Review of Li et al, EGUsphere, 2024

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General comments

Li and co-authors study the moisture sources of precipitation in 2 river basins for the (seemingly randomly chosen) 2022 July period with 2 moisture tracking algorithms WAM2layers and FLEXPART-WaterSip. They compare the methods and subsequently test sensitivities when changing certain parameters. The study is timely, relevant, generally easy-to-follow and substantiated with good figures and tables. However, I have two major comments:

1. The study is not at all reproducible as no detailed model settings are provided in relevant scripts. Moreover, people that use other moisture tracking models or settings would not be able to compare their results against that of the authors as no output data is provided. Only generic links to input scripts and data are available which are by far insufficient in this new era of FAIR and Open Science.
2. The authors make several strong statements and conclusions about the tracking models ability, which, in my opinion are mere hypotheses by lack of knowledge about an actual truth. These hypotheses should be substantiated by additional analysis and/or toned down.

Specific comments

L. 23: “the Eulerian or Lagrangian method”

There is no such thing as ‘the XXX’ method and there are many other factors (possibly more dominant factors) that contribute to differences in moisture source attribution.

L. 29-31: “The inherent ability in WAM-2layers to distinguish between evaporation and precipitation makes it more effectively in identifying varying moisture contributions arising from distinct surface evaporation sources.”

Effectively by what measure?

L. 31-33: “In contrast, in regions heavily influenced by smaller-scale convective systems with high spatial heterogeneity, such as the UTB when compared to the YB, simulations from FLEXPART-WaterSip tend to be more reliable.”

Reliable by what measure?

L. 34: “However, FLEXPART-WaterSip is prone to introducing additional errors when using specific humidity information in particles to infer moisture uptake and loss, although it accurately depicts the three-dimensional movement of air particles.”

Accurate by what measure?

L. 44-49: “In comparison, the Lagrangian method employs a particle trajectory tracking approach, inferring the movement of moisture through individual three-dimensional particle trajectories solved with differential equations. While Lagrangian models typically involves more complete physical mechanisms in particle dispersion processes, they exhibit substantially less numerical diffusion than Eulerian models, making them more adept at capturing small-scale atmospheric phenomena such as turbulence, convection, and dispersion, particularly over complex terrains (Wang et al., 2018; Tuinenburg and Staal, 2020).”

But do most or all Lagrangian models include actual diffusion through turbulence, velocity differences, rainfall re-evaporation etc.? If not, then having no diffusion either numerically or explicitly modeled would also lead to errors.

L. 53-55: “However, these studies have not extensively explored the limitations of different model types and the causes of discrepancies between moisture tracking results. Moreover, the studies on the generation mechanisms of model uncertainties through the moisture tracking intercomparison is severely lacking.”

I think the authors’ study is a good addition, but I do not think that objectively they do much more than these previous studies. So, they should tone down this comment and somewhere in the introduction explain the relevance of their own contribution. A missing moisture tracking model comparison study is also the one by Van der Ent et al. (2013).

L. 64-65: “the Eulerian… the Lagrangian”

Same comment as above.

Table 1: “Overview …”

- Please note that this overview table is non-exhaustive
- Particularly missing studies are those by Guo et al. (2019, 2020)
- Is CAM a tracking model?
- I’d say the the moisture source diagnosis of WAM2layers is simply the E and P from the data (as in QIBT or UTrack)

L. 92-93: “The model prescribes a two-layer division (~810 hPa with a standard surface pressure)”

Probably good to stress that the layer separation is very different over the Tibetan Plateau.

Figure 1: “method”

- In WAM2layers P also goes out the upper layer
- WaterSip is not necessarily 6 hours I suppose?

L. 145-146: “Our numerical experiments, as illustrated in Fig. S2b, indicate that within the first 10 days (20 days), we traced 89% (99%) of the precipitation moisture in the YB and 97% (99%) in the UTB.”

The amount of attributed moisture seems very high to me. Do the authors think this realistic? How does the E simulated from WaterSip compare to actual E from ERA5?

L. 156-159: “Another noteworthy detail is the clear north-eastward extension of moisture sources for UTB precipitation resolved by FLEXPART-WaterSip, reaching almost to the easternmost Tianshan Mountains (Fig. 2d), a feature absent in the results of WAM-2layers (Fig. 2b).”

