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Discussions

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

We would like to thank Helge Goessling for his comments on our manuscript. The referee identified four main issues that he thinks require further improvement. We agree with most of the comments and we will adjust our manuscript accordingly. Below we give a detailed reply.

2 Specific comments

P6727, Eq1: (i) Please consider denoting the units of the variables in the text, in par-

ticular for S_g (presumably $kg\ m^{-3}$?).

We actually wrote a water mass $S(t,x,y,z)$ of which a certain part S_g is tagged. This thus means that the dimension of S_g is mass M. We think that it is irrelevant to note here whether this is expressed in kg, g or another unit of mass. If you assume the density of water a constant the dimension of S_g could also be L^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).

This is correct and we will add such a comment.

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 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:

$$\frac{P_g}{P} = \frac{S_g u}{Su} = \frac{S_g v}{Sv} = \frac{S_g w}{Sw} = \frac{S_g}{S}$$

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).

This is a very good point of the referee and we thank him for noting this mistake. Indeed we should have written it down without the spatial and temporal derivatives.

(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 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.P_g

P denotes all water that is removed from the atmosphere by the precipitation process. We will make this more clear when we introduce the symbol P.

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?

Yes, we will follow the referee's suggestion here.

P6728, L12: $\frac{\partial S_g}{\partial t}\Big/\frac{\partial S}{\partial t} = \frac{\alpha_g}{\alpha}$ 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. $\left[\frac{\partial S_g}{\partial t}\right]_{residual}\Big/\left[\frac{\partial S_g}{\partial t}\right]_{residual} = \frac{\alpha}{\alpha_g}$

Actually, this is not what we meant to indicate with the formula, but the confusion is caused because we did not write it down correctly. We will change this to $\alpha_g = \alpha\frac{S_g}{S}$, and for models that do not consider the non-closure of the water balance $\alpha_g = 0$.

P6728, L22-25: I think one could make more clear what is meant with the different flavours of the "wellmixed" 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 "wellmixed" assumption are very central to understand the differences between the tracing methods.

We will add these equations to the manuscript.

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.

We will rewrite this sentence to make clear that 3D-T is a variation on QIBT, but instead of vertically calculating on quasi-isentropic (potential temperature) coordinates 3D-T calculates vertically on pressure coordinates and uses the vertical wind speed to calculate vertical motion of the tracked parcels.

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.

This is indeed the case, but this becomes only relevant in the case study where 3D-T is used. In general the 3D-T method is not be limited to forward tracking only. We will add further clarification that all three models run in forward mode when we introduce the case study.

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.

We will discuss the three models used here in more detail in the revised manuscript.

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 $(1cm)^2$ 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.*

We will change this to: "Lake Volta is a very active component of the regional hydrological cycle". An experiment with a RCM with Lake Volta removed is beyond the scope of comparing moisture tracking methods and as such beyond the scope of this paper.

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.

Good point, we will add the high spatial resolution as well.

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

OK

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.

This is correct, the amount is not proportional to the total column moisture, what is

meant is that the precipitation efficiency is assumed the same for all moisture in the column. We will rewrite this sentence accordingly.

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?

Whether the moisture has more time to mix vertically has to do with the temporal resolution of the results you are interested in. The size of the source area in that respect is less relevant. We intend to keep our original sentence here.

P6739, Eqs. (3)-(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).

We will rewrite this equation correctly in its continuous form.

(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 $(a, b)^T = (1, 0.01)^T$ and a flux of similar magnitude in the upper half is directed towards $(1, -0.01)^T$. The flux shear factors would be $F_{a,shear} = 1$ and $F_{b,shear} = 0$. However, if the coordinate system is rotated only slightly, the flux shear factors become $F_{a',shear} \approx 1$ and$F_{b',shear} \approx 1$. 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).

This is a fair point by the referee, however, we do see value in knowing in which direction this shear mainly occurs. We will discuss the figures of and F_m separately in the revised version of the manuscript.

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?

Some of the authors have a paper in preparation that is using WAM-2layers globally, where we will discuss this in more detail.

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.

We will add this comment.

3 Technical corrections

We will adapt the manuscript accordingly to the technical corrections except for the points below.

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

Not exactly as it is also refers to the "detail" of the research question itself, which can also refer to a high temporal resolution.

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

It was not missing. The year is 2012.

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