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Atmospheric Chemistry and Physics Discussions

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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The manuscript presents an innovative comparison between forward three-dimensional water vapor tracing using an AGCM, and conceptional much simpler two-dimensional tracing. Thereby rigid testing of the often-used well-mixed assumption is made. The analysis reveals shortcomings of the simpler method in certain regions which are explained and discussed in relation to the existing literature. The paper is in general of high quality and fits the scope of ACP well. However the presentation of the material needs to be improved substantially. In particular, the number of Figure panels needs to be reduced, and the writing should focus more strongly on the main findings. I therefore recommend major revisions as detailed below.

Major comments

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Interactive Discussion
Discussion Paper

1. 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.

2. Again on the point of restructuring the paper, 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.

3. 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".

4. Some clarification is needed on the WVT implementation concerning the tracer adjustment during precipitation formation.

5. 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.

Detailed comments

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

Pg.20122, L. 5: Which state variables would be needed that are not in, say, the ERA

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Interim reanalysis data?

L. 6: Should not only the temporal resolution be relevant here, since the spatial resolution usually remains unchanged in the output data?

L. 9: 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 ∼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).

L. 12: 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.

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.

Pg. 30125, L. 4: could add subheading here

Pg. 30126, L. 14: could add subheading here

L. 22: remove "As a side remark"

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

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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.

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.

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.

L. 7-11: Remove, not necessary to repeat this here.

L. 16: Could add a subheading here

L. 17: Replace 'typically' by 'To a first order approximation', or the like. Boundary layer height and mixing depend quite strongly on latitude, season and time of day (to name just a few parameters).

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

Pg. 30130, L. 6: Could add subheading here

L. 9: 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.

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Interactive Comment

Pg. 30130, L. 20-25: 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.

Pg. 30132, L. 12: Is this the same advection scheme that is also used for the total specific humidity? How is precipitation formed from water vapor, are there cloud water and cloud ice species?

L. 25: I suppose the sum of all WVTs is made equal to the prognostic moisture, not the other way round.

Pg. 30133, L. 1: 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)?

Pg. 30135, L. 17: 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'?

The material contained in this section up to here is very intricate and should be placed in an Appendix, leaving only a summary discussion in the main manuscript.

L. 18: Could add subheading here

The discussion of the two variants of mixing seems excessive in this manuscript, given that the results are in the end very similar. I suggest to remove this section here and to comment on this topic very briefly in a separate Discussion section in the end (see other comments below). This will simplify the complete further presentation of the material, since all references to the two cases can be deleted up to the Discussion.

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Pg. 30137, L. 25: 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.

Pg. 30139, L. 20: This result should be made more quantitative: Which fractional area is well mixed? What is the typical value of the metric z*?

Pg. 40140, L. 4: Deep moist convection is certainly active in the tropics, but what kind of convection is active in ECHAM in the extratropics?

Pg. 30140, L. 13: 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?

Pg. 30141, L. 9: Add subsection here

Pg. 30142, L. 19: Add subsection here

Pg. 30143, L. 8: 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).

I suggest to strongly revise Section 6 to present the results in a more condensed fashion. Firstly, the Section should be moved after Section 7 and become part of the Discussion. Pg. 30144 L. 1-22 should be removed or strongly shortened. In Fig. 8 the lower four panels should be deleted, and the remaining panels be incorporated to Fig 10. The comparison with 3D-s should be removed from Fig. 10.

Similarly, the two top panels of Fig. 9 should be incorporated with the top 4 panels of Fig. 11, and all remaining panels of the two figures removed.

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Furthermore, one of the 3D-variants should be removed from Fig. 13 and Fig. 14. The results are indistinguishably similar by eye, and there is no need to show a figure for that, a sentence in the text would be sufficient.

This way the discussion in Section 7 can be condensed heavily, as all references to the two 3D methods can be removed at this point. This makes the main findings of the paper much more visible, right now they are hidden in numerous small figure panels.

Pg. 30147, L. 5: 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?

L. 6: Could add subheading here

L. 26: Could add subheading here

Pg. 30149, L. 19: Please add some quantitative comparison on the total fractions contributed by all areas during the whole year. Make a reference to Table 2.

L. 25 to Pg. 30150, L. 22: 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.

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

L. 11: Could add a subheading here

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

L. 19: Could add a subheading here

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

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 30119, 2012.

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