It is not clear exactly where the Tianshan Mountains are in Figure 2. Moreover, the word ‘resolved’ suggests that there is orthogonal evidence for those moistures to be the ‘truth’, but I fail to see where that is presented.

Figure 2: “Spatial distributions …”

- FLEXPART-WaterSip attributes vast areas of evaporative sources from as far away as the Arabian Desert and the Sahara in the same order of magnitude as evaporative contributions from the Red Sea, Gulf of Aden and Gulf of Oman. With actual evaporation being several orders of magnitudes lower in the desert, this feature is completely unrealistic and warrants more investigation by the authors. What does this tell in general about the trustworthiness of this method?
- The blank area between MWE and AS seems not a very logical way to separate regions.

Figure 5 and 6:

What is the exact meaning in a quantitative sense of the red arrows?

L. 242-244: “This further implies that the modelling capability of WAM-2layers for moisture sources of the UTB may be less robust than for the YB, consistent with the observation that the simulation disparities between the two models are more pronounced in the UTB than that in the YB (Fig. 4).”

As mentioned before, this hypothesis is not substantiated by any quantitative analysis. Alternatively, my hypothesis would be that while moisture goes to the northeast (back in time), there was very little evaporation in that area from the ERA5 data, so it wasn’t identified as a source, whereas FLEXPART-WaterSip erroneously assigns an imbalance in its Lagrangian moisture budget as surface evaporation which may also have be caused by, for example, convergence. I do not have any evidence directly for my hypothesis either, but it is up to the authors to investigate the matter in more detail before jumping to conclusions.

L. 268-270: “A notable difference between WAM-2layers and FLEXPART-WaterSip, as highlighted in Fig. 2, is that FLEXPART-WaterSip model fails to capture most moisture source regions across the entire northwestern Eurasia for both basins when compared to WAM-2layers.”

The word fails suggests that we know that WAM2layers would be more correct, but we don’t know, do we?

L. 281-287: “Experiment 1 …”

This is a nice sensitivity test, however, its results can only be interpreted in case we also know how the timestep was adjusted, which together with spatial resolution drives the numerical diffusion and hence the average travel distance.

L. 297-304: “Experiment 3 …”

More details on the areal source-receptor attribution method are needed here as well.

Figure 8: “Relative moisture contributions …”

- What is the remaining percentage from other regions?
- What is the remaining percentage from outside the domain?
- What is the remaining percentage unaccounted for altogether?
- The labelling should be more precise for WAM2layers in terms of resolution for both exp 1 and the original run.

L. 328: “original WAM-2layers”

I think both experiments are WAM-2layers with different settings, so the word ‘original’ is perhaps a bit misleading.

Fig. 10. “Time series …”

- Please improve the caption to make sure all details are explained.
- Is precipitation and evaporation the ERA5 data, or the inferred data from the WaterSip algorithm.
- If the latter, how does it compare to actual ERA5 data?

L. 382-385: “Its effectiveness in regions with complex weather conditions is generally inferior to that of FLEXPART-WaterSip when operating with forcing datasets of the same resolution.”

By lack of a clear benchmark ‘truth’, observational or orthogonal evidence, these conclusions are not substantiated. The authors should refrain from using words like ‘inferior’ and/or provide additional analysis to substantiate or revise such conclusions.

L. 402-405: “Nevertheless, compared to WAM-2layers, FLEXPART-WaterSip offers a precise depiction of the three-dimensional distribution of moisture sources, especially in capturing smaller-scale convective systems with high spatial heterogeneity.”

In the lines before the authors discuss the shortcomings of WaterSip, but then they go on to conclude that FLEXPART-WaterSip offers a precise depiction … This reasoning does not seem logical to me.

L. 415-420: “Code availability … data availability …“

This is insufficient. The authors should revisit the policy of sharing data https://www.atmospheric-chemistry-and-physics.net/policies/data_policy.html and make the actual code and data they used during their research publicly available to the community. If software is used, they should refer to exact versions with doi’s and the scripts the authors used themselves to run the models, so not to generic websites that are subject to change. All data underlying the figures should also be deposited meaning numeric values for moisture sources, masks for the tagging region etc.

Technical corrections

Equation (1):

The equation as used by Findell et al. (2019) is more correct than the one in Van der Ent et al., (2014)

Figure 3: “Absolute differences ...”

The green outline with red underlying data is not color-blind friendly.

L. 380: “WAM-2layers model”

The WAM2layers model

References

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