Review of Dey et al

General comments

Dey and coauthors investigate the atmospheric hydrologic cycle, and more specifically the import and export of atmospheric water between 3 major oceans and the land surface. As such, they provide a 'complete' view of the atmospheric hydrologic cycle. The results are generally well presented and the paper is relative easy to read.

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.

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 × 10^9 kg s^{-1}) this equals 146 × 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 × 10^3 km^3 $year^{-1}$ (Rodell et al., 2015) or 81 × 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).

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.

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.

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

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.

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.

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).

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.

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.

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.

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.

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.

Figure 7: Units are missing

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.

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

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.

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.

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#math

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