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Interactive Comment

Interactive comment on "Improvement of vertical and residual velocities in pressure or hybrid sigma-pressure coordinates in analysis data in the stratosphere" *by* I. Wohltmann and M. Rex

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We would like to thank the referee for reviewing our manuscript.

1. It is correct that temporal averaging of the vertical winds from the continuity equation will reduce the noise in these winds and improve the agreement between the winds from continuity and our thermodynamic winds, which are implicitly averaged over 24 hours. For this reason, we showed the vertical winds from the continuity equation averaged over 24 hours in Figure 1 to allow for a more fair comparison (noted both in the text and the figure caption). As is obvious from the Figure, the winds from the continuity equation are still far more noisy than the winds from the thermodynamic approach.

The results shown in Figure 2 and 3 are based on instantaneous wind fields in case of the continuity equation. The reasoning behind this is that vertical wind fields from the analyses could be used without averaging and “as given” in some applications, and we would like to highlight the possible problems that could show up. In this sense, the presentation of results from instantaneous wind fields in Figure 2 is intentional. Ideally, our thermodynamic wind fields would be averaged over a shorter period, but that decreases the stability of our method and was not possible.

We understand your point that the comparison could be regarded as unfair in this respect. We now have added additional comparisons with a) winds from the continuity equation averaged over 24 hours and b) winds from the continuity equation averaged spatially over the nearest 9 grid points and additionally over 24 hours. An extended discussion of the results has been added to Section 4 and Table 1 (since the results look not qualitatively different, no additional Figure is given).

The additional averaging of the continuity winds does not improve their performance significantly. In particular, the vertical diffusion is still orders of magnitude higher than observed and the residual circulation is still too fast. Surprisingly, while the standard deviation of the vertical winds itself is about a factor of 2 smaller now (taken over the globe for a given date and model level), the standard deviation of the end points of the trajectories is only slightly smaller than before. We discuss this result in an additional paragraph in Section 4.

2. You are right that Eq. (4) is not used in calculating the thermodynamic winds. That could have been stated more clearly directly below the equation and is now done. However, it was stated later in Section 3.

You are right that interpolation to a new isentropic meteorological data set with several fixed θ levels as new vertical coordinate would introduce additional (and unnecessary) interpolations and indeed would be a clumsy method. The trajectory model avoids this pitfall by a smarter approach: Four-dimensional interpolations in longitude λ, latitude

ACPD

7, S7313–S7318, 2007

Interactive Comment

φ, vertical coordinate η and time t are needed in the trajectory model to interpolate wind and temperature from the grid of the analyses to the current position of the trajectory. The unfavourable approach would be to interpolate from a grid $(\lambda_i, \varphi_j, \eta_k, t_l)$ (i, j, k, l indices of the grid points in every direction) on a θ grid $(\lambda_i, \varphi_j, \theta_k, t_l)$ with several fixed θ_k and then to interpolate again on the position $(\lambda, \varphi, \theta, t)$ of the trajectory. In our model, we directly interpolate from a grid $(\lambda_i, \varphi_j, \theta(\lambda_i, \varphi_j, \eta_k, t_l), t_l)$ on the position of the trajectory. That is, the vertical coordinate is not only dependent on a vertical index and has fixed values, but it also depends on the horizontal position and time. This is easily implemented by just calculating θ at the original grid points of the $(\lambda_i, \varphi_j, \eta_k, t_l)$ grid by using the T and p values given there. It is also easily implemented in the 4D linear interpolation. E.g. one of the eight interpolations of temperature T to θ_I changes from $T(\lambda_1, \varphi_1, \theta_I, t_1) = \theta_1 + \frac{T(\lambda_1,\varphi_1,\theta_2,t_1) - T(\lambda_1,\varphi_1,\theta_1,t_1)}{\theta_2 - \theta_1}(\theta_I - \theta_1)$ to $T(\lambda_1, \varphi_1, \theta_I, t_1) = \theta(\lambda_1, \varphi_1, \eta_1, t_1) + \frac{T(\lambda_1,\varphi_1,\eta_2,t_1) - T(\lambda_1,\varphi_1,\eta_1,t_1)}{\theta(\lambda_1,\varphi_1,\eta_2,t_1) - \theta(\lambda_1,\varphi_1,\eta_1,t_1)}(\theta_I - \theta(\lambda_1, \varphi_1, \eta_1, t_1))$ (with $\lambda_1 < \lambda_I < \lambda_2$, $\varphi_1 < \varphi_I < \varphi_2$, $t_1 < t_I < t_2$ as the grid-points we interpolate between and η_1 and η_2 defined by $\theta(\lambda_1, \varphi_1, \eta_1, t_1) < \theta_I < \theta(\lambda_1, \varphi_1, \eta_2, t_1)$ and so on for the other seven interpolations). Now, we shortly explain this approach in a paragraph in Section 2. An Appendix is added to explain the interpolation in detail.

