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Interactive Comment

Interactive comment on "Should we use a simple or complex model for moisture recycling and atmospheric moisture tracking?" *by* R. J. van der Ent et al.

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1 General comments

The authors compare the performance of three moisture tracing methods of which the most complex one, 3-D online tracing, serves as the "virtual reality" to which two a posteriori offline methods with reduced complexity are compared. Focussing their investigation on the West African Lake Volta region, the authors find that the results of the moisture tracing are highly sensitive to the treatment of the vertical dimension because of the strong vertical shear that characterises the region at the considered time of the year (August).

The scientific content of the paper is generally of good quality and well suited for publication in HESS. There are some issues that require further improvement including the following:

- Inaccuracies in the presented equations.
- An insufficient description of the three tracing methods, in particular regarding the way they involve different flavours of the “well-mixed” assumption. This problem may be partly due to the fact that the method description is somewhat scattered over the manuscript.
- A problem with the flux shear factors.
- A number of misleading formulations.

2 Specific comments

P6727, Eq1:
(i) Please consider denoting the units of the variables in the text, in particular for S_g (presumably kg m^{-3} ?).
(ii) Do I understand correctly that E_g and P_g denote terms that are due to surface evaporation and surface precipitation? If so, a comment on why E_g and P_g are not zero away from the surface would help (see also the comment on Eq. (2) below).

P6728, Eq2:
(i) I think that this equation is not correct, whatever kind of assumption may be involved. As an example, one can easily falsify the second equality by considering a situation where u and v are spatially constant (non-zero), S_g does not vary in x-direction while

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S does (which means that the remaining part of S varies in x-direction), and where both S and S_g have the same non-zero gradient in y-direction (which means that the remaining part of S does not vary in y-direction). This would mean that the second term in Eq. (2) is zero and the third term is one. The point that, when removed by precipitation or transported by the winds, tagged moisture is affected proportional to its relative abundance would be expressed by the same equation but without the spatial and temporal derivatives:

$$\frac{P_g}{P} = \frac{S_g u}{S u} = \frac{S_g v}{S v} = \frac{S_g w}{S w} = \frac{S_g}{S} \,. \tag{1}$$

This also shows that, when formulated continuously in time and space (including the vertical), there is in fact no approximation involved regarding the transport terms (obviously, the velocities cancel).

(ii) It is not clear what P_g and P exactly are (while this is more obvious for S_g and S). It appears to make most sense (also in Eq. (1)) if P_g and P denote the local rate of condensation from the gas phase to the liquid/solid phase which is then removed by the precipitation process (compare Eq. (A6) in Goessling and Reick (2013)). Or are P_g and P the respective amounts of precipitation falling across the considered height level (compare Eqs. (A3) and (A5) in Goessling and Reick (2013))? This should be clarified.

P6728, L4: *this assumption becomes weaker with less model layers*

The term "weak assumption" commonly means that an assumption is associated with *small* errors, while a "strong assumption" involves *large* errors. Is this the intended meaning? Would it make sense to replace "weaker" by "less accurate" to avoid confusion?

P6728, L12: $\frac{\partial S_g}{\partial t} / \frac{\partial S}{\partial t} = \frac{\alpha_g}{\alpha}$

If I am not mistaken, one would have to indicate that only the part of the tendencies that is due to the residual term is considered here, e.g.

$\left[\frac{\partial S_g}{\partial t}\right]_{\text{residual}} / \left[\frac{\partial S}{\partial t}\right]_{\text{residual}} = \frac{\alpha_g}{\alpha}$.

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P6728, L22-25:
I think one could make more clear what is meant with the different flavours of the "well-mixed" assumption in points (2) and (3) by adding corresponding equations (for point (3) compare Eq. (5) and the subsequent paragraph in Goessling and Reick, 2013). Elaborating on this here would be important because the different flavours of the "well-mixed" assumption are very central to understand the differences between the tracing methods.

P6729, L25: *but additionally takes into account vertical wind speed*
In fact, QIBT implicitly does so as well (at least the large-scale component) by using quasi-isentropic coordinates.

P6729, L25-26: *3D-T tracks water parcels in a Lagrangian manner*
It becomes clear only later (in Sect. 3.2.3) that 3D-T has been used in forward mode rather than backward mode as QIBT.

P6727-6730, Sect. 2:
I think that this section (and hence the paper) could benefit from (i) more structure and (ii) an enhanced description of the models used in this study. One could subdivide the section into a general part (Sect. 2.1) and one part (Sect. 2.2) with the description of the specific models used here including their modified variants (Sect. 2.2 could be further subdivided for the different models). Quite some model description currently given in the results section (in particular in Sects. 3.3 and 3.4) could be transferred here. Adding central equations that highlight the differences between the methods (in particular those differences that concern the different flavours of the "well-mixed" assumption) would also increase clarity.

P6732, L14-16: *we can say that evaporation from Lake Volta is significantly impacting the regional system as over one-third of the evaporation recycles within the domain and about 2 % recycles over Lake Volta*
I doubt that the conclusion "evaporation from Lake Volta is significantly impacting the

regional system" can be drawn from the second half of the sentence. The latter could be true also for the evaporation from a single $(1\,\mathrm{cm})^2$ patch of Lake Volta, yet it would not matter at all if this small patch was dry instead. The previous sentence (*the tagged rain accounts only for a few percent*) is much more meaningful when it comes to the sensitivity of the region to evaporation from Lake Volta. Even more conclusive would be a sensitivity experiment with a (regional) atmospheric GCM (like MM5) where Lake Volta is simply removed (in the spirit of Goessling and Reick, 2011) – I would in fact be keen to see the outcome of such an experiment.

