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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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General comments

The work presented in this manuscript, hereafter GR12, investigates the errors and uncertainties involved when using atmospheric water vapour tracers to learn about source-sink relations of evaporation and precipitation. This work is valuable as it evaluates previous research results as well as provides an idea of the uncertainties and errors involved in atmospheric water vapour tracing. The applied methodology seems scientifically sound and the discussion presented at the end of Section 8 (although I think it fits better at the end of Section 9) is a good example of the 'free-thinking' attitude of the authors, which I highly appreciate. The paper is generally well-structured, but its length and the sometimes technical language used makes the paper somewhat

difficult to read. 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. Below, I clarify these main points of criticism and thereafter I give a list of specific comments and technical corrections.

Length

Whereas the main subject of this paper is said to be the relation between 2D and 3D moisture tracing (30143-1 - 30143-2), the amount of text and figures devoted to the uncertainties involved in 3D moisture tracing is almost equal to that devoted to 2D vs. 3D water vapour tracing. Certainly, uncertainties in 3D tracing are an important subject as well, but it is distracting from the main subject. Therefore, I think that Sections 3, 4, 6 and several other paragraphs should be significantly shortened or for a large part be transferred to supplementary material. The same goes for the associated Figures 5, 8 and 9; additionally Figure 5 is unrelated to the core messages of this paper. 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. Please note that my comments on the length should be considered an advice and not an obligation.

Literature treatment and assumptions

The authors devote a significant part of their paper to investigate the difference between 2D and 3D water vapour tracing for a region in the Western Sahel. They conclude: "*2-D moisture tracing strongly underestimates the amount of moisture originating from the tropical Atlantic that, in reality, is transported in the low-level monsoonal layer far into the African continent. Compensatingly, 2-D moisture tracing overestimates the contribution to Western Sahelian precipitation originating from beyond the Sahara*" (30155-2 - 30155-6), and "*It thus seems that the precipitationshed of the Western Sahel as determined by Keys et al. (2012) (see Fig. 3 therein) is strongly biased towards* *the northeast*" (30149-21 - 30149-23). As a co-author of the K12 paper I will be the first to admit that this is indeed the case and GR12 have done a good job in pointing this out and even go through the effort to try and rule-out the possibilities that the biases for the West-Sahel case in K12 are caused by difference in approach (backtracking instead of forward tracking) or data (ERA-Interim instead of ECHAM6). 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.

Nonetheless, I consider much of the criticism on 2D tracing in the West-Sahel region as applied in K12 fair, but 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.

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.

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.

Specific comments

30120-14 - 30120-18: *For example, computed by 2-D moisture tracing, the fraction* *of precipitation in the Western Sahel that originates from beyond the Sahara is 40%, whereas the fraction that originates from the tropical and Southern Atlantic is only 4%. Full (i.e. 3-D) moisture tracing however shows that both regions contribute roughly equally, which reveals the results of an earlier study as spurious.*

As explained above I consider this terminology inappropriate, and a sentence similar to the one in the conclusion (30155-2 - 30155-6) more suitable.

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 approaches 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).

equations (3) and (4).

The symbols $[$\hat{q}$ and $\hat{q}_i$ are somewhat confusing as they appear to have other units that q and q_i

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.

Section 4, WVTs in ECHAM 6

1) 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. 2) As mentioned before I suggest a large part of the text to go to supplementary material, but to keep the text around equation (20) and (21) as the core message. 3) I have to same comment on Ψ_i as on Γ here above.

30139-21 - 30139-23: *Computing Ψ_i for the moisture of continental origin (Fig. 6) and for the moisture stemming from the three rectangular source regions (Fig. 7) reveals that well-mixed conditions are overall rather scarce.*

Not only that, but it 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.

30151-1: *keeping to forward tracing*

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

30152-4 - 30152-6: *This suggests that the composition of the precipitation arriving at the surface is to a relatively large degree determined by the composition of the upper-level atmospheric moisture from which the precipitation originally formed. This seems to be in contrast with something mentioned earlier in the paper: "As an aside, this implies that, at least in ECHAM6, precipitation on average forms at those relatively low levels where there is still a noticeable enrichment of locally evaporated moisture" (30146-16 - 30146-18).*

Can the authors comment on this?

Technical corrections

30121-14 - 30121-17: *To determine the latter one has to recur to numerical tracing of moisture, which can be realised either o?ine, i.e. a posteriori, using suitable data on evaporation, precipitation, and atmospheric transport, or online within an atmospheric general circulation model (AGCM).*

It makes more sense to swop these sentences as that is also the sequence in which they are discussed in the following paragraphs.

30122-26 and 30149-19: *mediterranean*

Should be with a capital letter.

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