridional fluxes of water were computed from those trajectories. This is now clearly stated on line no: 124*

- 129 and line no: 100 -102.

Figure 7 bottom: Note that a very similar figure was presented by Van der Ent et al. (2014, Figure 1) though only for the land, however, one can easily argue that the only unknowns in their figure are the oceanic arrows (evaporation, precipitation and oceanic recycling). However, oceanic evaporation and precipitation can easily be obtained from other data sources (e.g., Trenberth et al, 2011; Rodell et al., 2015) and oceanic recycling then follows from a simple water balance. Moreover, several others (up to Dey and co-authors to more thoroughly search the literature) have presented land recycling estimates and following the same logic using simple water balance and oceanic evaporation and precipitation estimates it would be quite simple to re-construct this figure with other numbers.

Answer : *Yes, we agree. The objective of inserting Fig.7 is to report a quantitative view (achieved from the present study) of the atmospheric water transport connectivity within and between the global ocean and land, which is not possible to obtain from the surface water budget estimates. We have therefore updated Fig.7 and its caption in order to avoid the impression that these can only be achieved with the Lagrangian method.*

L110: The atmospheric water transports were computed using the surface pressure, specific humidity, specific cloud liquid and ice water content and horizontal wind velocities from the ERA-Interim reanalysis (Dee et al., 2011). So, the method does not use evaporation and precipitation fields directly, yet infers them from the water balance. The advantage is that the water balance remains closed, but the disadvantage is that this could lead to unrealistic evaporation and precipitation estimates that compensate for atmospheric errors. This should be acknowledged, analyzed and discussed.

Answer : *In the revised manuscript we have now mentioned clearly the limitations of the present study, its effects on the results and how we could overcome it in line no: 78 - 84, line no: 231 - 242 and line no: 287 - 293.*

L118-119: These water trajectories were started at the surface every 6 hours during 2016 where E > P and followed until they reached back the surface where P > E. It should be noted that E and P occur concurrent using 6-hourly data, which is seemingly then ignored by the model setup. See `https://doi.org/10.5194/hess-2020-651-RC2` for a similar discussion. Also, it should be noted that convergence and divergence could be an issue when assigning E and P along a Lagrangian pathway. See `https://doi.org/10.5194/hess-2020-651-CC1` for a similar discussion. See Cloux et al. (2021) and associated public peer review for further details.

Answer : *Thank you for the links. We are aware of these limitations which has now been addressed on line no: 78 - 84 and line no: 231 - 242.*
We are not assigning E and P along the trajectories. We have clarified this on line no: 75 - 76 by stating "We are hence tracing the actual atmospheric water and not the moisture change along air-parcel trajectories". A brief description of the trajectory computation is also provided in line no: 124 - 129.

L138-140: "Note that the streamlines represent the integrated atmospheric water transport routes and is based on the sum of the Lagrangian trajectories, which should not be confused with the paths of the individual trajectories." This information would be more logical to put in the caption of the respective figures.

Answer : *Added in the Fig. 3 caption.*

Figure 6: I do not see any red contours as stated in the caption. Quite often white (0 days) is right next to blue (>24 days). It seems to me that white sometimes means that E-P < 0 and hence there is no data. Not

only should this then be given another color it illustrates the unrealistic consequences of assigning E only to regions where E-P > 0. In fact this is acknowledged in lines 207-214, but I then keep wondering what then the physical meaning and usefulness of these results are.

Answer : *We have realized that the Fig. 6 caption was confusing and is now rephrased as "The average residence time (days) of the atmospheric waters mapped on their net evaporative points within and between the three ocean basins and land. Note that this has been mapped where the net evaporation exceeds a monthly mean value of 0.2 mm day^{-1}. The residence time has been calculated from the time the trajectories have spent in the atmosphere between their starting (net evaporation) and ending (net precipitation) points."*
The choice of the colormap was clearly misleading and we have now opted for a different colormap. Now white regions only means where we have not calculated the residence time.

Figure 7: Units are missing

Answer : *Added.*

L240-254: Rather than discussing shortcomings of the studies by Stohl and James (2004, 2005) I think the authors should discuss the shortcomings of their own method with some priority. Moreover, the method by van der Ent et al. (2010) is neither Langrangian nor traces humidity changes, but looks at actual water transport.

