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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Dehydration of the stratosphere" *by* M. Schoeberl and A. Dessler

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General comments

Schoeberl and Dessler present an analysis of stratospheric water vapor estimated from foreward trajectories released in the tropical tropopause layer. The dataset underlying the trajectory calculations is GMAO/MERRA. Their results are largely consistent with those of previous studies that were mostly based on ECMWF data, though I noted some interesting discrepancies compared to the study by Liu et al. (2010). The paper is generally well written and results are clearly presented and of relevance to the community. However, a number of statements (listed below) are, in my opinion, not quite adequate, or if they are, the paper should provide more quantitative support.

Specific comments

(items listed A, B, ...)

(A) Forward versus backward trajectories

I am somewhat surprised by the authors' statements regarding forward versus backward trajectories. There are very good reasons to choose a reverse domain filling approach, and re-iterating them here is not necessary. Certainly, both approaches have their pros' and cons', but I think I do not agree with the statements that:

(i) Forward trajectories solve the problem of trajectories that cannot be traced back within the integration period. The advantage of the paper of Schoeberl and Dessler is that they allow for trajectory lengths exceeding 1 year. However, there is nothing that would prevent a similar integration length for back trajectories. (As an aside, I would actually recommend that you give the information that you generally use the last few days of the 10year integration earlier than in Section 3 (page 10167) - this avoids some confusion and makes it clear what the maximum age is in your calculation.)

(ii) I'd argue that contrary to the statement on page 10161/Line 14, the issue of integration length has been discussed in the literature, but technical limitations prevented very long integration lengths until recently. Certainly, the study of Liu et al. also addresses this issue.

(iii) On page 10161/line 15 it is stated that *backtrajectories* neglect 'at least three important processes'; I think that back trajectories per se do not neglect these processes - what you probably want to say is that *previous studies* (independent of forward or backward trajectory) chose to neglect these processes. The reason for this is clearly stated in, for example, Fueglistaler et al. (2005) and Fueglistaler and Haynes (2005): The initial question was to see the performance of the simplest possible configuration that takes transport into account. Once this is established, one can add additional processes. As argued in Liu et al., there is some uncertainty even in the simplest configuration, and making a strong case for a specific process in addition to the advection through the temperature field is probably still not possible at this point. (To make sure there is no misunderstanding here: I have no doubt that a lot of other processes do play a role, it's just that it is difficult to make a case for a few specific ones as there is substantial ambiguity in the problem.)

In summary, I see no justification for statements like 'this approach (forward trajectories) avoids many of the pitfalls of the back trajectory studies, as well as allowing us to investigate issues that cannot be addressed with that traditional back trajectory approach ...'.

(B) Agreement with observations

As discussed in Liu et al., arguing for a specific process to improve agreement with observations only makes sense if there exists an error model for the other components that influence the result, and such an error model is a challenge. Hence, while it is interesting to see that inclusion of certain effects help to improve agreement with observations, it is not necessarily conclusive. (Also, the paper does not even mention the expected accuracy of the MERRA temperature field.) Also, from experience I know that the vertical interpolation scheme has quite an influence on the predicted water, and, if I understand correctly, this study uses linear interpolation which tends to give high-biased results).

It is stated that (page 10163/line 1) all data sets used are daily average fields, in order to keep the data amount manageable. That's fine, but this averaging produces an unspecified bias (for example, it reduces temperature variance and hence leads to a moister stratosphere - see also Liu et al., Figure 10), and it's not clear for example whether the tuneable 'wave' temperature correction mainly serves to restore the variance lost in the averaging process.

(C) P10160/L8: Here, and later in the main text, it is stated that the kinematic trajectories yield a drier estimate than the diabatic trajectories; and that this result is in agreement with Liu et al. (2010). This is not quite correct - for ERA40 the kinematic trajectories yield a moister stratosphere than the diabatic, whereas for ERA-Interim the

differences are more complex. Obviously, the interesting question is why the results disagree; it would be desirable if the revised version could address this issue.

(D) P10162/L2-3: This is not a correct summary of what Liu et al. found: first, they did not use time-smoothed vertical velocities; second, kinematic trajectories from the 4D-var analysis performed in many ways (but not all) similar to the diabatic trajectories.

(E) P10163/L25: If I understand correctly, you delete trajectories at the end of the day that are just below the starting level. Is this not biasing results inasmuch as regions with a quasi-stationary adiabatic subsidence will have a lower chance to enter your statistics? (I understand that you continously release trajectories, but this does not help in the case of a quasi-stationary structure.)

(F) Section 2.4/Gravity waves: What is the spatial scale of the correlation? The text says that you pick a random phase, so effectively your temperature perturbation field is decorrelated at the smallest scale you consider? While this may be ok for the high-frequency waves, it certainly is not a good assumption for, e.g. Kelvin waves. Can you explain?

(G) Section 3, page 10167/Line 12: As said above, it is interesting and puzzling that in your calculations the kinematic trajectories are drier than the diabatic. This discrepancy of results calls for an explanation. You argue (page 10168) that the "spurious vertical motion provides extra opportunities for dehydration" - in Liu et al. we argue that both dispersion and mean residence time play a role, and we argue that dispersion alone in principle should moisten the stratosphere (as it increases the chance that trajectories enter the stratosphere at 'warm' locations - which is the opposite of your reasoning, if I understand correctly, inasmuch as you argue that adding noise increases the probability to be exposed to anomalously low temperatures; is this a correct summary?).

Any comments?

(H) page 10169/line 3: As noted above, Liu et al. did not time-smooth the omega/wind

fields - the improvement is attributed to the less noisy omega field in the 4D-var scheme of ERA-Interim.

(l) page 10171/line 22ff/Figure 8a: I think the distribution of the points of last dehydration over South America is actually not so different to that shown in Fueglistaler et al. 2005, Figure 5a/d (for a brief comment on dehydration over South America, see also Fueglistaler et al. 2004; Section 3.3.2.4). The dipole structure over the western Pacific warm pool is indeed intersting, and is different from the results we get based on ECMWF data. Do you have a guess whether this is this because the temperature fields differ, or because of different transport?

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