P6734, L9-10: *At an input time step of 1 h, this is an acceptable assumption.*
I think that the high spatial resolution is as important as the high temporal resolution for an explicit representation of the mixing between the two layers.

P6736, L3: *it was known*
Maybe "known" is too strong here and "anticipated" would be more appropriate.

P6737-6738, L26-2: *[The "well-mixed" assumption for precipitation implies that] the amount of moisture that precipitates out of the parcel during a time step is proportional to the total column moisture.*
I think that this is not correct, please rectify this statement. Again, it would help much if the "well-mixed" assumption and its variants were presented very clearly in Sect. 2.

P6739, L6-7: *because tagged water from different parts of the source area will compensate for each other*
Is not the reason rather that the moisture has more time to mix vertically?

P6739, Eqs3-4:
(i) There seem to be factors missing that account for the thickness of the pressure intervals, i.e. Δp.
(ii) The sums should run from p_1 to p_n, where n is the number of vertical layers, or something the like, rather than from p_0 to p_s, which would be correct for the continuous form (with an integral rather than with a sum).

(iii) When splitting up the measure proposed by Goessling and Reick (2013) into two orthogonal horizontal components, the results become sensitive to the choice of the coordinate system. Imagine a situation where the moisture flux in the lower half of the atmosphere is directed towards $(a,b)^{\mathrm{T}} = (1, 0.01)^{\mathrm{T}}$ and a flux of similar magnitude in the upper half is directed towards $(1, -0.01)^{\mathrm{T}}$. The flux shear factors would be $F_{a,\mathrm{shear}} = 1$ and $F_{b,\mathrm{shear}} = 0$. However, if the coordinate system is rotated only slightly, the flux shear factors become $F_{a',\mathrm{shear}} \approx 1$ and $F_{b',\mathrm{shear}} \approx 1$. Now, if the two components F_z and F_m were separately discussed in the manuscript, that would be an argument for using this two-component variant rather than the original measure despite of the above described disadvantage. However, since this is not the case, I wonder why the authors do not just use the more robust measure used in Goessling and Reick (2013).

P6741, L27-28: *the vertical transport could be parameterized or obtained from the water balance*
Is it possible to elaborate somewhat on these two possibilities?

P6743, L6-7: *this model is very fast and flexible for the larger scales, especially in its updated form (WAM-2layers)*
When it comes to applying WAM-2layers at large, maybe global, scales, I surmise that the interface between the lower and the upper layer should not be at a constant height because the strongest shear will be at different heights in different regions and seasons. In contrast to the case considered here, In many regions, particularly in the extratropics, there will not be a distinct height with maximum shear at all. I suggest to add a comment on this.

3 Technical corrections

P6724, L3-4: *spatial extent of the research question*
I recommend using a different formulation because the area under investigation rather

than the research question itself has a spatial extent.

P6725, L13-14: *A completely different approach using stable isotopes of water:* $\delta^2 H$, $\delta^{18} O$ *and especially the corresponding d-excess value have been shown to be a good indicator for moisture recycling*
The part before the colon seems to be grammatically wrong.

P6725, L24: *GCM*
Please explain this abbreviation here rather than later (P6726, L17-18).

P6725, L25-26: *statistical distribution of moisture origin*
I find the choice of the word "statistical" odd here, because what is probably meant is the spatial or spatio-temporal distribution.

P6726, L1-2: *evaporation, use*
Is this comma correct?

P6729, L22: *highly detailed studies*
Are (regional) high-resolution studies meant?

P6730, L18-19: *Our case study consist of the same case as studied by Knoche and Kunstmann (2013)*
(i) "consist" misses an "s".
(ii) The formulation of this sentence could be improved.

P6733, L3-4: *the recycling fraction is a factor three lower than the RCM-tag simulation*
There is something missing here.

P6734, L19:
Missing "the" between "is" and "well mixed".

P6736, L2: *climatology*
I find this word odd here, maybe something like "prevailing winds" or "prevailing atmospheric conditions" would fit better.

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P6736, L8-9: *but failed mainly in getting to recycling ratios right*
Something is wrong here.

P6737, L7: *let to an underestimating*
Something is wrong here.

P6738, L23: *where*
Please remove the "h".

P6740, L21-22: *we assumed that the RCM-tag method was able to simulate the "virtual reality"*
This formulation seems odd. I suggest something like 'we considered the results obtained with the RCM-tag method as the "virtual reality"'.

P6741, L18: *In the WAM-2layers 3D-T (all runs) methods*
There seems to be an "and" missing between "WAM-2layers" and "3D-T".

P6746, L14:
An update is available to this reference, see reference Goessling and Reick (2013) below.

P6749, L14:
The year of publication is missing in this reference.

References

Goessling, H. F. and Reick, C. H.: What do moisture recycling estimates tell us? Exploring the extreme case of non-evaporating continents, Hydrol. Earth Syst. Sci., 15, 3217–3235, doi:10.5194/hess-15-3217-2011, 2011.

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