Review II of Tuinenburg & Staal (2019)

Tracking the global flows of atmospheric moisture

I am glad that the authors have extended their analysis and parts of the manuscript, and believe this has improved the quality of the latter. I thus thank the authors for taking my comments into account. In general, I agree with the responses to my review, and believe the manuscript is now suitable for publication – and would be even more so if my (minor) comment below were considered. I have also made a few additional comments and suggestions on the updated manuscript, listed under "Further comments".

Minor comment

"- 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."

> "We appreciate the suggestion, but we would advise the model users against using short tracking times. It happens often that after ten days not all tracked moisture has been allocated yet, in which case continuing the tracking will be better than terminating it. This happens especially over drier areas. Although we agree that the uncertainty increases with time, the quality of the forcing data is not reduced with longer tracking, as they are observation-based and not model-based."

> With the approach used and described here, evaporation, represented by water vapor parcels, is tracked until less than 1% of the original amount remains, or 30 days are exceeded. Since each time a precipitation event occurs, the water content of parcels is depleted in accordance with the entire column (as now also described more clearly in the Methods), this means that the amount of allocated moisture increases logarithmically – the first few precipitation events (with respect to the beginning of tracking) naturally "remove" larger amounts of the originally evaporated and tracked moisture than after, e.g., 15 days, when a lot of tracked moisture has usually already rained out. Consequently, while extending trajectories from 5 to 10 days has a strong impact on the moisture sink (or source, depending on the approach) region, differences between 15 and 20 days are already marginal (Sodemann & Stohl, 2009).
> Whereas I would still exert caution in the interpretation of moisture sink regions that were obtained with trajectory lengths up to one month, it is indeed, as the authors point out, the only way to attribute nearly all of the tracked moisture. Personally, I would

include an, e.g., 95% option, possibly in combination with a maximum allowed trajectory length of e.g. 15 days, so that the user can easily choose between
a.) 99%, default: maximizing amount of ‘explained’ moisture
b.) 95%: maximizing efficiency* & accuracy (of trajectories and hence simulated ‘fate of evaporation’, or downwind precipitation)

* I suspect if tracking was always halted at 95%, trajectories would be frequently cut off far ‘sooner’ than after 30 days, and thus cut down runtime.

However, because the vast majority of moisture reductions (in terms of amount) tends to occur within the first few days (Sodemann, 2020), and even though the authors argued against lowering their maximum allowed trajectory length of 30 days without exploring further, I believe their choice does not affect any of the conclusions drawn in the study. Moreover, unfortunately, while it is clear that trajectory uncertainty does increase with tracking time, assessing this in detail would probably require a publication of its own. Thus, while I believe that the trajectory accuracy constraints I mentioned remain valid, as even the (high-)quality forcing data cannot prevent the accumulation of trajectory errors, I would like to encourage the authors to at least (briefly) inform the reader about this uncertainty, but do not see the need to insist on any additional changes.

Further comments

- p. 2, l. 54:
  “If the time step is chosen too large, real moisture transport may occur faster than the simulation grid and time step allow for (i.e., if the Courant number $C = v\Delta t/\Delta x > 1$). If the time step is taken too small, numerical diffusion will occur, meaning that moisture transport in the model will be faster than in the forcing data.”

  I am no expert in numerical modelling, but I believe that this phrasing is a bit misleading. As far as I know, numerical diffusion is primarily a consequence of the use of numerical schemes to represent partial differential equations as finite differences, which enables a numerical rather than analytical (and thus exact) solution. While particularly higher-order schemes provide very good approximations, even those are still associated with tiny errors, which, together with discretization errors and machine precision limitations, cause ‘numerical diffusion’. I thus suggest to slightly rephrase the second quoted sentence to reflect that numerical diffusion becomes problematic for small timesteps (rather than implying the former is solely caused by the latter), because of course, if there are inaccuracies in the calculation of e.g. wind speeds due to the use of some advection scheme, this becomes exacerbated if these calculations are repeated more often (due to smaller timesteps).

- p. 4, l. 94:
  "In general, atmospheric moisture tracking is achieved by [...] keeping track of how much of that moisture rains out **where**."

  To me, it reads better if the 'where' is moved to '... keeping track **where** and how much of that moisture rains out'.

- p. 13, l. 411:
  "Also here, the figures show that slower vertical mixing increases the area where rainfall depends on evaporation from the studied sources. However, with omega, the rainfall from the sources is more equally distributed within the footprints than without omega. In other words, with omega the footprints show a pattern that is less influenced by diffusion (Figs. 10, S44−S49)."

  The first and second sentences make perfect sense to me, but not the third one. I suppose it is true that there are stronger contrasts in the top row (no omega) of e.g. Fig. 10 as compared to the bottom row (omega), or phrased differently, rainfall is more equally distributed with (than without) omega. But then, considering how the footprints with omega are more smooth, and diffusion (by definition) diminishes gradients, how are the omega-footprints less influenced by diffusion?

- p. 14, l. 444:
  "Also**,** the distances of moisture transport differed by several degrees for each of these models."

  I believe a comma after 'Also' would help here.

- P. 15, l. 467:
  " … , surface release will be the default in our model."

  Perhaps "... , surface release **is** the default in our model" instead? I believe this would also be consistent with sentences further below, e.g. "As default we take a mixing time of 24 h without omega."

References

Sodemann, H. & Stohl, A. Asymmetries in the moisture origin of Antarctic precipitation. *Geophys. Res. Lett.* **36**, 1–5 (2009).

Sodemann, H. Beyond Turnover Time : Constraining the Lifetime Distribution of Water Vapor from Simple and Complex Approaches. *J. Atmos. Sci.* **77,** 413–433 (2020).