Review of Tuinenburg & Staal (2019)

Tracking the global flows of atmospheric moisture

In this study, a Lagrangian moisture tracking model driven by ERA5 reanalysis data is presented, and recommendations concerning both input data resolution and model set-up in light of accuracy and efficiency are given based on sensitivity experiments. This method is designed for tracking the fate, or as the authors phrase it, the 'footprint' of evaporative moisture released at point locations around the globe. Such a sensitivity analysis comes at the right time, as ERA5 drastically improved both the temporal and spatial resolution as compared to the widely used ERA-Interim in moisture tracking studies, yet this wealth of data is also accompanied with data storage limitations, and considerable simulation time increases. This well-written, nicely presented study may therefore boost scientific progress by providing guidelines on how to use ERA5 data most efficiently, and additionally, demonstrates the power of Lagrangian modelling for tracing moisture. Although generally easy to understand, some descriptions lack clarity, and I believe the manuscript could be ameliorated if the authors considered the following points listed below (not sorted), and thus become suitable for publication in HESS.

- **Number of particles:** If I understand it correctly, then even the setting with the lowest number, 100 particles per mm of evaporation, is still high compared to some previous studies; in case of e.g. Manaus, for a range of say 3 to 6 mm of daily evaporation, this would correspond to 300 to 600 particles being released per day.
  Studies such as Gimeno et al. (2012) and many, many others follow the method introduced in Stohl & James (2004) and are based on air parcels (that, altogether, represent the entire atmosphere), and not a 'collection of water particles', as is the case here, but I believe the number of particles may still be compared. Even though some studies based on the Stohl & James (2004) approach do not even mention the number of parcels (or 'particles'), a lot of publications are based on simulations with 6-hourly timesteps for which 2 million air parcels for the entire globe are used (e.g. Sorí et al., 2017; García-Herrera et al., 2019, …). Läderach & Sodemann (2016) used 5 million parcels globally, which corresponds to an average of about 70 parcels per column residing over each grid cell at any given timestep, so that about 280 parcels are available per grid cell and day, compared to about 120 in many other papers (2 million parcels globally). Therefore, the setting used in this study with 'only' 100 particles per mm of evaporation, at least in the case of high evaporation rates as in Manaus, still results in far more particles than were employed in many peer-reviewed moisture tracking studies. Indeed, as the authors point out in the discussion, larger study areas and longer analysis periods likely decrease the number of particles needed. Still, it would be interesting to see an extended Fig. 4 for e.g. 50, 25 or even 10 particles per mm of evaporation, and this might also reveal a lower limit, since even 'only' 100

particles perform nearly as well as 10'000. Considering the near-linear runtime dependency on the number of particles, this information might be valuable.

- **Particle vs parcel:** Related to the comment above, I am not sure if the usage of 'particle' throughout most of the manuscript is ideal. Particle by definition implies a small size, but depending on the number of particles (per unit of evaporation) used, each particle represents a considerable amount of water, and obviously not a tiny droplet or even a single water molecule, as one might first think. As of now, both 'particle' and 'parcel' are used in the manuscript, so I suggest to remove one of these to ensure consistency, preferably the former.
- **Omega:** It is unclear to me why the authors decided to omit omega in their default configuration. In this context, it is important to point out that the statement on l. 423 is incorrect: the approach of Stohl et al. (2005) does use omega, yet it is complimented by random vertical displacements of air parcels to represent convective (vertical) redistribution. Also, the argument starting on l. 436, "We assume that this speed of mixing is rapid enough to supersede larger-scale vertical flows so as to simplify the model and exclude omega", seems to contradict a previous statement, in which the large effect of omega (vs no omega) is highlighted; moreover, evaporation footprints with and without omega are only similar for (probably unrealistically) fast mixing times of 1 hour, which strongly indicates that this aforementioned 'superseding' does not really take place for mixing at timescales of 24 hours. The lack of a proper reference, since the baseline model is essentially identical to the 3D Lagrangian model (a major limitation that is not really emphasized in the manuscript), clearly makes it difficult to justify any choice here; however, I am not convinced by the explanations provided so far, other than omitting omega 'for simplicity' and to achieve faster simulation times.
- **Horizontal resolution:** Especially because of the authors' valuable recommendation not to degrade the vertical resolution, it would be helpful for the scientific community to know whether the same is true for degrading the horizontal resolution. As stated in the manuscript, many previous Lagrangian studies are based on input data at 0.75x0.75°, or even 1.0x1.0° horizontal resolution, so that upgrading to 0.25x0.25° represents a massive improvement. However, this comes at the cost of extensive input data size, which makes studies covering multiple decades rather than a few days extremely challenging. I suggest adding this analogously to the degradation of vertical information; e.g. for increments of 0.25° from 0.25x0.25° up to 1.0x1.0°. For the same reasons, investigating the sensitivity to temporal resolution would be helpful too, but I am aware that the authors cannot include everything in their manuscript.
- **Tracking time:** 99% of moisture allocated, or 30 days: Dirmeyer & Brubaker (2007) used 90% & 15 days, and nearly all tracking studies do not exceed 15 (or even 10!) days either, as the trajectory accuracy is known to decrease with increasing length (Stohl & Seibert, 1998). Due to how the model is set up, at least if I understand it correctly (see also below), this choice might not really affect the results, but I still suggest to check if the conclusions hold for considerably shorter trajectory lengths, such as 15 or 10 days.

