

## ***Interactive comment on “Atmospheric water vapour tracers and the significance of the vertical dimension” by H. F. Goessling and C. H. Reick***

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Answer to Review by H. Sodemann

Overall response to both reviews:

One main point of criticism, on which both reviewers agree, is that the manuscript is too long and, associated with this, that the main results are somewhat hidden behind less important details. We therefore decided to implement the following main changes to the manuscript. We will (I) put the description of the WVT implementation (ch.4) into appendix A, and (II) put a strongly reduced version of the 3D vs 3D comparison into appendix B. The main part of the paper will then be much more concise and focussed on the main results, i.e. the assessment of the ‘well-mixed’ assumption and 2D moisture

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tracing. We also decided to revise the title of the paper accordingly. Moreover, we will reduce the length of the manuscript further by leaving aside some less important parts (including a few figure panels).

In the following we respond only to those issues raised by the reviewer (repeated in italic letters) that have not become obsolete with the above said.

*The paper contains far too many figure panels, and touches on too many aspects, which firstly makes the key results difficult to identify, and secondly distracts the reader with too many details. Now the manuscript contains 62 figure panels, distributed into 16 Figures. I suggest to remove roughly half of the figure panels, and to show the information in 10-12 Figures as mentioned in the detailed comments below. In order to streamline the writing, I suggest to place part of the more detailed explanation into Appendices, and to move the presentation of the two 3D-variants to the very end of the manuscript.*

The number of figure panels will be reduced considerably by strongly reducing the 3D vs 3D comparison (and putting it in the appendix). Also, we will put the description of the WVT implementation into an appendix and leave out other figures (as suggested by the reviewer, see below).

*I suggest to add subheadings throughout the manuscript to guide the reader in the presentation of the material (see detailed comments below). All figure panels should be identified by letters and used when referring to the panels from the text.*

We will implement these reasonable suggestions as close as possible to the reviewer's precise suggestions.

*The main aspect of the manuscript is the comparison with the 2D method and its basic assumptions, which is currently not expressed in the title. I suggest to change the title to include this, e.g. "Comparison of 3D and 2D water source identification, and the validity of the well-mixed assumption".*

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With our decision to discard the 3D vs 3D comparison from the paper, the title is indeed not quite suitable anymore. The revised version of the paper will have a more meaningful title along the lines suggested by the reviewer.

*It needs to be more clearly pointed out that the uncertainty of the AGCM WVT results, even though the method is more complex than the 2D approach, is also substantial and concerns many aspects of the model's concrete numerical implementation.*

This is of course correct. What we meant is the maximum uncertainty associated with vertical redistribution within a “perfect model” framework. We will clarify this.

*Pg. 30121, L28: it could be noted here that AGCMs also only represent many unresolved processes as parameterisations, and therefore have their own unknown uncertainties.*

We meant to cover this aspect of model deficiencies by the mention of “inevitable model biases” (P30121,L26).

*Which state variables would be needed that are not in, say, the ERA Interim reanalysis data?*

One would at least need to know the transfer coefficients for vertical eddy transport. Depending on the turbulence closure used in the reanalysis model, it is not straight forward to derive these from the mean quantities that are provided in the reanalysis data sets (Think for example of TKE, which is handled as an additional prognostic variable when a TKE closure is applied). In any case, the limited temporal resolution of the reanalysis data will be a problem because the transfer coefficients are subject to strong variations on the subdiurnal timescale.

*Should not only the temporal resolution be relevant here, since the spatial resolution usually remains unchanged in the output data?*

Yes, we will correct this.

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*Vertical level changes of water vapor also happen with non-turbulent motion, such as large-scale ascending/descending motion during meridional transport. One question is certainly whether an AGCM at  $\sim 2$  degree grid spacing is able to resolve such transport, or whether it just becomes “turbulent diffusion”. Numerical diffusion is a further relevant redistribution process that should be mentioned, in particular at low vertical resolution (such as the L47 applied here).*

The large-scale ascending and descending motions, such as those associated with the Hadley- and Walker-circulations, or those associated with synoptic-scale atmospheric Ekman pumping, are captured explicitly by an AGCM even with a 2 degree grid. Our sentence was somewhat ill formulated as the vertical transport by large-scale vertical motion is not mentioned - we will correct this. We will also mention numerical diffusion.

