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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 the manuscript.

1. We now have added 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. The same issue was raised by referee 1 (see also answer to his comments 1 to 3, which we partially repeat here). You are correct that not only our vertical winds are implicitly averaged over 24 hours, but also that the used heating rates are tendencies over the last 6 hours.

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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. 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 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 a fair comparison in a mathematical sense is not possible. E.g., the vertical integration in the continuity equation will also tend to minimize noise. We think the important question should not be "Is the comparison 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.

In this sense, the presentation of results from instantaneous wind fields in Figure 2 is intentional. At least in some applications, vertical wind fields from the analysis will be used "as is", and we would like to highlight the problems that could show up. We understand that this comparison could be regarded as unfair and now discuss the additional averaging of the continuity winds in some detail in the text.

2. Yes, as mentioned in the introduction (and now also in the summary), we propose to use the thermodynamic winds from the Semi-Lagrangian method in CTMs. The result of our Lagrangian calculations are velocities on the original Eulerian grid, which can be used as the "Eulerian" winds, just as a replacement for the original winds of the analysis (that is why the term Semi-Lagrangian is used to name the method). This is one of the main advantages of the method, i.e. nothing in the code of the CTM has to be changed, only the vertical wind fields have to be read in from another source. There is no difference in using these vertical winds in a (Lagrangian) trajectory model,

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which also needs wind fields given on the same Eulerian grid as in a CTM as input. Therefore, we have difficulties to understand what you mean by appropriate Eulerian velocities.

One could be worried about degrading the time resolution of the winds. However, in most CTM and trajectory model applications the correctness and stability of the long-term transport and a correct representation of vertical mixing will be more important than a correct modelling of short-term fluctuations. As shown in the paper, vertical mixing and long-term transport are far more realistic with the thermodynamic winds. Additionally, short-term fluctuations in the winds from the continuity equation will be dominated by noise.

3. This is easily done by e.g. running trajectories for some hours with the winds from the continuity equation and calculating the potential temperature at the start and end points of the trajectories. We will not extend the text in the paper since we think the method is obvious. It is also immediately visible in Figure 2. For example, there are trajectories which started at 475 K and ended up at 600 K 20 days later, which means an implied heating rate of (600-475)/20=6.25 K/day. This is clearly outside the range that radiative transfer models can explain.

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. A new Section 5 is added which summarizes the paper. Three appendices are added for a more detailed explanation of the method. The text is extended in several places as indicated in the reply to referee 1.

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

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