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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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Answer to Review by R. J. van der Ent

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 mois-

ture 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.

Furthermore, the paper should be better embedded in the current literature. The authors now focus almost exclusively on the biases in a specific part of a single study: Keys et al. (2012), hereafter K12, whereas many other studies and their assumptions are left largely untouched.

We would like to stick to the analysis of this specific case because it is quite instructive. While a detailed analysis of other methods is beyond the scope of the paper, we will slightly enhance the discussion of alternative methods of offline moisture tracing.

It is praiseworthy that the authors compare the ECHAM6 data of this study to ERA-Interim data, but Figure 15 and 16 could easily be transferred to the supplementary material.

We will omit these figures and only mention the results of this comparison in 1-2 sentences, going without supplementary material.

However, according to my own calculations of the results presented in K12, the SW region (Fig. 12) is contributing 10% of the rainfall in the West-Sahel during the growing season (June to October), which is admittedly less than the 20-25% (derived from Fig. 13 in GR12), but substantially more than the 4%, which is mentioned by GR12 for 2D tracing. Similarly, I have calculated 26% for the contribution of the N region (Fig. 12), which is much lower than the 40% mentioned in GR12. I do not immediately know what causes the discrepancies between K12 and GR12, but I think it would be fair to note that the findings in K12 are less dramatic than suggested by GR12.

We will include and comment on these interesting results in the revised version of the

paper.

In the abstract this criticism boils down to: "which reveals the results of an earlier study as spurious". I consider this terminology inappropriate, especially for an abstract. I recommend this language to be removed entirely, replaced by more appropriate language (as in the conclusion), or the language to be adjusted to target the specific result that is found to be spurious, so as not to misrepresent either the extent of the research conducted in the paper, or the uncritiqued portions of K12 e.g. the precipitationshed concept and the vulnerability analysis.

We will use more appropriate terminology and make clear that we do not at all criticise any other parts of K12.

Moreover, the specific mention of K12 might give the wrong impression that this is the only study that suffers from the 'well-mixed' assumption, or erroneous assumptions in general. The studies using the model of Dominguez et al. (2006), which also use 2D water vapour tracing, albeit in a Lagrangian manner, are completely left unmentioned (e.g. Dominguez et al., 2008; Bisselink and Dolman, 2009; Dominguez et al., 2009). Moreover, the quasi-isentropic back-trajectory method (Dirmeyer and Brubaker, 1999, 2007) is only referred to as a sophisticated approach (30122-11 - 301222-13). Surely, this method is more sophisticated than 2D moisture tracing and it does not use the well-mixed assumption for horizontal transport, but still evokes the well-mixed assumption for the release and recovery of their water vapour tracers (precipitation and evaporation) and neglects vertical transport. In fact, application of this method leads to similar biases in the West-Sahel region (see Dirmeyer et al. (2009) and the associated website http://www.iges.org/wcr/), which becomes clear when you look at the contributions to Burkina Faso and this was also noted by Paul Dirmeyer himself (Dirmeyer, 2011). The vertical transport, which was introduced to this model by Tuinenburg et al. (2012), can, as mentioned in this paper, only partly be handled due to the subgrid-scale processes dominating the vertical transport when working on large grids (30126-22 - 30126-25). In a forthcoming paper (van der Ent et al., in preparation), we will reveal this vertical

transport to be a large source of uncertainty in water vapour tracing. Obviously, the literature mentioned above deserves some attention in this paper as well.

We will slightly enhance the discussion of these alternative methods. However, the way our study is designed we can quantify errors only of the 2D tracing method, and can only speculate on the implications of our findings for the other methods. We would therefore like to leave the main focus as it currently is.

