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Interactive Comment

Interactive comment on "The direct inversion method for data assimilation using isentropic tracer advection" *by* M. N. Juckes

Anonymous Referee #3

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General comments:

The paper describes a novel variational data assimilation method using an isentropic advection scheme as a weak constraint. The main advantage of the proposed scheme is its higher computational efficiency compared to standard methods. To evaluate the system, SBUV and MIPAS ozone observations are assimilated covering a six month period in 2003. Additional comparisons with independent observations are given. Results show in general good correspondence with independent observations and give clear evidence of some information gain by the assimilation. On the other hand, results of a sensitivity test show the predominance of spatial smoothing, while the influence of advection is rather small.

I generally recommend the publication of this paper describing a potentially valuable numerical scheme. The work is surely of relevance to the ACP community regarding data assimilation and atmospheric diagnostics. Having said that, the paper has some serious weaknesses with respect to its concept and form that I request to be corrected before publication. As an introduction paper for a new numerical scheme it does not clearly explain the special characteristics (and advantages) of the newly proposed method. Mathematical derivations are not explicit enough. On the other hand, the appendices give too detailed information on rather auxiliary numerics. This could easily be given in a more condensed form. For example, the relaxation method described in appendix D is standard (as the author mentions) and could be skipped completely.

While numerical aspects are described in great detail, the paper is rather cursory in discussing the experimental results. Information on the instruments needed for data comparison is scattered throughout the paper. It should be given in a more concise and condensed way in a dedicated data section (alternatively, the appendix A could be augmented). An error discussion for the used satellite observations is missing. It is misleading to use assimilation results as synonym for observations (section 3.5). There is also no discussion of the dependency of results on different atmospheric conditions or on initial values. Only one example result for the 850 K surface on 10 July 2003 is given. A more detailed discussion is laxly postponed to some publication "elsewhere". This is clearly not sufficient and must be revised by the author. Otherwise, if the author likes to shift the general focus more to describing the numerical scheme, I would recommend a more explicit discussion of the pros and cons with respect to other methods. The weak influence of the advection term, a main result, raises a general question that has to be answered more thoroughly.

Specific comments (see online version):

Abstract) In general, too much numbers! It should be indicated that the method works only on single isentropic levels. Otherwise, statements on the altitude dependency of errors are misleading. Line 5-6: ...for systems studied here _with up to 200000...in total_ (Simply say: for complex systems as studied here...) Line 10-11: ...850 profiles from Electrochemical Concentration Cell... (Omit total numbers of profiles. The reader can assume that the comparison is statistically sound. ECC is too specific, better say balloon sondes) Line 12: Comparison_s_ against...

Part 1) An outline should be added to the end of the introduction. What is the general plan of the paper? What is the aim of numerical experiments performed? Clearly, this is not an evaluation paper for satellite instruments. How would the author himself classify it? (Page 8881) Line 16: ...computational resources tha_n_ the latter... Line 20: ...applications (Bennet et al., 1998),... Line 21: ...more expensive tha_n_ currently implemented...

Part 2) The experimental set-up and remarks on the used data are part of the methodology chapter. I find this confusing. They should be separated. Appendix A holds additional information on the used data. It would be much better to have a data section before the experimental part. Also, more quantitative information on the MIPAS and SBUV data used should be given (e.g., data version, accuracies, etc.). Results will depend crucially on the different data characteristics, e.g., vertical resolution of profiles. Data quality should therefore be discussed in more detail. Tables 3 and 4 fulfill this requirement only partly. Better skip the tables and be more accurate in the discussion. Section 2.1) The author uses results already attained with the proposed method to motivate its application. Moreover, he states as fact what should be proved by the paper: that advection as a physical constraint is necessary to achieve synoptic analysis with global coverage. One may argue that, e.g., a multi-day composite would give sufficient information. The author should discuss the special advantages of his method. (Page 8882) Line 12: ...measurements occu_r_ing with_in_ 2h of the time shown... Line 17-18: ...,at this height,... (what altitude? Please, be more precise here) Section 2.2: The relevance of the stucture function and of equations 2-4 in this context is not clear to me. Please, explain more explicitly. Operator E and variance sigma are not defined. (Page 8884) Line 4: ...exponential of ___ minus... (Something is missing here) (Page

8885) Equation 7d: Index is missing. (Page 8886) Line 7-8: ..., as is standard practice. (What means standard here? In fact, in state-of-the-art data assimilation, observing operators account for the spatial characteristics of the observing system.)

Part 3) Section 3.1) Hemispheric results are discussed only very briefly considering one sample day only. This is clearly not sufficient for a statement on the general quality of results with respect to different atmospheric conditions. Because the proposed method aims at filling data gaps on single isentropic levels, its limitations have to be clarified more precisely. (Page 8890) Line 18-19: ...interpretation of this feature will be discussed elsewhere... (Where? It should be possible to give a concise physical interpretation here. Otherwise, this part should be skipped.) Section 3.2) Please, define information content beforehand (=log(rms)?). I agree, that the estimates shown in figure 3 can be used to derive a random error and also show the advantage of KS relative to KF. But, the difference BF-KF shown in equation 16 is not clear to me. In the center of an integration interval they should give similar results. Please, explain or reduce this section accordingly. (Page 8891) Line 18: ...shown by Fig. _3_....

Section 3.5) Some information on the data versions used is needed here. Especially for MIPAS there exist to different data streams from ESA and IMK. What version has been used here? Line 5: The Tsidu 2003 citation is not appropriate for MIPAS, because it only deals with level 1b data. Better would be Glatthor et al., 2005: Antarctic vortex split in September/October 2002 as inferred from source gas and ozone distributions from MIPAS/ENVISAT. J. Atmos. Sci., 62. Line 6: Thomason and Taha 2003 describe only SAGE III aerosol measurements. A citation on SAGE II and SAGE III ozone is needed here. Line 16: There is little systematic variation of the residuals...(This is not evident from figure 4. For example, there is a strong increase of scatter with increasing latitudes for the southern hemisphere for nearly all instruments. At least a short remark should be given on this.) (Page 8894) Line 18: Figure 5 shows the (the) means and variance of... Line 28-: ...MIPAS is measuring low... (Don't use assimilation results as "observations". Though, figures 6 and 7 seem to indicate that results are not biased, there are of course differences. And, more serious, assimilation does not in general improve the comparison with other instruments. This is especially evident at 40 km altitude, where the comparison with SAGE II gets worse.) (Page 8895) Line 2-4: ..., this may point to a problem with the underlying spectroscopy. (There are more obvious parameters that can lead to deviations between the different instruments: e.g., resolution, coverage, retrieval scheme, etc. The hint on spectroscopy is not well founded.) Line 25: ...to be ind_e_pendent,...

Part 4) (Page 8897) Line 16: ...some of the problems (of) associated with...

Appendix A) Header line: _T_he instruments (capital letter)

Appendix B) (Page 8901) Equation in line 6: The nabla operator should have an asterix as lower index 3, not "a".

Appendix C) This is in general far too detailed. Statements like: "The European Centre uses a thinning algorithm..." are only of marginal interest and should be skipped. (Page 8902) Line 24: Theta is not defined here. I guess it means the latitude.

Appendix D) Is obsolete, because it describes a standard algorithm.

Appendix E) Too much detail again. Should be substituted by a general remark on optimization.

Figures) The scales on figures 1, 2 are missing. No axis labels are given for figures 5, 6, 7, 8 and 9. The subplots of figure 4 have no parameter indication (MIPAS, SBUV?).

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 8879, 2005.