As far as we can tell from the manual, this is basically the same method that the popular FLEXTRA trajectory model (see http://zardoz.nilu.no/ ~andreas/flextra+flexpart.html) uses for interpolation.

We use exactly the same approach to interpolate from $(\lambda_i, \varphi_j, p(\lambda_i, \varphi_j, \eta_k, t_l), t_l)$ to (λ, φ, p, t) in the isobaric implementation of the trajectory model if σ-p model levels are used (pressure levels are just a special case of this).

We did not add a further step-by-step description as requested in your comment, but only extended the description in some places. The description in Section 2 is complete and an additional step-by-step description would only duplicate information. All necessary information (the linear interpolations used, the calculation of p from the interpolated temperature, the staggered grid etc.) is already given in the text. We added an Appendix on the staggered grid.

3. You are right that iteratively interpolating the data would tend to smooth the data. As outlined in the answer to 2, we do only one single interpolation for each data point and variable. This sums up to one interpolation of the wind components u, v and Q at every time step of the trajectory to advect the air parcel and one additional interpolation of temperature at the end point of the trajectory (Pressure is now calculated from θ and T by the definition of potential temperature. Interpolating pressure first and calculating T gives almost identical results).

The method of interpolation (linear in all dimensions) and the method of calculation of pressure is stated at the end of Section 3. However, a statement that the same is true for the isobaric trajectory model used on top of that in Section 4 missed. We now added a note that it uses the same interpolation.

We also experimented with using other interpolators (cubic spline interpolation), but found no significant differences in our results (e.g. in the standard deviation of the trajectory end points). That is, although there will be more damping by a linear interpolator, this effect is largely insignificant. We added a note to the text.

We think the question if it is fair to compare the continuity winds with the thermodynamic winds without spatially and temporally smoothing is almost impossible to answer. The approaches are so different in nature that an exact definition of this question in a mathematical sense is not possible. There are several averaging and interpolation processes both in Equation 2 and the Lagrangian method which take place at different locations and dates (i.e. a vertical integration over several levels and the derivatives in the divergence operator in Equation 2, the additional averaging in time over 24 hours introduced in the revised manuscript, the trajectory interpolations in the Lagrangian approach). We do not see an easy way to determine which averaging would be completely fair. E.g., the vertical averaging in the continuity equation will also tend to minimize noise. We

ACPD

7, S7313–S7318, 2007

Interactive Comment

think that the question should not be “Is it fair?” (because it is difficult to answer and not relevant in the end), but “Which of the methods actually used in the models gives the better (more realistic) results in the end?”. This can be decided by comparing with observed quantities as done in Section 4. The problem is that it is often impossible to find out from existing literature which averaging procedure (if any) is actually used in existing models to treat the vertical wind. If we would know, we could have included some of these methods in the paper. Since it could be that sometimes no averaging is applied, we would like to leave Figure 2 as it is to show up the possible problems. We have added more discussion in Section 4 and the two additional averaging procedures for the continuity wind to allow for a more fair treatment.

4. This is a typo. It should have been “horizontal wind”. We have added an Appendix which gives the details of the method, since it is not explained in detail in Weaver et al. (2000).

5. This choice is a compromise between the stability of the method, which gets worse for smaller averaging periods, and a high temporal resolution of the vertical winds. The period was determined empirically by experimentation. A sentence is added to the text.

The diurnal cycle played a role. There is a diurnal cycle in the pressure on an isentrope in the analysis data, which could introduce systematical bias in the calculated thermodynamic wind fields that would cause the air parcels to drift systematically from the correct isentrope.

6. Yes, the main application we can think of are chemical transport models and trajectory models. A new Section 5 is added which summarizes the paper.

Additional changes: The order of the columns in Table 1 is changed to reflect the order of the discussion of the quantities in the text. In Section 4, the paragraph about heating rates is moved to a more appropriate position.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13401, 2007.

ACPD

7, S7313–S7318, 2007