- **Model description:** is insufficient. According to my understanding, tracked moisture remains static until precipitation occurs ('over' the grid cell underneath), and then decreases accordingly to the ratio of precipitation over precipitable water (of the entire column, I presume?). If so, this invokes another assumption, namely that each water (vapor) molecule within a tropospheric column has the same odds of condensing and precipitating (Dirmeyer & Brubaker, 2007). Please clarify.

Minor comments and suggestions

In addition to the major comments above, a few additional comments and suggestions are listed here.

- l. 25: This sentence describes *continental* moisture recycling, not the more general concept of moisture recycling as it is most commonly defined (e.g. Brubaker et al., 1993; Dirmeyer et al., 2009). I suggest to include 'Continental' at the beginning of this sentence, and replace 'continental evaporation' e.g. by 'terrestrial evaporation'. This would also be consistent with the results section, where the continental recycling ratio (CRR) is used already.
- l. 113: Not all Lagrangian models are initialized with a collection of water particles, but indeed, this is true for the approach presented in the manuscript. I suggest rephrasing this.
- l. 166: Is this the only instance where the (very short) study period is referred to? I think it would be justified to add a sentence in the discussion to remind the reader of this limitation.
- l. 171: Is there any reason behind the choice of both Utrecht and Stockholm, which are rather 'close' both geographically and climatologically compared to all other point sources, other than the authors' affiliations? This is merely a question/comment, not a suggestion, since the conclusions drawn by the authors do not depend on this choice.
- l. 265: Is the usage of time units (#particle mm^{-1}**h^{-1}**) correct here? Besides, elsewhere in the text, the number of particles simply relates to some evaporation amount/volume (without referring to time at all), which is already a sufficient description to me.
- l. 306: Do the authors have an explanation as to why the simulation time increases with fewer vertical levels? I do not doubt that this is correct, I simply find it counter-intuitive.
- l. 423 contains an incorrect statement, as Stohl et al., 2005 do not disregard omega in their approach (also mentioned above)
- Fig. 9: Why does B resemble Fig. 3D more than C? According to the text (l. 204), the default choice for the 3D Lagrangian model is 2'000 particles per mm of evaporation.

Further comments

Below are a few additional, language-related comments.

- l. 36: "Universal approach**es** and principle**s** moisture tracking models **is** that they apply…", this reads a bit weird to me.
- l. 105: "courser" => coarser?
- l. 127: "weighed" => weighted?

- l. 185: “on full resolution” => at full resolution?
- l. 345: “CRR decreased much more rapidly ***with (increasing)*** mixing time than ...”, maybe a word (or two) went missing here?
- l. 442: “***The*** Continental recycling ratio”, not sure if this is correct without any article

Concluding remarks

The paper investigates a multitude of choices and assumptions related to setting up a (Lagrangian) moisture tracking framework based on ERA5 data, and even though the main figures are only for a single location (Manaus) among a total of seven, and the analysis time is very short, I believe this choice is justified. Except for the discussion on vertical mixing and omega, as well as the interpretation on the sensitivity to the number of particles, the conclusions stated in the study make sense to me. Additional experiments (e.g. horizontal & temporal information degradation) and some more explanations might complete the package, hence I would gladly serve as a reviewer again and look forward to reading the revised paper.

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