Answer : *The whole paragraph has now been modified (line no: 270 - 293), which should serve the purpose. Additionally, we have now stated the limitations of the present study, its effects on the results and how we could overcome it in line no: 78 - 84, line no: 231 - 242 and line no: 287 - 293.*

Conclusions: absent (see `https://www.hydrology-and-earth-system-sciences.net/submission.html`)

Answer : *Added.*

Technical Corrections

Sv: Throughout the paper Sv is used to present 1×10^9 kg s^{-1}. However, 'Sv' in the SI system already stands for Sievert (which is something completely different). I never saw this notation before, but after some searching I did find that 'Sv'. (with period) is used in oceanic flow and known as the Sverdrups current. In hydrology this notation is, however, very uncommon and will thus be quite confusing to HESS readers and not only that it makes comparison to other studies which tend to most often present their results in km^3 year^{-1} quite cumbersome.

Answer : *We have now presented a conversion from kg s^{-1} to km^3 year^{-1} on line no: 51. The Sverdrups (Sv) unit has been used in many atmospheric studies before such as Craig et al. 2017, Craig et al. 2020, Sabin et al. 2020, Schmitt 2008 etc. Note that the present study uses units that are based on the mass transport (i.e. kg s^{-1}) and not on volume transport (i.e. m^3 s^{-1} or km^3 year^{-1}). This requires information of the water density, which is not necessarily exactly equal to 1000 kg m^{-3} .*

L13 "evapotranspirated": Evapotranspiration is already a somewhat redundant word (Miralles et al., 2020), but constructing a verb out of it always sounds even stranger. This is my personal opinion (for similar reasons as indicated in Miralles et al) and I do not want to impose it, but if the authors insist on keeping evapotranspiration they may at least consider changing the verb simply into evaporated.

Answer : *Thank you for the suggestion. We have now replaced the word "evapotranspirated" with "evaporated".*

Unit notation: Throughout the manuscript physical quantities (P and E) are often in roman font and units in italic (kg/s) which should be exactly opposite following commonly accepted notation: `https://www.`

hydrology-and-earth-system-sciences.net/submission.html

Answer : *Done. Thank you.*

References

Craig, P. M., Ferreira, D., & Methven, J. (2017). The contrast between Atlantic and Pacific surface water fluxes. Tellus A: Dynamic Meteorology and Oceanography, 69(1), 1330454.

Craig, P. M., Ferreira, D., & Methven, J. (2020). Monsoon-induced zonal asymmetries in moisture transport cause anomalous Pacific precipitation minus evaporation. Geophysical Research Letters, 47(18), e2020GL088659.

Sabin, T. P., & Pauluis, O. M. (2020). The South Asian monsoon circulation in moist isentropic coordinates. Journal of Climate, 33(12), 5253-5270.

Schmitt, R. W.: Salinity and the global water cycle, Oceanography, 21, 12-19, 2008.

Reply to Mr. Andreas Link *Thank you for the response on our article. We are grateful for all your constructive suggestions, which have helped us improving the manuscript. Below you can see our answers. The line numbers are from the revised manuscript*

Comments

The authors wrote that earlier studies focused more on the regional or basin-scale water budget analysis and perhaps miss two studies within this field, which were conducted on a global scale: One of these studies refers to a publication at which I worked with other researcher on the global fate of land evaporation ("The fate of land evaporation - A global dataset"): ESSD - The fate of land evaporation - a global dataset (copernicus.org). The other one, in turn, refers to the following publication: "High-resolution global atmospheric moisture connections from evaporation to precipitation" ESSD - High-resolution global atmospheric moisture connections from evaporation to precipitation (copernicus.org) While other global studies are available, one point of improvement could be to put the determined results into the context of those. Some of the determined patterns/ key numbers could, for instance, directly be compared and discussed to those studies. The work of Tuinenburg et al., for instance, determined that 70% of global land evaporation rains down over land, which is the range of the author's work. Our work, however, determined a recycling ratio over land of appr. 59%. Perhaps, a comparison of some key numbers would generally be interesting.

Answer : *Thank you for your input. We have now modified the sentences and included the suggested references in line no: 254- 257. The global land recycling estimates achieved in the present study has now been compared with the previous studies and discussed in line no: 220 - 230.*

Figure 6 of the work provides the average residence time in days for

water travelling from specific types of source to receptor regions. Is it perhaps possible to put them into context of resident times which have been determined in previous studies (e.g. overall residence time in atmosphere independent from its source: 8 days as estimated by Shiklomanov and Rodda; Shiklomanov, I. A.; Rodda, J. C. World Water Resources at the Beginning of the Twenty-First Century. International Hydrology Series; Cambridge University of Press, 2004.).

Answer : *Thank you for your suggestion. The global atmospheric water residence time maps and global average water residence time are now included in the supplementary material and also in line no: 202 -207.*