*While it does not seem necessary here to give a complete literature overview on the offline 3D moisture tracing methods, the distinction between 3D and 2D methods and the focus on the latter for this study should be pointed out more clearly.*

OK.

*Pg. 30123, L28: This paragraph could be removed from the introduction, it seems somewhat confusing here to make reference to results that are only presented later on as a motivation already here.*

Since the new version will not contain the 3D vs 3D comparison, this sentence is obsolete.

*L. 24: I would think that this is a question of grid spacing. With increasing horizontal grid spacing the need to parameterise convective processes decreases. It could also be mentioned that there is always some degree of numerical diffusion. As you show later on in Fig. 2, vertical transport is not in all regions and at all seasons dominated by convective processes. I suggest to revise this section to make clear that AGCMs do represent a substantial part of the vertical motion explicitly, otherwise one would not be*

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able to e.g. calculate meaningful three-dimensional kinematic trajectories from model output.

Yes, but the scale at which convective processes begin to be resolved explicitly is two orders below the 2 degree (which is about 200km), namely the order of the boundary layer height, i.e. 1-2 km (Cloud-resolving models). Otherwise, see above.

*Pg. 30128, L. 8: In order to shorten the manuscript, the material in Section 2 up to here could be placed in an Appendix, and the writing from here on could be left in the main text and extended to summarise the findings derived in the Appendix.*

We consider section two rather central to our paper and hence prefer to leave it in the main text. Instead, we will place most of section 4 and a reduced version of section 6 in appendices (as the reviewer suggests elsewhere).

*Pg. 30128, L. 25: Please mention how many levels are in the troposphere, and the approximate altitude of the lowest model layer. Somewhere it would be good to have a comment on the influence of vertical/horizontal model resolution on your results.*

Given a surface pressure of 1013hPa, the model has 22 levels below 200hPa. The lowest level is about 8 hPa (~60 m) thick. We will include this information. However, an assessment of the influence of vertical/horizontal model resolution on the results is beyond the scope of the paper.

*Pg. 30129, L. 5: I suggest to remove Fig. 1, since a precipitation/evaporation climatology is not really necessary for the further results, and much of the later discussion and interpretation could refer to Fig. 2 instead.*

We will follow this suggestion.

*L. 24: This is just one possible metric for the relevance of convection. I am a bit puzzled by the high values in the North Atlantic in January. Does the convective precipitation parameterisation you use include both deep and shallow convection (as the Tiedke scheme typically does), and can you distinguish the two? The latter will only be*

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*important for boundary-layer venting of moisture, not for deep mixing.*

Yes, this is indeed just one way to quantify the strength of moist convection. In ECHAM6, moist convection is divided into deep, shallow, and midlevel convection (see Stevens et al., “The Atmospheric Component of the MPI-M Earth System Model: ECHAM6”, submitted to JAMES - we will include this reference also in the revised version). The high values in the North Atlantic are mainly due to midlevel convection, meaning that not all but at least the lower portion of the free troposphere is subject to mixing. Shallow convection does not contribute much to the metric because it is rarely associated with sufficient amounts of precipitation (we chose 10

*Does the strength of horizontal advection not also play a role in the maintenance of inhomogeneities, since the mixing will not have enough time to remove vertical gradients with faster advection speeds? This question implies also a scale dependence, the larger the region considered, the easier it will be for the mixing to remove existing gradients.*

This is true. However, higher horizontal advection speeds will not be able to generate vertical inhomogeneities, at least in case of “barotropic” (i.e. vertically uniform) flow. They would only enable vertical inhomogeneities to “survive” a longer distance. And yes, the scale dependence is present.

*Fig. 3 presents much more information than is used in this paragraph. Part of the information in Fig. 3 is used only much later. Consider removing the Figure or some of its panels.*

We would like to keep this figure here because it gives the reader an intuitive picture of how the atmospheric flow is vertically structured. We thought of discarding the January panels, because we refer more to the July panels, but decided to rather keep them for the sake of consistency throughout the paper.

*Is this the same advection scheme that is also used for the total specific humidity? How*

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*is precipitation formed from water vapor, are there cloud water and cloud ice species?*

Water vapour, liquid water, and frozen water are all advected individually by the Lin-Rood scheme.

*Some clarification is needed on how precipitation flux is formed in ECHAM. Is precipitation supposed to fall directly after condensation (i.e. no cloud water present in the model)?*

There is liquid as well as solid cloud water in ECHAM6. Those droplets that exceed a critical size fall as precipitation directly to the ground.

