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.*