Review of Dey et al., 2021

The authors employ Lagrangian tracking of (6-hourly net) evaporated water in accordance with Eulerian water fluxes, integrated from the top of the atmosphere down to the surface. By doing so and then reducing the dimensionality of water transports across the globe with Lagrangian streamfunctions, water fluxes between land and the major ocean basins are revealed and quantified. As emphasized by the authors, this approach marks a notable deviation from many previous studies, owing to the fact that water – and not air – is tracked, and that mass is conserved along trajectories. They provide estimates of the water fluxes on a global scale, and also present atmospheric water residence times (or, technically, the time between net evaporation and net precipitation) – as such, I consider this to be a valuable contribution to the field. I would like to encourage the authors, however, to not only explain their approach and assumptions in more detail, but to also provide additional quantifications, which would facilitate a comparison to existing work (examples provided below). I would like to commend the authors for this carefully designed study, and provide additional feedback in the following.

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 10'000 parcels.
- 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.

- 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.
- 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).

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.
- 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.
- 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., Domínguez et al., 2006; Dirmeyer & Brubaker, 2007; Keys et al., 2012; Keune & Miralles, 2019), and not the entire land mass.
- 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.

- 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.
- 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).

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?
- L. 69: "[...] **this** trajectory calculations [...]"
- L. 184: "[...] waters **are** stay in [...]"
- L. 207: "This since in the present study, [...]";

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