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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of meteorological analyses and trajectories in the Antarctic lower stratosphere using Concordiasi superpressure balloon observations" *by* Lars Hoffmann et al.

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This is an interesting study that compares meteorological data from several (re-)analysis data sets to balloon measurements in the stratosphere. While the results are perhaps not entirely unexpected and the methods applied are not particularly novel, the study is solid and it is always good to see validation of re-analysis data (and trajectory calculations based on them) against independent data. The paper would, however, benefit from some shortening of the Results section (see also comments below). Most of the results are relatively easy to interpret and do not need to be discussed in such

great detail as there is now – the figures are to a large extent self-explanatory, given that the analysis methods and statistical quantities presented are relatively simple. I have some more detailed comments below but if they are addressed adequately, I am in favor of publication of this paper.

Major points to consider:

Please confirm explicitly (both in your response and in the paper) that the Concordiasi balloon data was not assimilated in any of the data sets that you are using. I assume this is the case but if the data were assimilated, of course your study would not be very meaningful as the data could not be considered independent.

Section 3.1: What is the motivation for comparing the (re)analysis data sets against smoothed balloon data rather than against the unfiltered data? This reduces the relative differences between the high-resolution operational analyes and the coarser resolution re-analysis data because the operational data captures some of the high-resolution variability, while the coarser-resolution re-analyses capture very little of it. Thus, by filtering the performance of the higher-resolution data sets is artificially degraded (relative to the other data sets), and that doesn't seem to be very logical. I think it would be much more meaningful to do the standard/main/reference comparisons against the unfiltered data and use the filtering as a sensitivity study to show that the poorer performance of the coarser-resolution data sets is due to their inability to capture some of the fine-resolution details.

The results section is in many ways too detailed. For example, in section 3.1 (but also in other sections) there are too many numbers that the reader can't all remember. These numbers are all available in Figures and if summaries are needed, this information could be put in tables. However, putting so many numbers into the text, makes it very exhausting to read. I would suggest to substantially shorten this by concentrating on the main findings/messages and the conclusions that can be drawn from these results, rather than listing all individual results. References to the figures should be enough.

Section 3.2: Here, it is stated that the best results are found when constraining the vertical position of the trajectories to the actual pressure heights of the balloons. But this should not be presented as a finding, as it is a trivial result. Anything else than that would indicate some error in the calculation! Notice also that this method has been used before and this may be mentioned. I think the first publication of this was by Baumann and Stohl (1997) but there have been other uses, e.g. Riddle et al. (2006). Related to this, it is also a trivial result that the AVTDs of the isopycnic approach increase with time (page 10, line 25). This does not indicate any real errors, but just shows the fact that the trajectory height is not constrained to the balloon altitude, and since the model does not (cannot) account for altitude variations of the balloon, of course there are errors.

Section 3.4: It would be interesting to calculate the AHTDs also for the ensemble-mean trajectory of all the trajectories with superimposed subgrid-scale fluctuations. Are the AHTDs for this trajectory larger than for the reference trajectory without sub-grid wind fluctuations?

Minor points:

Introduction, first few lines: When speaking of the polar vortex, make clear you mean the southern hemisphere.

Page 3, line 33: you say that GPS positions are recorded at each observation time. I suppose this means every 30 seconds, but where you write this, it is not really clear, as you mention the other observations only later. So explicitly say how often GPS data are recorded.

Figures 5 and 6: The ranges used for the y axes are not ideal. This is extreme for the upper right panels (meridional wind bias) where the data range occupies clearly less than 10% of the available space. This makes it very difficult to actually read the values and makes the figure almost meaningless.

Is figure 10 really needed?

Typos, etc.:

Page 4, line 23: evelopE

Page 12, line 26: . . . because theY are . . .

References:

Baumann, K., and A. Stohl (1997): Validation of a long-range trajectory model using gas balloon tracks from the Gordon Bennett Cup 95. J. Appl. Meteor. 36, 711-720.

Riddle, E. E., P. B. Voss, A. Stohl, D. Holcomb, D. Maczka, K. Washburn, and R. W. Talbot (2006): Trajectory model validation using newly developed altitude-controlled balloons during the International Consortium for Atmospheric Research on Transport and Transformations 2004 campaign. J. Geophys. Res. 111, D23S57, doi:10.1029/2006JD007456.

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