It should also be noted that the a posteriori or offline methods (e.g. Dirmeyer and Brubaker, 1999; Yoshimura et al., 2004; Dominguez et al., 2006; van der Ent et al., 2010; Goessling and Reick, 2011) also have advantages above the online water vapour tracing deployed in this paper and other papers (e.g. Bosilovich and Schubert, 2002). Namely, that the offline methods are far less computationally expensive, allow for backward tracking, and are thus much more flexible. The disadvantage is however that for at least precipitation the well-mixed assumption must be invoked as there is in general no other factual information present to do otherwise. The widely applied FLEXPART method by Stohl et al. (2005) avoids invoking this well-mixed assumption, but as a consequence cannot diagnose surface fluxes of moisture, but only the fluxes into or out of the tracked air mass (Stohl and James, 2005). The (dis)advantages of the several methods mentioned above do not need to be discussed in detail, but deserve more attention than currently given in the paper.

Regarding the advantages of offline tracing, we wrote that "offline moisture tracing is computationally less expensive and can be applied to different kinds of data including reanalyses that arguably constitute the best guess of the evolution of the global atmospheric state during recent decades" (P30122,L1-3). We will add the possiblility of backward tracing to this list of advantages. Moreover, we will slightly enhance the discussion of the above mentioned other offline methods.

30122-11 - 30122-17: Several offine moisture tracing techniques have been developed that cope with the limitations of reanalysis-like data. Among these are sophisticated ap-

proaches like the Lagrangian particle dispersion method (e.g. Stohl and James, 2004) and the quasi isentropic back-trajectory method (e.g. Dirmeyer and Brubaker, 1999), but also the conceptually simpler approach of 2-D moisture tracing (Yoshimura et al., 2004; van der Ent et al., 2010; van der Ent and Savenije, 2011; Goessling and Reick, 2011; Keys et al., 2012). In the latter case the atmospheric fields are integrated vertically before the tracing is then performed only in the horizontal dimensions. As mentioned under 'literature treatment and assumptions' this part should be build out further and preferably not only in the introduction. As a suggestion, the authors could make use of a recent review by Gimeno et al. (2012).

Agreed. However, we would like to constrain the discussion of other offline methods mainly to the Introduction because the way our study is designed we can make strong conclusions only about the 2D method itself.

Eq. 3 and 4: The symbols $\hat{q}$ and $\hat{q}_i$ are somewhat confusing as they appear to have other units than q and q_i

They do have other units indeed (kg m^{-2}), but we do not see why this should be confusing.

30130-29 - 30131-12: We therefore introduce . . . as directional shear. I really like this elegant metric, but to me it makes more sense to immediately give the metric Γ its name on line 30131-1 instead of waiting until 30131-12. Moreover, I think makes more sense to call the metric the directional shear coefficient and refer to the phenomenon as directional shear. Otherwise it is unclear whether one talks about the phenomenon or the metric.

We agree that the last sentence of that paragraph ("In the following we refer to Γ simply as directional shear.") is misleading. What we intended to say is that when we talk of "directional shear" we mean "directional shear of the horizontal moisture flux". It seems that it is not really necessary to give the metric Γ a name, as our ill-formulated sentence implies. We will correct this.

Is (horizontal) transport of liquid water and ice included in the tracing, i.e. clouds moving from one grid cell to the other? If not, this should be mentioned, and if it is done the term water vapour tracers (WVTs) does not make much sense to me.

The advection of condensed water is included in ECHAM6. We agree that the term water vapour tracers is slightly misleading. We speculate that the term has been introduced as it is because usually by far the largest fraction of the atmospheric water (and its transport) is maintained by the gaseous phase. We will make clear in the revised version that also the condensed phases are transported.

I have the same comment on Ψ_i as on Γ here above.

We will treat Γ and Ψ_i in a consistent manner.

It [Ψ] also reveals that well-mixed conditions are scale-dependent. For Fig. 6, which has a very large source region (all continental areas), it does not look as bad as for the smaller regions in Fig. 7. Although it is rather logical and obvious, I still think it is worth mentioning this finding.

Yes, we will mention this valid point.

I think it is worth mentioning that, at least to my knowledge, backward tracing is not possible within AGCMs.

This is correct - we will include a note.

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