*It is not fully obvious to me that what is done to ensure consistency between the tracers and the total moisture field has a clear physical basis, even though I agree that it is desirable to remain internally consistent during a model run. What is the effect of this approach to fixing mass conservation issues on the tracer amount in numbers? In other words, is it less than a percent that needs to be filled, or more substantial at each time step? How about assigning the added tracer moisture to an additional “consistency tracer”?*

We experimented with different approaches to this problem, including the addition of a consistency tracer where the right-hand-side of Eq. (15) is negative (implying a spurious negative (upward) precipitation flux). However, the amount of added consistency tracer was minute (mostly less than a permill compared to  $q$ ), and the results were virtually insensitive to this.

*Section 4.2 can be removed completely, or reduced to 1-2 sentences, without compromising the credibility of the results. Figure 5 should be deleted as well.*

Agreed.

*This result should be made more quantitative: Which fractional area is well mixed? What is the typical value of the metric  $z^*$ ?*

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Frankly speaking, we do not consider additional numbers on the areal extent of “well-mixed” conditions here to help much. Such numbers will be strongly sensitive to the choice of the threshold for what is considered to be well-mixed, and the spatial patterns seem to be much more interesting than a global number. Regarding the second question: The typical value of  $z^*$  is, quite independent of latitude and season, about 1.5 km (850 hPa). We will include this information.

*Deep moist convection is certainly active in the tropics, but what kind of convection is active in ECHAM in the extratropics?*

In the extratropics it is rather midlevel convection, see above. We will correct the corresponding sentence accordingly.

*It could have been insightful to test areas of different size, to provide an intermediate result between the continental source experiment and the regional experiments. Can you comment on the influence of the domain sizes on your results?*

As both reviewers have realised, there is a degree of scale dependence, which one can already see from the two scales we consider, and which we will mention in the revised paper. However, adding experiments for an intermediate scale to investigate in more detail the scale dependence would add even one more dimension to the paper, which is, as the reviewers point out, already quite rich in content (and, hence, length). We would therefore like to refrain from performing additional experiments.

*Strictly speaking only one uncertainty in the moisture tracing procedure is tested. There are many other influences that are not touched upon in this study, including the choice of e.g. the convective parameterisation, the horizontal and vertical grid sizes, the numerical schemes etc. Further influences are from the model setup, including SSTs, solar forcing, atmospheric variability etc. This is not to say that the sensitivity test is not meaningful, but it only covers one specific aspect. This is one motivation for taking the WVT method to a regional NWP model (as done in Sodemann et al., 2009; Winschall et al., 2012).*

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This is of course true. We will (then in the appendix) make more clear that we are assessing only the uncertainty associated with the degree of mixing between precipitation and ambient water vapour within a “perfect model” framework. That is, we can not assess the realism of the tracing with respect to the real world.

*Is it not surprising that despite the well-mixed criterion being violated more often than not the results of the 3D and 2D tracking still look quite similar? How strongly needs the criterion to be violated for the 2D tracking to produce wrong results?*

This is a good question. If vertical inhomogeneities lead to large errors or not depends not only on the degree of inhomogeneity, but also very much on the degree of directional shear of the winds. This is exactly the reason why the errors are much larger in the tropics than in the extratropics, even if the degree of vertical inhomogeneities is comparable.

*Please add some quantitative comparison on the total fractions contributed by all areas during the whole year. Make a reference to Table 2.*

Since R. van der Ent fortunately provided us with such numbers, we will be able to incorporate them into the revised version.

*These paragraphs on the comparison to ERA-Interim can be deleted, as well as the corresponding Figures 15 and 16. A comparison to ERA Interim seems clearly beyond the scope of the manuscript.*

Agreed. We will only mention the comparison with 1-2 sentences.

*Pg. 30151, L. 4-10: This paragraph can be incorporated in the comparison to the 2D method.*

Agreed.

*L. 11-26: include this in the revised discussion of the 3D variants*

*L. 27-Pg. 30152, L. 18: Shorten these paragraphs to 1-2 sentences*

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Much of these paragraphs is now obsolete since we largely omit the 3D vs 3D comparison.

*In general, all Figure panels should be marked with a letter and be referred to from the main text (e.g. Fig. 8a, etc.).*

Agreed.

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