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.