

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 the editor for his positive remarks and constructive feedback on our manuscript. Besides some textual suggestions the editor raised one important issue:

“However, there is one consideration (also raised by Helge Goessling) that is not entirely clear to me, and so could also be confusing to other readers. The “well-mixed” assumption (Eq 2) leads to the strategy to distribute evaporated water proportional to

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the vertical water vapor distribution. You state that at coarser resolution this assumption may hold, but is violated at finer resolution. I would have expected an explicit notion that the spatial dimension of the source area do play a vital role here. For evaporation to reach the upper atmosphere, time is needed, depending on turbulence and convection. I would expect that at typical vertical displacement heights of water the air parcels may have moved 100s of km horizontally before the water has reached the mid free troposphere at, say 5km height. Therefore, it does not come as a surprise that the original 3D-T method is improved a lot when implicitly taking the relationship between source area size and effective source height into account (by reducing the effective source height). I therefore recommend an explicit description of the relationship between source size and the required release height.”

This is an important issue raised by the editor, which we should clearly clarify. Actually, both the temporal and spatial resolution of the input data is an issue. We completely rewrote the text around Eq. (2) and we added a new equation describing the problem of a coarse temporal resolution. Additionally, spatial resolution could be an issue due to the absence/presence of a convective scheme. For a more detailed explanation we refer to the new text between Eq. (1) and (4). Moreover, in the concluding remarks we now state that the investigation of exactly which initial mixing assumption would be appropriate for which input data is beyond the scope this paper.

2 Specific comments

Below we list the textual suggestions given by the editor and our replies.

“Introduction: it would be good to justify the choice of your case study domain already early: a wet area/season (monsoon) with a complex atmospheric flow structure (West Africa) and the presence of a clear water source (Lake Volta): this combination challenges your tracking methods nicely”

This is an excellent suggestion. We included a justification for the case study choice in the second-to-last paragraph of the introduction, where we describe the aim of our

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“P2, right column, last para: “. . . in a moisture tracking model it can be desirable to add the water to higher levels. . .”: this only applies to offline (a posteriori) tracking models”

We added that this only applies to a posteriori models.

“P3, below Eq 5: “spatial resolution . . . increase”: I suggest to use “gets finer” instead of “increase”, as “resolutions” is sometimes ambiguously interpreted as the size of the grid cells”

Based on this comment, we decided to remove this whole sentence here, because this obviously lead to confusion. In fact, the resolution of the input data might affect the validity of the equations in a way that is not straightforward and even different for the individual equations. Thus, this sentence was also not entirely correct. Based on the main comment of the editor we added an additional description of how input data resolution affects the validity of Eq. (2).

“P3, right column, top: you may refer to vdHurk and v Meijgaard here as an example of a study that applies the tracking backward in time”

We added this study and a few other studies as well that are examples of backward tracking.

“section “RCM-tag”: is the “cumulus” parameterization in fact a “convection” parameterization? Or is it something else?”

It is, we have now put the word convection in brackets behind it and rewritten the paragraph slightly to reflect better what is argued about this parameterization in the accepted version of Knoche and Kunstmann (2013).

“P4, left column, top: “ $S = S_t$ in Eq 1”: there is no S_t in Eq 1”

The editor is right here, but we meant to say that in Eq. (2) the water storage could be replaced by total column water storage. We now note that explicitly.

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“same para: “the its simplicity” -> “to its simplicity””

OK

“case description (section 3): a bit more “environmental” information on the case study could be given: the selected months are in the heart of the monsoon season; fig 1 could use a geographical referencing on which one can recognize West Africa”

We included another panel showing the location of the study domain within Africa.

“3.1, 2nd para: evaporation “clearly” being highest where precip is highest is not immediately obvious from fig 2a and b, who show quite different patterns”

From our interpreting of the figures this was the case (at least on land). We modified the sentence as follows: The land evaporation (Fig. 2a) seems highest at the places that receive the most rain (Fig. 2b).

“Results in 3.2.1-3.2.2: I think it would be good to describe already in these sections that the patterns of tracked moisture follow the surface winds clearly in RCM-tag, while the upper winds dominate the results in WAM”

In 3.2.1. we changed the second sentence to: The general pattern is that moisture is first transported by the surface winds (Fig. 1b) to the north-east until it reaches higher levels of the atmosphere, after which it is picked up by the African Easterly Jet (Cook, 1999) transporting moisture westward (Fig. 1c).

In 3.2.2 we changed the first sentence to: Figure 4 and Movie 2 (Supplement) show that according to WAM the evaporated water from Lake Volta appears to be primarily transported to the west, thus the moisture transport in WAM is apparently dominated by the upper winds (Fig. 1c).

“3.2.1, 2nd para: insert “areas” between “also reached” and “south of Lake Volta””

OK.

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“3.3.2, 2nd para: to me it is not clear what you refer to with “peak intensity””

The peak intensity refers to the patterns being less smooth (having higher peaks). We have rewritten this paragraph in such a way that peak intensity is not mentioned anymore.

“same para: start a new sentence at “The parcel trajectories””

OK

“3.4.4: I would make explicit that you derive this conclusion from the implicit evidence that RCM-tag and the modified 3D-T methods give comparable results”

We have now started the second sentence with: We derive this conclusion from.

“section 4, 2nd para: the relation between the size of source area and presence of vertical wind shear does need some specification of this size. From fig 11 it is clear that only for source areas of (sub) continental scale this compensation from different areas in the source area could be true.”

We agree with the editor that you would indeed need source areas of (sub)continental scale for this to compensate for wind shear effects in WAM. However, we stated that this is just a general reasoning and at this point in the paper we did not yet introduce Fig. 11. Therefore, size specification would in our opinion not really fit here. However, later in the manuscript, we do state the suitability of WAM for different scales in Table 3, which, based on our explanation here, we hope the editor accepts as sufficient specification.

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